

===== PAGE 1 =====
LÁSZLÓ GÖTZ
THE SUN RISES IN THE EAST
OUR CULTURE FROM HISTORICAL ANCIENT TIMES

===== PAGE 2 =====
GÖTZ LÁSZLÓ
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===== PAGE 3 =====
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===== PAGE 4 =====

THE HUNGARIAN ORIGINAL HISTORY OF GYULA LÁSZLÓ GÖTZ

Every person starts out with many different abilities, only to have their abilities largely fade under the pressure of their chosen career, leaving only the "main occupation." Quite often, however, it happens that the suppressed abilities claim space for themselves.
It is understandable that, in contrast to those researchers who confine themselves to their own narrow field of expertise, I sympathize with those who, in addition to their own profession, also train themselves in other research. There is only one danger here — amateurism. And this is rampant in the field of Hungarian prehistory; ineradicable. It was with such misgivings that I picked up four volumes (the fifth has not yet been published) on our prehistory by my doctor friend living in Sankt Pölten. The various attempts in my library take up about 1.30 linear meters of books and studies on prehistory published in Hungarian emigration, so I am well aware of the imagination that gallops with the reins thrown at it.
However, I read László Götz's books with sincere admiration and many lessons, I hardly believe that there is a Hungarian prehistorian who would have chewed through such a vast literature. Götz is not an enthusiastic amateur, but a serious and serious practitioner of our science. His four volumes published so far (and a summary study) provide 1200 pages of text, the number of referenced technical books is around 600, which he not only read but also studied. He published his volumes himself, in order with the following subtitles; I. Towards the Proto-Asian Primitive Language; II. In double measure; III, "Dissect only a linguist"; and the first part of volume IV, entitled: The Sumerian Question. I must confess that studying these volumes was extremely instructive and useful for me, not only because of the vast factual material processed in them, but also because of his methodological approach. I would explain this with a short digression.

What Lajos Ligeti said to me about my thesis written in The Prehistoric History of the Hungarians and my presentation based on it in 1943 is vividly alive in my memory. (And he was also a linguist!) I was glad for your study because you do not build on our linguistic results, We should not hear back from archaeologists what we as linguists say, because then our research will reach a dead end, it will end up in one of us confirming the other, Your work relies exclusively on archaeological sources and shows possibilities different from those of the linguists. This would be the way for our cooperation, not that you always follow in our footsteps." If I did not give Lajos Ligeti's words literally, I was not wrong in their essence! Indeed, our prehistoric research = partly my own older

5
===== PAGE 5 =====
works - were also based on the results of linguists. It is essential, however, and I emphasize here, not on the examination of the material of the language, but on the historical conclusions drawn by linguists from the examinations! This is a big difference! While we cannot comment on their linguistic work strictly speaking, because we are not competent, their results projected onto areas and time sequences do not follow from linguistic observation, but only their historical explanations. Leaving the linguistic facts untouched and acknowledging the complete error of the linguistic historical ideas, the questions must be raised again, and we must largely seek a solution within the framework of the discipline that creates its prehistoric ideas in possession of tangible material with settlements, population movements, etc.

László Götz's contributions are in the same spirit in Volume II, when he criticizes our two prehistory books because they follow purely linguistic principles. László Götz's work makes up for the criticism "history" we lack, to a certain extent, and for that we owe him nothing but gratitude.
One more word in advance, this concerns the style of László Götz: as if the powerful tone of religious debates were revived, he mocks, judges with strong words, almost dismisses in the awareness of his own superiority. This is unpleasant in his work, but we rather admire religious debaters for their outspokenness than condemn them for their rudeness. Let us overlook the author's energetic style defending his own truth, and let us not set aside these valuable volumes for this reason. I recommend that, in any case, if a new edition is published, he tone down the hurtful echoes of his judgments.

The subtitle of his first volume (Altötting, 1981) is: Critique of European Linguistics and Historical Perspective. He sees very correctly that the background of linguistic influences is, on the one hand, the spread of cultural goods (this does not involve population migration), and on the other hand, there are people's territories and settlements, and he does not examine this issue merely as an acknowledgment, but seeks the reasons that may lie behind a larger proportion of population movements, and he sees the reason in the population differences between hunter-fishermen and producers (agriculturalists and animal breeders). While in the former 100 square kilometers are needed to support a small family, in the latter the population increases a hundredfold. This was also one of my arguments against the assumption of a compact Ural population. Now, the explosive growth that began in the Fertile Crescent of Asia Minor
Population
growth led to large-scale emigration and colonization, which |
coincided with the spread of new forms of production; these are repeated every four to five centuries. Considering that the new modes of production and with them urbanization developed primarily in the Near East cultures and, as archaeologically demonstrable, spread from there to the east, north and west of the Eurasian continent; three main directions are observed in archaeological research

6
===== PAGE 6 =====
based on traces, in the expansion: 1. Through Anatolia to the Mediterranean basin, the Balkans, Southeastern, Central and Western Europe; 2. Towards Southern Siberia, Turkestan, Iran, Afghanistan, Western India and East Asia in general; and 3. Towards the South Russian steppe and the Volga region.
To justify his findings, he takes the archaeological changes observed in these areas one by one. These accounts show that he knows these issues in detail, the literature used (here and elsewhere) also takes into account the latest works, and he quotes not only the large summaries but also the detailed literature. Needless to say, he knows Hungarian archaeological literature thoroughly, since Hungarian is his native language! For my part, I regret that he did not incorporate the work of József Csalog, the brilliant Hungarian paleontologist, into the description of the successive waves of pre-Asian civilizations. The pre-Asian colonists filled the Carpathian Basin, among others, almost continuously for about 3,000 years during the Neolithic and Copper Ages. Not only the overgrowth of the population (demographic explosion) played a role in the three-way expansion, but also the trade of raw materials, their more precise exploitation, the construction of colonial cities, etc. Meanwhile, he pays very close attention to the archaeological chronology, which he also intends to play a very important role in what follows. Already in this chapter, he suggests that the migration and emergence of this large number of settlers must certainly leave a mark on the language of the indigenous peoples of the flooded areas, since many new means of production, knowledge, etc. were spread, and this had to be reflected in the language! This is how he then arrives at the presentation and strong criticism of the schools of Indo-Germanic linguistics and history. It is also known that our linguistics strongly links the chronology of Finno-Ugric linguistics and the history of its peoples to the findings of Indo-Germanic studies.

It is therefore understandable that the author devotes long pages to the development and refutation of the Indo-Germanic linguistic chronology. I myself do not feel competent in these linguistic issues, so I will only briefly summarize what Götz has to say. The Indo-Germanic chronology is based on the law of sound changes, saying that the lawful changes occur one after the other in time. László Götz, examining the issue, shows that the various variants coexist in time, so that their observation is unsuitable for establishing a chronology based on them. Yes, but since the chronology of Finno-Ugric linguistics (Uralic, Finno-Ugric, Ugric) is based precisely on the Indo-Germanic "results", the chronology of Finno-Ugric studies is also disrupted.

But the so-called family tree theory, that peoples who were later considered related once emerged from a single language family, also collapses. This was mentioned earlier, when he wrote that it is impossible to speak of a large population concentrated in a small area with primitive production. The

7
===== PAGE 7 =====

In contrast to the older definitions of culture and homeland, László Götz is a supporter of the prehistory initiated by NS Trubetzkoy, which considered the linguistic fusion of smaller peoples who were separately areal (i.e. settled in isolation) as the basis for the emergence of common, communicating languages. So, speaking in Hungarian, in our context: there was no Finno-Ugric unity, but it emerged over time as a communicating linguistic kinship. In a separate chapter, Götz discusses the theorem of "regular sound changes", inherited from J. Grimm and still serving as a guideline, and shows that they can be explained in other ways, or simply the direction of development. At the time, I also had a lot of trouble understanding this "law", it is as if a theorem included in the natural history of languages were valid, but we could not come up with any reason behind it as an explanation.
Götz found an explanation for the separation of the shades of meaning of words, while I myself primarily thought that words that have entered a foreign "articulatory basis" do indeed change according to law, because they adapt to the laws of the host language, but it is noticeable that these questions extend beyond strictly limited linguistic "no more", he said. Anonymous, since linguistics is not my thing. Although research. The remaining, about a hundred pages, collects similar-sounding words from different languages,

of words with the same or similar meaning, and discusses linguistic issues in a narrower sense. Although these tables have the power of surprise, I cannot comment on the questions raised, as I do not understand them. In any case, one thing is certain: László Gótz approaches the solution on a very broad, complex basis.

Let us turn to the second volume, subtitled: On the methods and results of Hungarian prehistoric research (Altötting, 1981), also published by the author. From its historical introduction, we are pleased to highlight the effort to do justice to the undeservedly forgotten or even ridiculed researchers of the last century, and to examine their theories again and again. In this respect, he agrees with István Szýcs (Kolozsvár), who also found great value in the often ridiculed István Horvát. To be taken to heart

This is also true for us, because they had many insights that later fell under the one-sided yoke of linguistics and were lost in our historical studies. The Finno-Ugric derivation accepted by the Academy and the university excluded from the researchers' sphere of interest anything that would contradict the doctrine, and this one-sidedness still prevails in the research of Hungarian prehistory today: archaeologists provide almost only illustrations for the theorems of linguistics. As proof of this, he analyzes two recently published works on prehistory: Antal Bartha's work: A 9th-10th century Hungarian society (1968, 1973) and István Fodor's book: Verecke's famous path (1975).

8
===== PAGE 8 =====

His choice was unfortunate in that Antal Bartha's book is actually an elaboration of Erik Molnár's hypothesis, The Crisis of the Pastoral Society. The only valuable things in it are the abundantly cited Soviet works, to which he had access during his years of work abroad. However, since his observations (i.e. Gótzé) are methodological in nature, we cannot avoid a brief account. Bartha also follows Erik Molnár in describing the Hungarian people as still in a primitive community in the centuries before the conquest. (In this, he could have also referred to József Deér, or to several of our linguists who projected the prehistoric state of the Hungarian language and the original meaning of its words onto the age of the conquest, and therefore considered it very poor and primitive.) Bartha builds extensively on our "Bulgarian-Turkic" Chuvash vocabulary defined by Zoltán Gombocz. In his criticism of this, Gótz cites a work that is unknown in our history and linguistics, namely the statements of J. Benzing, who bases his argument on Kashgari, that the rhoticisms in Chuvash could not have arisen before the 11th century. After this, it is hardly possible to talk about a 6th-9th century Bulgarian-Turkish adoption and build theories on it! This issue must be thoroughly examined by our linguists-historians in the future, because it is customary to refer to this as a "fact".

undoubtedly

Bartha's ideas are discussed extensively. László Gótz deals with Iranian issues and follows in the footsteps of Géza Nagy, but he also quotes other recent authors who doubt the Iranianization of the steppe. I think so. I think we should give credit to the ancient writers who visited the Scythians and whose descriptions show them as having a strong Mongol character. The contemporary greats of Asian studies, F. Hancar, B. Jettmar, HW Haussig and others, strongly doubt the one-sided Iranian designation. I think it may be that we perceive the enormous extent of the Scythian style as a kind of Scythian empire and its people as homogeneous. Obviously, people of various origins and types are called Scythians. László Gótz also writes very correctly that the deer goddess connects the Scythians with the Hungarians (Szarvas-monda). When presenting the instability of the Iranian language, Gótz also quotes Gyula Mészáros, who also fell into the "sinkhole" of historical science. Let us not forget that the research on the nationality of the Scythians came into the sphere of interest of László Gótz in connection with the fact that Antal Bartha, with noble simplicity, considers them to be Iranian speakers.

He examines Bartha's prehistory ideas in several areas, and always shows their superficiality. Unfortunately, he could not yet have known István Fodor's detailed, devastating criticism of Bartha's extensive work in Volume I of the History of Hungary, in which Fodor rightly considers the prehistory section useless, and László Gótz is the native-9

===== PAGE 9 =====

write the same thing about the part related to las. It is precisely because of these many negatives that I did not consider it fortunate that Gótz chose Bartha's book to criticize Hungarian archaeological positions based on the claims of Finno-Ugric linguists. The other author whose work demonstrates how harmful a single-minded reliance on linguistics can be is the work of István Fodor (in the famous way of Verecke). Perhaps his youth explains why he is sometimes too decisive. For example, László Gótz objects to his statement that: "The Hun-Hungarian relationship or identity is based on Western chronicle traditions and simple "historical" fiction." It is obvious that he is adopting the findings of Györfy György (Our Chronicles and Hungarian Prehistory) here, and his certainty is not based on his own research. Here Fodor is confronted with the testimonies of our chronicles, as well as with many Hungarian historians of the last century and our century. It can hardly be disputed that the emerging or already established Hungarians belonged to the Hunnic empire, and the memory of this is preserved by the Hunnic tradition of our princely family, and the sense of descent that is also tenaciously alive among the Székelys. Of course, this does not fit into the thinking of Finno-Ugric derivation. The teaching of Péter Hajdu, which places the beginning of the formation of the Ural-Finno-Ugric people in the tundra, is taken over in the same way by him. At that time We also argued with him, saying that we are not competent to criticize his linguistic results, since human life was absent in the tundra at that time, so it is impossible to go into the details of his criticism of We Cannot Go There, because it takes place in the field of archaeology and would require archaeological knowledge from the reader. Instead, we can summarize it by saying: Gótz criticizes Fodor for adapting archaeological facts to the assumptions of Finno-Ugric linguistics. In this regard, let me remind the reader of the thoughts with which Lajos Ligeti accompanied my early prehistoric work, praising me for not getting into the groove of linguistics. "her homeland". Although Gótz takes Fodor's work more seriously than Bartha's, his polemical manner sometimes leads him to be arrogant here too, which is by no means appropriate.

Let me mention that Gótz often refers to my work and comes to many similar results. Among them, I would like to mention his statement made in this chapter: "The signs therefore point to the fact that in the case of the Finno-Ugric languages, it is not a question of the branching of some original, unified proto-language, but, on the contrary, of the gradual equalization of the originally different languages of many small tribal groups". He disputes another irrefutable statement by Fodor that the Scythian tradition has no scientific basis in our country". Let it be enough to refer here to the central role of the deer (Istcnarya) to make us think about the question (and then there are numerous Scythian legacies in the archaeological records of the Onogur-Avars and the Hungarians of Árpád).

10
===== PAGE 10 =====

László Gótz's comment on the eye and mouth covering plates found in graves from the time of the Hungarian conquest is interesting. The Obi-Ugric connections of these were discovered by István Dienes, who was the first to discover them, and Fodor based on fr, Gótz, on the other hand, draws the prehistory of these phenomena up to the Scythians and beyond. We must agree with Gótz, He disputes the Panalan theory on broad grounds (and here we come to the centuries preceding the conquest), for example in the case of the Saltov-Mayacki culture. We do not wish to deal here with Fodor's objections and their refutations in connection with the "double conquest", because we do not consider them appropriate. László Gótz's objection to the work of Bartha and Fodor is that both of them got caught up in the Finno-Ugric linguistics and tried to shape the archaeological facts to suit it. He devotes the third volume of his work to the methods and results of Finno-Ugric linguistics. By shattering the Finno-Ugric theory, which is similar to the edifice of Indo-Germanic linguistics, he approaches what Loránd Benkly succinctly formulated as follows: "Alongside and behind the extensive Turkological and Finno-Ugric studies, the circle of research in which the questions of our prehistory are addressed against the dogmas of the Hungarian in the narrower sense of the word 'we have taken this from this, that from that' has not developed sufficiently, and indeed has lagged behind." But you can also illuminate it from the side of linguistics." Gótz tries to serve this requirement when he says that we cannot follow his linguistic correspondences, not being linguists.

The introduction to the 150-page third volume has thus been very brief, because as archaeologists we do not want to take a position on strictly linguistic issues. The same is true of the fourth volume (subtitled: Archaeological, ethnogenetic and historical analysis of research). Although the basis of this chapter is archaeological, the in-depth study of the great cultures of the South has +so far — fallen out of my sphere of interest, and so I can only accompany Gótz's discussions with imaginative comments. Let us turn to the "most delicate" question, the role of the Sumerians. There are many over-the-top, even fraudulent, statements about this in the West - and here too - sometimes in fancy book form. The amateurish linguistics of "the Sumerian heritage lives directly in the Hungarian language", etc., have made this teaching almost ridiculous. László Gótz discusses the question from a completely different perspective.

> Ultimately, these archaeologically clearly demonstrable mass pre-Asian colonizations make it essential to take the Sumerian language into increased consideration, not only in the Sumerian-Hungarian and Sumerian-Finno-Ugric relations, but also in the linguistic research of other Western European language groups and language families. In other words, let us remember this well - we are not talking about some kind of separate Sumerian-Hungarian linguistic relationship or perhaps ethnic descent.
11
===== PAGE 11 =====

but about a much, much more far-reaching linguistic historical issue. Namely, that the very close and trappán Sumerian linguistic correspondences that are still found in large numbers today in almost all languages of Western Eurasia, at the current level of our archaeological and prehistoric knowledge, can only be explained by the former existence of an ancient linguistic koine, i.e. language community, or "lingua franca" in the pre-Asian region, in the Neolithic and Early Metal Age times there.

The mass of linguistic elements of this language community reached the southern regions of Europe and West Asia with the agricultural and later metalworking colonies that originated in Pre-Asia, and in the new settlements over time, on the one hand, through their own internal development, they were initially divided into dialects and later into independent, separate languages, and on the other hand, the national and tribal languages of the fishing-hunting indigenous population, more or less distorted — pidginized — the dialects of the Pre-Asiatic colonists living among them or in their immediate vicinity, adopted them to a greater or lesser extent and incorporated them into their own language system, adapting them to their own sound order."

If we now transfer this linguistic historical perspective to the kinship relations between the Hungarian and Finno-Ugric languages, it means in Hungarian that in the case of the so-called Ural-Finno-Ugric language family we are not faced with a genetic, i.e., a lineage-based language relationship, but rather with the partial equalization of the originally separate national languages of many small Paleolithic-Mesolithic fishing-hunting communities against the dominant areal effects of the language of an agricultural-animal-breeding-metaworking ethnic group that came from the south and had overwhelming ethnic dominance, i.e. we can speak of a so-called areal language association...

Hungarian research cannot be left without mentioning László Gótz's seriously considered theory. In this brief presentation, I could not present all of his controversial and debatable observations, but — I think — this taster is enough for Hungarian research to get to know and discuss the theory. It is with this hope that I conclude this unduly brief introduction to 6 monumental works -- from which a great deal of doubt was omitted. I myself found much to think about in the conclusions drawn from the enormous amount of data, and I wish this enrichment to my professional colleagues as well. (Zsenyie, March 14, 1989) So far, it The introductory text to the first four volumes of "The Sun Rises in the East." He finished the fifth volume a few months before his death. The following short essay is about this volume. has been prepared as an introduction, presenting a consistent conclusion to the findings of the first four volumes,

12

===== PAGE 12 =====

His method did not change; he liked to present his ideas through the criticism of selected works, thereby also making up for the criticisms that were quite neglected in Hungarian archaeological literature.

The first major chapter of his fifth volume — almost a self-contained little book — deals with the dating of archaeological cultures. This chapter should be taught, it is so important. For a while, our archaeology firmly believed that measuring the amount of cosmic carbon (C¹⁴) provides a solid chronological order and definitively determines the succession of archaeological cultures. However, the application of the C¹⁴ results gave rise to two contradictory schools of thought: one saw the emergence of agricultural, animal husbandry, settled, and urban civilizations in the emergence of settlements, colonization, and trading colonies directed northward from the Fertile Crescent of Mesopotamia as a result of the sudden overpopulation, while the other school disputed the European origin of all of these and their radiation from west to east. Both referred to the C¹⁴ results. This obvious contradiction was waiting for an explanation. Well, after a thorough investigation by the natural science expert, László Götz was able to reveal the reasons for the contradictions, the factors affecting the C¹⁴ studies, which must at least be reconciled with the evidence of the annual rings of trees (dendrochronology). Thus, reviewing the C¹⁴ bases of the individual assumptions, Götz definitely sees the Neolithic revolution of the Mesopotamian Fertile Crescent in the background of the northward civilization and migration of people, and he also provides archaeological evidence of these. We will give him the floor almost as a summary of this chapter: "as a result of the fundamental, theoretical and methodological error recognized by László Jakucs, the C¹⁴ method cannot be considered an objective, natural science procedure today, because we do not have any method for calculating the numerical ratio of carbon atoms of mineral origin..."

In the spirit of this new insight, he examines new Egyptian chronologies; this lengthy discussion sidetracks the train of thought, and so, although a valuable contribution in itself, we feel that it was unnecessary to include it here. All the more useful is his examination, which he devotes to the new theories that have arisen around the cultural primacy of Europe, and in which he refutes them with impeccable methodology. These debates inevitably lead to the question of the Indo-Germanic (Indo-European) ancestral homeland. Of particular interest to us from this chapter is the theoretical and practical debate on the concept of people families and biogeographic ancestral homeland studies. Since the Finno-Ugric ancestral homeland concepts and language families are strongly based on Indo-European theories, we indirectly receive a critique of Finno-Ugric ideas. Similarly, the doctrine of "primitive languages" is sharply criticized, and this is also reflected in similar efforts in Finno-Ugric linguistics.

The third chapter of the fifth volume aims to examine the latest results of Hungarian prehistory research. For me, it is interesting and

13
===== PAGE 13 =====
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It is reassuringly close to my Swedish idea, which is that the Finno-Ugric languages and peoples, formed by the southern, Gravettian-educated population migrating north, developed their languages that are close to each other in a series of separate languages. However, a closer look at the Andronovo culture and language reveals the established communicative language that occupied a vast area and then existed until the beginning of the 1st millennium BC. Then, starting from about 1000 and 900 BC with the disintegration of the Karasuk cultural unity, as a result of the loosening of relations and the aforementioned migrations from west to east, the individual groups of the cultural and linguistic community in question went their separate ways again linguistically, i.e. their languages continued to develop in different directions. In this way, according to our view, one of these earlier ethnic groups of the Andronovo or Karasuk culture could have participated in the later processes of formation of the Indic and Iranian languages, another group apparently played a very important role in the consolidation of the Turkic languages, and the further linguistic development of a third group led to the formation of the Proto-Hungarian language and the areal equalization of the Finno-Ugric languages." This is how the author summarized the results of his research, in which he apparently approached Trubetzkoy's linguistic ideas. Unfortunately, the author was not familiar with the results of Hungarian anthropology, which also place the Andronovo areas as the scene of the formation of the Hungarians as a people (T. Tóth, Gy. Henkey).

Following these findings, he sharply criticizes the prehistoric studies published in the 1990/3 issue of Magyar Tudomány. In the next chapter, he examines the issues of colonization, trade routes, and mining settlements from Mesopotamia northward with his meticulous knowledge of the literature. In order, under the title of Cultural, Ethnogenetic, and Paleolinguistic Connections of the Eurasian Steppe Zone with East Asia, he first presents the connections of West Asia, then turns to Eastern Europe, and turns to the areal language equalization theories of the relationship of the Uralic-Finno-Ugric languages. He concludes these discussions with the theory of the Ugric-Hungarian language community. In this chapter, he renews the theory of the brilliant Hungarian prehistorian of the turn of the century, Géza Nagy and his associates (e.g. Károly Flók), which was rejected at the time by the Hunfalvy and Budenz school, which proclaimed the exclusively Finno-Ugric origin of the Hungarian people and language, without any closer examination. I quote from the author: "Géza Nagy and his colleagues included in this "Ugric-Hungarian" language community group, in addition to Hungarian, the languages of the Ogurs or Ugrians, the Onogur-Bulgarians, Sabirs and partly the Avars, Huns, whose practically unknown or unclear affiliation, as well as, with certain restrictions, the languages of the Scythian-Sarmatian-Szaka peoples of the 1st millennium BC". In short, László Götz is inclined to give our chronicles and Hungarian historical research of the past century a new meaning.

===== PAGE 14 =====

jon truth, as opposed to the exclusive Finno-Ugric school. "In particular, Géza Nagy and his colleagues explained the common features of Hungarian, Uralic Finno-Ugric and Turko-Mongolian (Altaic) languages as linguistic effects of this Ugric-Hungarian language community causing areal language equalizations — more than four decades ahead of Trubetzkoy and modern Indo-Germanic research." The area of this language community is roughly the same as the area of the Andronovo culture, and thus Géza Nagy and his colleagues' suspicion is also confirmed by Götz's investigations.

Finally, László Götz sees the development of Eurasian history towards the Hungarians in four stages. In the first stage, the Mesopotamian origin in the Caucasus and Asia can only be vaguely felt. In the second stage, the vast area of the Andronovo culture is already clearly outlined in the Andronovo culture. The author clarified the connections with it in the previous chapters. The third stage of the development of steppe cultures can be traced back to the turn of the 2nd/1st millennium, when the cultures of the forest-steppe migrated further south and developed the culture that we can now refer to with folk names: Cimmerician-Scythian, Sarmatian, and they exploited the vast grassland areas with bound soil that had not been exploited until then, and then, having an impact on the more northern areas (Ananino, Gorodiscse culture), the communication and linguistic relations between the Hungarian and Finnic peoples developed. In particular (stage 4), direct Hungarian influence can be observed in Ob-Ugric, as well as other languages, especially Chermis. All this may have taken place in the middle Volga region, where Julianus found the Hungarians who had remained there.

This unduly brief summary lacks the presentation of detailed evidence. Only the presentation of the entire text would help with this, fortunately this has now happened, it is not about pre-historicism, but about new assumptions to be taken seriously, "The Sun Rises in the East" with the presentation of all five parts. I don't need to emphasize again and again that this is not about the almost epidemic of art lovers which at the same time also show that the assumptions accepted so far are actually theories, which are given credence by the excellent team of researchers who developed them. In Götz's work, the teaching of the Hungarian chronicles is actually brought to life in the light of modern research. It would be a great disservice to our prehistory if its theories were not taken seriously and discussed.

March 14, 1994 Gyula László
===== PAGE 15 =====
BOOK I
TOWARDS THE EAST ASIAN LANGUAGE
Criticism of European linguistics and historical perspective
===== PAGE 16 =====

Chapter 1
THE IMPACT OF NEOSTILE, MEDIEVAL AND EARLY BRONZE AGE PRO-ASIAN CIVILIZATIONS ON THE CULTURAL DEVELOPMENT OF WESTERN EURASIA
In the introduction, we describe the development of the pre-Asian Neolithic, Copper Age and Early Bronze Age cultures, and then discuss their cultural and ethnological connections with West Eurasia, dividing the area in question into three regions for better clarity. Since we will only be discussing the pre-Christian era, we did not consider it necessary to indicate this separately in each case. The dates given correspond to the so-called long Mesopotamian chronology (Strommenger, E.: Fünf Jahrtausende Mesopotamien). In Near East, traces of food production and settled lifestyles can be traced back to the 8th-9th millennium BC. Archaeology has uncovered numerous agricultural and animal husbandry settlements from these ancient times in a continuous chain from Asia Minor through Syria, Palestine and Northern Mesopotamia to Western Iran. The initially isolated centers were already in lively contact with each other in the 6th millennium. Evidence of the start of long-distance trade is the appearance of obsidian from Eastern Anatolia and Armenia throughout the entire region, as well as the close parallelism of the vessel shapes and decorative motifs of ceramics (Hassuna period, ca. 6000-5600) appearing at the turn of the 7th and 6th millennia BC, in sites located far from each other. Hammered copper appears surprisingly early, at the beginning of the 7th millennium BC, at the Cajón settlement in eastern Anatolia. The beginnings of metallurgy can therefore most likely be traced to Asia Minor.

It would be too lengthy to provide an exhaustive description of the cultures of the Pre-Asian Neolithic, Chalcolithic, and Early Bronze Age, so we refer to the relevant literature for details, and we will only briefly outline further development. At the turn of the 6th and 5th millennia BC, we find several archaeologically clearly distinguishable cultures in East Asia, in which ceramics, tools, buildings and the ubiquitous clay figurines of women, probably reminiscent of a fertility cult, appear in characteristic forms. Between 5600 and 5000 BC, the Samarra civilization developed from the Zagros Mountains to later Assyria, and from there, to the west and northwest, as far as the Mediterranean Sea, Mersin in Cilicia and the area around Lake Van, the Tell Halaf pottery circle emerged, which shows close connections with the somewhat later 19
===== PAGE 17 =====
with the first and second phases of the Eridu-Übaid culture of del-Merzopotamian (5400-4500). The Eridu-Übaid culture expands around 4500 to almost the entire Tell Halaf area, even to the area around Samarra, and to Western Iran. (Übaid 3rd and 4th periods, 4500-3800). This is the first unified culture that encompasses all of Mesopotamia and the surrounding areas. Its small branches can be traced as far as the Indus in the east, as far as Turkestan in the northeast, and as far as Egypt in the west. The Uruk period lasted from about 3800 to 3200 BC. At that time, the presence of the Sumerians in Mesopotamia can be proven, although several researchers attribute the Übaid civilization to the Sumerians. However, based on our current knowledge, this is only probable, but cannot be stated with certainty. According to the most widespread view, the Sumerians were in Mesopotamia at the beginning of the 4th millennium BC.

They migrated into Mesopotamia from the north or northeast. SNKramer, one of the most renowned Sumerologists of our time, claims, based on the ancient Sumerian epic "Eumerkár and the Lord of Aratta" and other considerations, that they came from the southern coast of the Caspian Sea or from the region east of the Zagros Mountains. This migration route seems to be supported by the undoubted parallels of the pottery of the Uruk period, as well as that of North Iran and Transcaucasia of the same period. However, since the chronology of North Iranian pottery has not yet been determined with sufficient accuracy, this question must be left pending for the time being. (B.Hrouda: Vorderasien.)

The next, so-called Jemdet Nasr period lasted from about 3200 to 2900, and then we count the age of the early Sumerian dynasties until the establishment of the Akkadian empire (around 2450-2400). Around 2300, the Gutian people, who invaded from the Zagros region, overthrew the Akkadian empire and ruled Mesopotamia for about 70-100 years. After their defeat, the III. Ur dynasty organized the Neo-Sumerian empire (around 2200-2100), which, however, collapsed after 100 years of existence due to the mass immigration and destruction of the Semitic Amorites, as well as the simultaneous attacks of Elam. We do not need to dwell on the events that followed, because they fall outside the period that we now want to examine.

The development of the pre-Asian geographical region described above therefore, according to our current knowledge, preceded the cultivation of other regions of our earth by thousands of years. The transition to a settled lifestyle, food-producing farming — farming, animal husbandry — the discovery of the secrets of ore-bearing rocks and the increasingly widespread use of metals obtained from them — initially by hammering and then by smelting — i.e. the basic conditions of material civilization, all without exception, appeared in this area at such early times when elsewhere we could only find rudimentary Paleolithic (Paleolithic) or at most transitional Paleolithic (Mesolithic) conditions. But in the same way, the first stages of intellectual culture tangible manifestations; the development of higher religious and cosmological concepts, cities,

20

===== PAGE 18 =====

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The emergence of states and the invention of writing can also be registered for the first time in Elé-Ázsia.

In principle, it would be conceivable that sooner or later the rest of humanity could have reached a similar level of culture — independently of each other — but archaeological data show otherwise. In the light of the finds, we see that in most of Eurasia, even thousands of years after the pre-Asian development described above, the population remained at the Paleolithic — transitional stage, and the beginnings of culture in these regions can always be attributed to the archaeologically clearly documented influences of the pre-Asian — Mesopotamian center. Trace by trace, step by step, the gradual expansion of the ancient cultures of the Middle East can be followed: first to the surrounding, immediately neighboring areas, and then to more distant regions, similar to the ringing of concentric waves caused by a stone thrown into water. This phenomenon clearly proves that in most of Eurasia we cannot count on the independent development of individual areas, but rather — at least in the western half of the region — the fertilizing effect of the pre-Asian primary cultures is present almost in detail in the formation of various cultural centers.

These, appearing to a lesser extent in some places and to a greater extent in others, are part of the Middle Eastern — Mesopotamian culture-making process that extends to all areas of culture in certain regions. We will try to track these effects in the following and demonstrate the correlations on a case-by-case basis.

Globally, we can distinguish three main directions in the expansion of pre-Asian civilizations:

I. Northwestward, through Anatolia, into the Mediterranean basin, the Balkans, and Southeast, Central, and Western Europe.

II. Towards the northeast, through the Zagros Mountains and Iran to Turkmenistan, and from there to Afghanistan and Western India on the one hand, and to Southern Siberia and East Asia on the other.

III. To the north, via Transcaucasia, across the Caucasus towards Southern Russia and the Volga-Ural region.

We consider it most appropriate to introduce the Neolithic and Early Metal Age development of each region separately based on this three-part division.

In this work, we will not discuss the also very significant East Asian cultural influences that can be identified in East Africa (Egypt) and South Arabia, because these require a separate dissertation.

I. Southeastern, Central and Western Europe

The earliest European Neolithic civilizations known today developed in Thessaly at the beginning of the 6th millennium (Nea-Nikomedea, Seskio) and soon after (5000

21

===== PAGE 19 =====

around) spread to the Balkan Peninsula and the Kárpát Basin (Starévo-Körös). The Neolithic cultures of these southeastern European — Central European regions are without exception the creations of Proto-Asiatic agricultural settlers who arrived en masse, in several waves, via Anatolia and the Aegean Sea. The immigrants brought with them their tools, ceramics, clay figures, construction methods, settlement forms, religious ideas (fertility cult, mother goddess worship, bull cult, etc.), customs, and symbols.

Mindczek can be traced all the way to Hungary, to the Danube and Tisza regions, one by one in their historical material. The pictographic signs of the Tordos and other Transylvanian settlements from the 4th millennium BC are found in Troy and Mesopotamia, from the same period. The "tells" of the Tisza culture from around 3000 BC are exact copies of the pre-Asian settlements. However, the close connections were not broken in later times: the pictographic tablets of Tatarla correspond to the Sumerian pictographic signs of the Jemdet Nasr period (around 3000 BC). The late Neolithic Polish culture — in the 3rd millennium BC — is largely the creation of a new wave of settlers from the Middle East.

With a corresponding "phase delay", 500-1000 years later, agriculture, ceramics, and a settled lifestyle appear in the northern and western areas of Europe. Here, however, we can primarily speak of cultural influence only, and no large numbers of pre-Asian settlers can be detected. The teachers of the peoples here were the cultures of the Carpathian Basin.

In the 4th millennium — from the Netherlands to the Dniester — the cultural circle of ribbon-decorated ceramics emerged; the first agricultural culture of the northern regions of Europe, which received the greatest cultural impulse from the Körös culture. The culture of ribbon-decorated ceramics, in turn, had a very strong influence on its northern and western neighbors. It passed on to them the knowledge of agriculture and pottery making.

The pre-Asian settlers coming from Asia Minor and the Aegean Sea therefore repeatedly, in several waves, flowed into Europe throughout the Neolithic Age. They refreshed and enriched the local cultures again and again, but they also brought very significant pre-Asian masses to the Balkans and the Carpathian Basin. We would like to emphasize this latter circumstance, because we must attribute an outstanding role to the aforementioned pre-Asian immigrants in the paleo-ethnogenesis of these regions. We must not forget that only agricultural cultures can sustain a higher population density, so the fishing-hunting-gathering indigenous population could only represent an insignificant ethnic group. This line of thought is also supported by the Dinaric-type population, which still plays a leading role in the Balkans today, and whose closest anthropological relationship was represented by the Clo-Asiatic-Armenoid type.

Neolithic civilization was also brought to the western Mediterranean by pre-Asian settlers. The first wave — in the 6th millennium — was the so-called Carthaginian period.

===== PAGE 20 =====

This led to the development of groups of dium ceramics in the Mediterranean region, on the coasts of Italy, Dalmatia, France, and Spain. However, their development stalled.

Much more significant were the later immigrations, which appeared in the 3rd millennium in the megalithic civilizations in the Mediterranean, on the Atlantic coasts and as far as Scandinavia. The starting point of this colonization was also East Asia. According to 616%, the settlers arrived here in several waves and mainly occupied the ore deposits. The people of the bell-shaped vessels also belonged to the megalithic civilizations. They penetrated from the west as far as Hungary at the end of the European Neolithic and knew copper and gold. Several researchers see them as ore-seeking groups.

Given the enormous size and mass occurrence of megalithic structures, we can also expect significant pre-Asian settlements in Southern and Western Europe. We consider it particularly important to point out the planned nature of the occupation of ore-rich regions, as this indicates conscious, economically motivated colonization.

Copper appears in Hungary around 2700 BC, but only sporadically. The Copper Age proper begins in the middle of the 3rd millennium. This is when ore mining begins in Transylvania — at least according to one group of researchers. The immigration of the Proto-Asians continues during this period. Around 2200, several waves of settlers arrive again from the south, and the population increases significantly. From 2100, the Pécel (Baden) culture develops, which extends to the entire Carpathian Basin and can even be found in Eastern Austria and Bohemia-Moravia. Close parallels to its characteristic urns have been excavated from the II-V. stratum of Troy. It is therefore likely that the ethnic groups that came from the south can be brought into closer connection with the Trojan culture.

The first Bronze Age civilizations in Europe were also influenced by East Asia: in the middle of the 3rd millennium BC on the island of Crete and on the coasts of the Aegean Sea. Shortly afterwards, bronze appeared in other southern coastal regions and from there it spread further north along the rivers: around 1900 BC it reached the Carpathian Basin, and then around 1700 to other regions of Central Europe. From the direction of spread, it can be clearly established that here too East Asiatic influences pushed from the coasts towards the interior, as it were, following the trade routes.

But we know not only about trade relations. At the turn of the 3rd and 2nd millennium BC, new ethnic groups from Asia Minor — according to several experts, from Transcaucasia — pushed through the Balkans into the Carpathian Basin and the territory of present-day Romania. They brought the knowledge of bronze. Shortly before that, some ethnic groups of the pit grave (kurgan) culture of southern Russia also moved west and penetrated the Tisza region in several waves.

23

===== PAGE 21 =====

Here they mixed with the people of the Pecel (Baden) culture, a significant portion of which, as we saw earlier, came from the area of Troy in the second half of the Copper Age.

In the Middle Bronze Age, two advanced metalworking centers developed in Central Europe. In the area of the so-called mound-grave culture, the Alpine one in western Hungary, and the Transylvanian one in the east. The border between the two was roughly the Danube-Tisza region. The Transylvanian bronze-working center occupied an outstanding place in Europe at that time with its wealth of forms and the level of craftsmanship. It had very strong connections with the metalworking centers of the Pontus, the Kuban region and the Caucasus. At first glance, the circumstance that strikes us about these Central European bronze centers is that both centers developed in areas where large-scale pre-Asian settlements could be repeatedly registered in earlier times.

In presenting the European Neolithic, Copper and Bronze Ages, we have so far mainly relied on the works of Nándor Kalicz, Tibor Kovács, Gyula László and Jan Filip given in our bibliography.

However, it is essential to mention that one of the most outstanding experts on ancient Aegean and Balkan civilizations, Fritz Schachermeyr, in his cited work published in 1976, comes to a fundamentally different conclusion regarding the beginnings of pottery in southeastern and central Europe. According to him, already in the early Vinéa period - i.e. around 3500 BC - single-colored, glazed pottery appears in the northern Balkans, which undoubtedly belongs to the so-called "metal shock" (metallurgischer Schock) sign with the potters: i.e.

that his . . . They tried to imitate "modern" metal vessels. This "fashion" was soon adopted by a number of surrounding cultural groups. Schachermeyr's observations thoroughness (he had already expressed his metal shock thesis in the 1950s) is strikingly proven by the recently discovered 4th millennium copper mines near Belgrade. On the basis of these facts, Schachermeyr considers the existence of a northern Balkan-Transylvanian metalworking center to be proven in the 4th millennium. But he says even more. He undoubtedly shows northern Balkan, 51276640 and Vinca influences in the contemporary Greek cultures. Later, between 3000 and 2500, he states the "massive influence" of the Bükk, Tisza and Tordos cultures on the Thessalian Arapi and Dimini cultures.

He lists ample examples from both periods to prove his claim. We can therefore consider it very likely that at the end of the 4th millennium the Carpathian Basin area was included. Schachermeyr is not alone in his views. Similarly, the world-famous English archaeologist and cultural historian Gordon Childe 1s concludes in his work "Vorgeschichte der europäischen Kultur" that, in addition to the Neolithic cultures of the Balkans and the Carpathian Basin, the ribbon-decorated

===== PAGE 22 =====

He also attributes the Râmia region to the pre-Asian Tclepsccknck, together with the Cucuteni-Tripolje culture of western Moldavia in Ukraine, which he believes was formed by the further expansion of the pre-Asian population of the Carpathian Basin and Balkan cultures. The finds of the Tripolje culture fully correspond to the historical material of the earlier North Balkan and Transylvanian cultures, he writes (mother goddess, fertility cult, seal presses, etc.). In discussing the Bodrog-Keresztúr and Pécel cultures (at the turn of the 3rd and 2nd millenniums), he emphatically points out that exact copies of the burials with oxen burials here can be found in the royal tombs of Ur from around 2600 and in the Maykop kurgans from around 2300-2200. The many parallels definitely prove organic Mesopotamian connections. In Transylvania, we can already speak of a real metal industry during this period. The metal objects are of a distinctly Anatolian type, which is also a clear sign of direct influences from East Asia. Franz Hanáar shares almost the same view as Childe: the Tripoli civilization is nothing . . . In his enormous monograph "The Horse in Prehistoric and Early Historical Times". He states that more than the eastern expansion of the Balkan and Carpathian Basin (Danubian) civilizations and shows very important connections with East Asia. For example, the bull's head buried under the domestic hearth proves the existence of the East Asian bull cult. In Hungary, the construction of the Budakalász chariot model dating from 2000 is completely identical to the chariot finds from Ur.

We will conclude our brief overview with one of Schachermeyr's earlier works, where, speaking of the older, Neolithic times, he explains that the entire Sesklo form treasure can be found in the finds of the Neolithic and Chalcolithic cultural circles of Northern Mesopotamia (Hassuna, Samarra), Syria (Tell Halaf), Cilician (Mersin) and Central Anatolia (AliSar); therefore, we must not only speak of simple coincidences, but of complete identity. Looking further north, the Starévo and Körös cultures are none other than the branch cultures of Sesklo. That is, the Pre-Asian cultures reached the Carpathian Basin by direct expansion as early as the 5th millennium BC. (F. Schachermeyr: Die vorderasiatische Kulturtrift.) Given all these circumstances, it is therefore very obvious that we cannot consider the appearance of the Sumerian pictographic tablets from Lower Tatarlak as a mere coincidence - precisely in Transylvania and precisely at that time (around 3000 BC) when the Carpathian Basin and Danube civilizations were apparently at the forefront of European development. We are talking about much more here than just scattered finds brought there by trade or other means. The drawing of a Cycladic-type sailing ship from the same period, found in the meantime in Cluj-Napoca, can only confirm us in this opinion. But let us not forget to include in our considerations that the expansion of the Carpathian Basin, Transylvanian and northern Balkan civilizations

25
===== PAGE 23 =====

The Tripoli culture also appears in the Moldavian-Ukrainian region at precisely this time -- around 3000 BC. We will return to this circumstance when discussing the development of Eastern Europe and the Volga-Ural region.

Based on what has been presented, we can state that the emergence and development of the Neolithic and Early Metal Age cultures of Southern, Central and Western Europe can only be explained by the direct and massive influence of the pre-Asian primary cultures. In addition, the mass immigration waves of the pre-Asian colonists — which can be dated beyond all doubt — and then continuously repeated for 3000 years raise such primary paleoethnographic and paleolinguistic questions that no future research with the claim to scientific method can ignore.

Asia II
The Mesopotamian Hassuna culture appears before 5000 in the finds of the Tepe Sialk I period in Iran, and soon afterwards in Turkmenistan (the Jeitun or Chagallı culture, then the so-called Mondjukli period), with agriculture, village settlements and pottery. Shortly afterwards, the ceramic types of Samarra and Tell Halaf, then Eridu and Ubaid also appear almost unchanged in Western Iran (Sialk III), Northern Iran (Hissar I), Turkmenistan (Namazga I), Afghanistan (Mundigak I) and Baluchistan. The geometric patterns found on the ceramics of Tepe Sialk II/5 layer (first half of the 4th millennium) can almost be confused with the contemporary Ubaid ones. (Jean L. Huot: Persien I. p. 107. Archaeologia mundi.) The female clay figurines of the Namazga I period (3800-3500) in Turkmenistan are surprisingly similar to the contemporary Ubaid ones. (George F. Dales: Turkmenistan. Propyläen Art History, 1974. XIII. kötet 168. old.) Copper also appears in the Namazga I period. These cultures were created by a mass immigration from Mesopotamia through Western and Northern Persia to Turkmenistan. (K. Jettmar el alii: Geschichte Mittelasien, 1966. Handbuch der Orientalistik; SP Tolstow: Auf den Spuren der altchoresmischen Kultur.) In the aforementioned Tepe Sialk 111/4-65 layer, as well as in the contemporaneous Tepe Hissar I period, the potter's wheel appears around 3500, the earliest use of which can be traced back to the Mesopotamian Late Ubaid and Early Uruk periods, between about 3800-3700. In Turkmenistan, we can see a completely parallel development at the same time. In Namazga II. In the period (3500-3000) the use of seal presses begins here too, and copper becomes increasingly widespread. These influences can be traced uniformly from Afghanistan to Baluchistan. Female clay figurines are also found in large numbers throughout the area, not one of which can be called a true work of art.

26
===== PAGE 24 =====

At the end of the 4th millennium, in the Sialk IV period, we see another strong Mesopotamian influence. Uruk pottery and Sumerian pictographic tablets appear, of Mesopotamian origin. accompanied by seal cylinders. In the V. layer of Godin-Tepe (between Kermansah and Hamadan), also in western Iran, we find typical Uruk pottery from the same period, probably brought by a colonial population from Mesopotamia or the plains of western Elam, directly bordering Mesopotamia. (P. Amiet: Die Kunst des Alten Orients. p. 552)

The Sumerian city-states that were consolidating around this time therefore extended their cultural influence to the western Iranian territories very early on, sending out swarms of salt settlers there. However, Sumerian influences soon reached Turkmenistan, and can then be traced through Afghanistan and Baluchistan all the way to the Indus. In these areas, between 3000 and 2500 BC (the Namazga III period in Turkmenistan), we can observe the beginnings of urbanization. Citadels and urban settlements emerge: Altın Tepe in Turkmenistan, Mundigak in Afghanistan.

Most researchers explain this extremely rapid Mesopotamian-Sumerian cultural diffusion, which affected distances of several thousand km, by the development of the lapis lazuli trade routes. The Sumerians also held lapis lazuli in great esteem, which was abundant in Afghanistan. It was therefore important to organize and secure its import. We cannot yet definitively decide how the Sumerians solved this task: whether through a chain of colonial settlements or military garrisons, or in some other way. However, it is certain that lapis lazuli flowed continuously and in large quantities into Mesopotamia from the end of the 4th millennium, which in turn presupposes not only a firm control of the trade routes, but also a large-scale organization and development of mining in Afghanistan. We do not believe that we are far from the truth if we attribute the rapid development of urban settlements to the 4th-3rd millennium. after the turn of the 3rd millennium, precisely along the northern lapis lazuli route (Sialk IV, Hissar II, Turang-Tepe in Iran, Namazga III, Altın-Tepe in Turkmenistan, Mundigak in Afghanistan), we attribute it to conscious Sumerian colonial activity. All the more so because there is unmistakable human evidence in northern Iran and Turkmenistan of a second ~ large-scale immigration from Mesopotamia that took place around 3000. Between 3000 and 2000, close contacts between Mesopotamia and Turkmenistan were constant, and even immigration from Mesopotamia to Turkmenistan continued throughout the 3rd millennium. (Jettmar et alii: id.m.; Tolstow, SP: Auf den Spuren der altchoresmischen Kultur.)

These repeated Mesopotamian-Sumerian settlements and colonizations, which can also be detected archaeologically, also prove that the advanced, cast-iron

===== PAGE 25 =====

The rapid growth of the Mesopotamian-Sumerian population living within the framework of organized city-states practicing intensive agriculture made it necessary to remove the surplus population at regular — not too long — intervals, which, according to the above facts, was solved by sending out swarms of colonists and settlers. We have seen in the above that this process began already during the Neolithic and the Chalcolithic periods, and filled the sparsely populated areas of Southern and Central Europe, as well as Iran and Turkmenistan, with repeated waves of settlers from East Asia.

However, there was another southern lapis lazuli route. This route reached Afghanistan via Elam via Tepe Yahja in southern Iran and Sahr-i-Sokhta in eastern Iran. Several Mesopotamian cylinder seals from the Jemdet Nasr period have been found at Sahr-i-Sokhta. Based on these traces, research attributes a very significant role in the emergence of the Indus (Harappan) civilization to the Mesopotamian cultural impulses coming from both directions and meeting in Afghanistan and Baluchistan, although due to the very incomplete archaeological research of the intervening Iranian-Afghan regions, we are not yet clear on this issue.

In Turkmenistan, in the following period, from 2500 to 2000 BC, which corresponds to the Namazga IV period, development initially continued unabated. Turang-Tepe, Namazga-Tepe, Altın-Tepe grew into real cities. According to the Soviet scientists who excavated it, Altın-Tepe was surrounded by a wall. A Sumerian-type stepped tower temple (zigurat) with a floor area of 27.5 x 10.5 m and a height of 8-10 m, i.e. of considerable size, was built there, which was significantly expanded in later times. In its final form, its facade was 55 m long and at least 12 m high. In Turang-Tepe, in the southeastern corner of Lake Caspian, an even more monumental tower temple was found in recent years. These zigurats are undoubted evidence of the pre-Asian hegemony of the Sumerian civilization in the 3rd millennium BC, extending to Turkmenistan. (P.Amiet: 1.m. p. 137.)

The potter's wheel was already in general use everywhere at this time. Bronze was also known. Mundigak in Afghanistan was also a city at this time: with a wall, a monumental palace and a temple. (Mundigak IV.)

However, by the end of the 3rd millennium - according to Soviet scientists, somewhat later - development came to a halt, and the cities of Turkmenistan were soon depopulated - as was Mundigak. No satisfactory explanation has been found for this striking phenomenon. Most people look for the cause in ecological changes, in the drying up of the oases.

The question of where these peoples of Mesopotamian origin and Mesopotamian culture disappeared around 2000 BC, who could already look back on millennia-old cultural traditions, can only be answered by examining the adjoining areas of Asia Minor.

28

===== PAGE 26 =====

By the beginning of the 3rd millennium, following the second large-scale Mesopotamian-Sumerian colonial wave mentioned above, the rapidly growing, urbanizing Turkmen civilizations themselves had expanded their settlements to more distant regions. During this period, agricultural settlements appeared one after another along the Aral Sea and the Amu Darya, as far as Fergana — with pottery that was almost identical to that of Turkmenistan. (GF Dales: Turkmenistan, Prop.Kunstgeschichte XIII. 1.

Jettmar et alii. id. m.) Moreover, this 8 — ultimately Mesopotamian — colored pottery continues eastward in an unbroken chain during the 3rd millennium and can be traced all the way to the Chinese Neolithic Yang-Shao culture. At this time, wheat and cattle also appear in China — demonstrably from the west.

These archaeological finds point to a western migration, the route of which led along the southern edge of Turkmenistan and parallel to the Tien Shan range to China. The people were anthropologically in all probability Turanoid and originated from the area of pre-Asian agricultural cultures.

in the middle of the 3rd millennium BC. This migration is also evidenced by the multiple pre-Asian parallels of the Yang-Shao culture, such as the "sacred" cattle - also as sacrificial animals - the similarities of the earth-bound (chthonic) deities, the ritual masks, the fertility rites, the moon worship, the festivals, the similarities of astronomical knowledge, the identical forms of ox-carts, the axes, the appearance of bronze. Most of the parallels point to the Mesopotamian Ubaid period as the ultimate source.

(F.Kussmaul: Frühe Nomadenkulturen in Innerasien; G.Childe: Stufen der Kultur.) North of Turkmenistan, roughly beyond the Aral-Käspii line, with connections traceable up to the Ob-Irtis region, in the 3rd millennium we find the primitive Kaltenminar culture, with characteristic comb-imprinted ceramics, but without the knowledge of metals, at a Mesolithic cultural level. The very similarly decorated ceramics of the later eastern groups of the Ural-Finno-Ugric peoples partly originated from here. It cannot yet be decided whether the northern appearance of comb-imprinted ceramics at the end of the 3rd millennium can be explained by a simple adoption or perhaps by the northward migration of Kaltenminar people groups. However, certain signs point more to the possibility of migration. Such are, for example, the striking similarities between the tools of the Paleolithic-Mesolithic settlements in the East Urals, as shown by Chernyvetsov. Now, we note that in the 19th century, no archaeological connections can be demonstrated between the ethnic groups of the Eastern, Trans-Ural, and European groups in the periods discussed here. In the region of the Altai-Sayan Mountains and the Yenisei River, around 2200 BC, the Afanasyevo culture developed, the finds of which include numerous undeniable Mesopotamian parallels.

===== PAGE 27 =====

We find a connection — through Western Iranian and Turkmen mediations — especially in the shape of the tools and the ceramic motifs.) Das Pferd... etc.) Similarly, very close parallels can be shown with the pit-grave culture of southern Russia and the Tripoli culture. Apart from the connections of the material relics, however, the most surprising thing is that the anthropological picture of the Afanasyevo and pit-grave cultures is also the same. In both areas, the same swarthy, low-faced and rather short-headed people

We find a "plump-Europid" population, which in later times, until the appearance of the Mongoloids, will constitute the dominant human type of the Eurasian steppes. (HanÉar: im, 1956.; Jettmar et alii: im; Childe,G.: Vorgeschichte der europäischen Kultur.) However, from the point of view of our current investigations, the most important circumstance can be considered that this human race first appears at the end of the 3rd millennium BC only in the Altai-Yenisei region and in southern Russia, at a distance of about 3000 km from each other, while in the geographical space between them, in the region of the Ob, Tobol, Irtysh, Ural, there is still no anthropological link between these two very distant but anthropologically identical groups.

From these beginnings, after 1700 BC, the unified Andronovo culture developed in the vast area stretching from the Yenisei, Altai to the Urals, and from the Ob-Irtysh-Tobol to Kazakhstan. Its population, now uninterrupted, from Eastern Europe to Altai was of the same "plump-Europid" anthropological type as the peoples of the Afanasyevo and Pit-Grave cultures at the turn of the 3rd and 2nd millennia. Research explains the great expansion of the Andronovo culture by the slow, continuous ethnic and cultural integration of the Afanasyevo, Northern Turkmen, Pit-Grave, and Early Bronze Age cultures* that have appeared in the Volga-South Ural region since 2000. (HanÉar: im, 1956.) In this period, agriculture becomes general in the steppe and almost A very high level of bronze art, both in terms of form and craftsmanship, appears without any transition, and it is already being produced for export (Jettmar et alii: im).

The above data clearly indicate that the depopulation of the Turkmen urban cultures at the end of the 3rd millennium and the development of the Afanasyevo and then Andronovo cultures must be assumed to be ethnogenetically, Moreover, there is a very significant cultural-historical difference between the emergence of the Chinese Early Bronze Age, which grew out of the Yang-Shao civilization and appeared after 2000 BC, and especially linked. Between 2500 and 1600 BC, the entire Eurasian

* We will discuss these in more detail in the next section, when discussing Transcaucasian and Eastern European development.

30

===== PAGE 28 =====

In the steppe, a higher culture can undoubtedly be established. This new population introduced animal husbandry and partly agriculture, and brought metals and bronze to the Cro-Magnon type of indigenous population (A. Randa: Handbuch der Weltgeschichte).

Archaeological research in this region has not yet reached the stage where we can draw more far-reaching conclusions 10. However, we consider the circumstance to be very important and worth considering - 1 we emphasize again - that the population of the Afanasyev and pit grave cultures, despite the enormous distance and lack of communication, shows the same anthropological picture, and in addition, their memorial material is largely the same.** We will return to this question later.

III. Caucasus, Pontus, Volga-Ural region

Before we can begin to examine the Pre-Asian connections in the areas north of the Caucasus, we must also deal in some detail with the developments in eastern Anatolia, Armenia, northwestern Iran, and Transcaucasia. In this region, the conditions are much more complicated than in the regions discussed so far.

On the other hand, this part of East Asia is closest to the Volga-Ural region, which is of particular interest to us.

The early civilizations that developed here could easily come into contact with the Pontic, Southern Russian and Ural regions at all times through the Caucasian gates and the Caspian-Volga-Ural or Black Sea-Don-Dnieper waterways. It is therefore obvious that Transcaucasia primarily conveyed the Pre-Asian influences to these areas. Therefore, in our introduction, when briefly describing the ancient civilizations of Pre-Asia, we will not even touch on Transcaucasia. It seems more correct to discuss the development here in conjunction with the Eastern European one. We would like to point out in advance that the earliest known settlements of food production and settled lifestyles in Anatolia, Southern Armenia and the Zagros Mountains are quite close to this broader Transcaucasia (including Eastern Anatolia, Armenia and the northwestern part of Iran). That is, here we are actually facing the immediate northern fringes of the most ancient pre-Asian Neolithic zone.

** We met B. Stawiski after the manuscript was completed: the Mittelasién. — Die Kunst der Kuschani", in which on page 19 the population of Turkmenistan is moving northwards

author nailed his blunder. (More on l. p. 322.)

31

===== PAGE 29 =====

We have already mentioned that hammered copper pieces from the beginning of the 7th millennium BC were found in Cayönü in Eastern Anatolia. Traces of advanced construction were also found there. However, due to the shortcomings of archaeological research, we are not yet able to follow these promising beginnings in a coherent manner in Eastern Anatolia. Only a few scattered settlements have been excavated so far, such as Elazig in the Keban Dam region, which dates back to 6000 BC, or Tilki-Tepe on the shores of Lake Van, which dates back to around 5000 BC, where first-class quality Tell-Halaf pottery was found. This strong Mesopotamian influence can be linked to the nearby obsidian deposits (Nimrud Dag), from which, according to mineralogical studies, the vast majority of obsidian finds in Mesopotamia at that time come. We have no finds from the 5th millennium so far, and from the 4th we only know the Karas settlement near Erzurum.

Eastern Anatolia was apparently very sparsely populated during the Neolithic and Chalcolithic periods and only later became involved in the development of Transcaucasia.

Much more important are the Neolithic and Chalcolithic settlements excavated around Lake Urmia. This region formed a coherent, unified cultural circle with Transcaucasia, especially the Kura and Araxes valleys, in the 6th-4th millennia. This early development is not surprising, considering that the agricultural and animal husbandry system developed in the nearby Zagros Mountains.

one of the oldest centers of life. Ganj-Dareh-Tepe (C*-date: 8450±150 BC), Jarmo (early 7th millennium BC) and Tepe Sarab (6250-5850 BC) lie barely 150-200 km from Lake Urmia. The earliest Neolithic influences therefore reached here directly, even before the development of southern Mesopotamia began. The age of the lowest layer of Hajji Firuz along Lake Urmia, determined by the C* method, is 5537±89 BC, that of Janik Tepe (southwest of Tabriz) is 5297±71 BC.

In the Araxes Valley, some Neolithic sites are dated by Soviet archaeologists to 6000 BC, but in the absence of C* measurements, their age can only be given approximately. We have C* data from Somu-Tepe and Sulaveri in the Kura Valley. The former dates to 5560-70 BC, and the latter's I. layer to 4659-210 BC.

However, after their formation, the large southern cultural circles began to influence the initially independent cultures of the Urmia-Araxes region more and more strongly. The very first massive emanations of Tell Halaf and Ubaid were directed towards the northern Iranian region adjacent to these areas (see Part H). In Tegel — next to Esmiadzin — pottery very similar to the Tell Halaf ware was found from the 5th millennium. Imported Halaf pottery from the turn of the 5th/4th millennium also occurs in northwestern Azerbaijan and in Kultepe near Nakhichevan. However, the local pottery of the 5th millennium (Dalma Tepe) is of a completely different nature and cannot be compared with that of Tell Halaf.

In the 4th millennium, Mesopotamian influences significantly increase. Pisdclli-Tepe (on the Solduz plain) and Geoy-Tepe (around Lake Urmia)

===== PAGE 30 =====

The pottery of the Nyekens partly shows Ubaid traditions, and partly a characteristic, red, brightly colored type of pottery, which is also characteristic of Yanik-Tepe during this period.

At the latter site, it appears in a virtually identical form from the end of the 5th millennium to the end of the Chalcolithic Age, around 3200 BC, in a layer 9 m thick. This unbroken tradition spanning a thousand years makes it very likely that this ceramic style originated in the area around Lake Urmia. From a general cultural-historical point of view, it is very significant that this red, glazed pottery is surprisingly similar to the red version of Uruk pottery that appeared in Mesopotamia in the first quarter of the 4th millennium, around 3800 BC, the latter of which is associated by the vast majority of researchers with the Sumerian immigration. From the parallels of pottery along Lake Urmia, many scholars conclude that the Sumerians migrated into Mesopotamia from the Transcaucasian region immediately before the Uruk period and brought their characteristic pottery with them (Hrouda: Vorderasien. — Handbuch der Archäologie; Heine-Geldern: China, die ostkaspiische Kultur und die Herkunft der Schrift). This assumption would explain several controversial questions regarding the more detailed circumstances of the development of Sumerian culture.

Close parallels to the Dalmatian pottery of the 5th millennium have been found at Ilani-Tepe in Azerbaijan, from the beginning of the 4th millennium. It is also here, and also at the beginning of the 4th millennium, that the comb decoration method first appears on clay vessels. Thus, the homeland of this decorative motif, which later appeared en masse in the northern regions of Eastern Europe and Western Asia, as well as in the Eastern Minyan civilization, was Transcaucasia. For now, let us be content with recording this fact. We will follow the northward migration of comb pottery when discussing the Eastern European civilizations.

In Georgia, from the end of the 5th millennium onwards, we know of a significant number of settlements with their own local pottery, architectural traditions, and especially highly developed stone-working methods. (Zopi, Aruchlo, Saduchlo, etc.) At the end of the 4th millennium, similar settlements appear north of the Caucasus ridge: Samele Klde in northwestern Georgia and Som Legat in the northernmost part of Ossetia. Their ceramics are related to the southern Transcaucasian finds. In the last quarter of the 4th millennium BC, a unified "early Transcaucasian civilization" developed in the Caucasus, Upper Euphrates and Lake Urmia regions, encompassing Transcaucasia, Eastern Anatolia and Northwestern Iran. Soviet archaeology calls this culture the Kura-Araxes civilization. The unity of the culture, which is represented by completely identical artifact complexes over a large area, cannot be explained solely by the organic development of earlier, mosaic-like local clusters. The assumption of a common ethnic base seems to be the only acceptable one. All signs point to the appearance of a new population around 3250 BC and the rapid expansion of the

33

===== PAGE 31 =====

took possession of the entire area. The pottery from Malatya (Eastern Anatolia) to the Caucasus and from Erzinçan to Lake Urmia is completely identical during the following period — more than a thousand years. Only after 2200 does it begin to gradually break down into local variants. The early Transcaucasian civilization is divided into three distinct periods (I., II., III.). The limits of its extension north of the Caucasus cannot yet be determined due to the lack of appropriate finds. So far, we know of only two settlements in northern Georgia (Kvatzchelebi and Amiramis gora), which can be traced back to 3000 BC by radiocarbon measurements. We only have indirect data on the identity of the aforementioned newly immigrated population, but these are relatively numerous and, on the other hand, determine the population of the newcomers with a probability that almost reaches the limit of certainty. It appears in early Sumerian written records to designate the areas north of Mesopotamia, in an inscription of "subar-ki", i.e. king of Lagas\$i, from the 27th-26th "subar-land" term -- first used in Eannatum centuries BC. After that, the name is continuously found in the phonetic forms "subur-ki" and "subir-ki". The Semitic Akkadian language adopted this Sumerian term in the form "subur-tu". The early Sumerians had very close ties with this subaric land. One of the most ancient—c. The Sumerian epic (Enmerkar and the Lord of Aratta) from 3000 BC tells us that Aratta, which lies to the north, "beyond the seven mountains and seven valleys", sends metalworking masters and ore to Uruk. SNKramer, one of the most renowned Sumerologists of our time, places Aratta in the northern part of the Zagros Mountains or in the southern region of the Káspi-tó. If we now recall what has been said about the red, glazed pottery of Transcaucasia and Uruk-era Mesopotamia in the 4th millennium, we must conclude that we have probably come one step closer to the key to the mystery of the Sumerian migration. This opinion is also confirmed by the statement in the Sumerian king list 6s, according to which the first antediluvian king, Alulim, ruled in Eridu and Subaru.

The term "subar-ki" or "subir-ki" therefore undoubtedly referred to the Transcaucasian, northwestern Iranian and eastern Anatolian regions in the 3rd millennium BC, and even northern Mesopotamia, later Assyria, was included in the term "subar-ki". Examining the ethnic side of the question, we need to deal more with the so-called Hurrian peoples. According to later, 2nd millennium data, these Hurrians lived in the area of "Subar-ki". Previous research generally believed that the Hurrians. They were previously called "Hurrites" based on the Bible. They migrated to the regions in question at the turn of the 3rd-2nd millennium, from somewhere in the eastern or northern region of Lake Káspi.

34

===== PAGE 32 =====

However, in the last 20-30 years it has been proven that we cannot count on any Hurrian immigration in this period. In Northern Mesopotamia, more and more Hurrian names appear in written records since the Akkadian period (c. 2450-2300), and from the time of the 11th dynasty of Ur (c. 2200-2100) Hurrian names also occur en masse in Southern Mesopotamia. On the other hand, the sources do not know of any other people north of Mesopotamia, and several records explicitly identify the inhabitants of the old "Subar-ki" with the Hurrians. The Epic of Gilgamesh also mentions, for example, "the country of Hurruru in the mountains", and in a record from around 2000 the expression "the king of Subartu and Ansan" occurs, which indicates the existence of a large, unified country in Northern Mesopotamia and Western Iran, considering that "An-San" was the Sumerian-Akkadian name of Elam. (Ungnad, A.: Subartu.) Ungnad also writes that the Subartu language monuments and the names of the gods both prove that the Subartu language is identical to the later Hurrian. But archaeological finds also testify to the identity of the Subarians and the Hurrians. They show that the Subarian area had been an integral part of the Sumerian-Mesopotamian culture since the beginning of the 3rd millennium, and then in the middle of the millennium the population began to expand, initially slowly, to the south and southwest, which intensified into a real migration at the turn of the 3rd and 2nd millennia. The peoples who flooded northern Mesopotamia, Syria, Cilicia and Palestine in the first half of the 2nd millennium are clearly called Hurrians in the sources.

But let us examine more closely what the finds show. As in all ancient times, here too we can use ceramics as a guide. The precursors of the typical ceramics of the early Transcaucasian civilization are found in the finds of Samele Klde, Chiatura, Zopi in Georgia, and in the 4th millennium settlement of Sengavit near Yerevan (the "proto-Kura-Araxes" type of Soviet archaeology). Here, in the Araxes valley, we can locate the center of the culture, although due to the small number of excavations, the last word on this issue cannot yet be said.

From these beginnings, before 3000, the unified Transcaucasian pottery developed. Very close parallels to this pottery have been found in the princely tombs of Alaca-Hüyük in central Anatolia dating from about 2500 BC, but it also occurs in large numbers in the Amug Plain in northern Syria and in several settlements in Palestine, also belonging to this period. This latter Palestinian pottery is called Khirbet-Kerak ware in the literature, after the first site in the Jordan Valley. However, further excavations have shown that this is in fact nothing more than a variant of the typical pottery of the early Transcaucasian culture. We can therefore conclude that already in the middle of the 3rd millennium, Transcaucasian ethnic groups appeared in Central Anatolia, as well as in Syria and Palestine, predating the latter location.

===== PAGE 33 =====

ken the Semitic Amorites. Around 2100 this typical pottery also appears in Cilicia, and in Northern Syria, Northern Mesopotamia — in the vicinity of the Kabur River — these types of vessels are found already in the Akkadian period — and in association with a large number of Hurrian personal names in the former written records. This circumstance undoubtedly proves, on the one hand, the identity of the Hurrians and the early Transcaucasian civilized population, and on the other hand, that the ethnonym Subar-Subur-Subir also means the Hurrians - or at least a part of them.

The Hurrian language was an inflectional language. According to the unanimous statement of international linguistics, it is not related to either the Semitic or the Indo-Germanic languages. Some of its linguistic phenomena point towards Sumerian, but the small number of surviving linguistic monuments does not yet allow definitive comparative linguistic studies. However, the later Urartian language is undoubtedly a development of Hurrian.

To the west of the Hurrians—in Asia Minor—we find the Hattian peoples in the 3rd millennium. Their language is also inflected and neither Semitic nor Indo-Germanic. In our review above, we have mainly relied on the works of Burney-Lang: Die Bergvölker Vorderasiens; Ungnad: Subartu; Oppenheim: Der Tell Halaf and Geotze: Die Kulturgeschichte Kleinasiens (Handbuch der Altertumswissenschaft), among other relevant works given in the literature. Having reached this point, we cannot ignore certain very significant connections that arise in connection with the country and people names subar-subur-subir, which, to our knowledge, were first pointed out by Viktor Padányi. Dentumagyarina" (pp. 123-126 and 241-245). These ethnonym parallels are so surprising that we considered it necessary to examine the authenticity of Padányi's data. The results of our research in this direction so far are summarized below, in a nutshell.

As we have The term "subar" referred to the region north of Mesopotamia and its inhabitants. In the 2nd millennium, however, it was mostly used in connection with these areas seen, we read about the Hurrians in Assyrian-Babylonian sources, but the name "Subar-ki" also lives on in the Semitic form "Subartu", mainly to designate the Hurrian state also called "Mitanni", and also as the name of Assyria, until the 14th century. (Fischer: Weltgeschichte, chapter VI.) In fact, even in the 1st millennium it occurs several times instead of the name of Assyria. For this we also need to know that the original, 3rd millennium "Subar-ki" concept included the later Assyria, and then in the 2nd millennium, during the flourishing of the Mitanni empire, Assyria was an integral part of this empire for a long time, and its population was even very much mixed with the Hurrians. In the 1st millennium, the main enemy of the Neo-Assyrian empire was the

36

===== PAGE 34 =====

The Hurrian successor was Urartu, whose borders, even at the height of the Assyrian empire, were barely 50 km from Nineveh. These data -- taken in themselves -- could undoubtedly be explained as literary clichés, just as, for example, Byzantine or Western European sources often call the various migration peoples Scythians. However, we also have data that do not allow this "topos" interpretation, because they indicate that a people or group of people must have lived in the Transcaucasian-Armenian regions since ancient times, whose name serves as the basis for the name Subar-Subur-Subir. Even more surprising, however, is that in close connection with this people, as it were, another people's name appears in a series, namely in the form of "mada", , mata", , manda", , mita", etc. Around, although some Hittite texts from Bogazköy already mention the "mada" people, placing them in the time of the Akkadian king Naramsin (c. 2300). (Ungnad: Subar-tu.) But the name Mitanni itself also belongs here from the middle of the 2nd millennium. Then in the 1st millennium, the The ethnonym "mada" first appears — beyond all doubt — in an inscription of the Old Babylonian king Ammiditana in 1600 BC. Assyrian king Sulman-ASSaredu (with Greek-Hebrew distortion, Salmanassar") again mentions the "Matai" or "Madai" people east of Lake Urmia (in 836). From this time on, we can already date the parallel occurrences of the people names "Subar" and "Mada" in a continuous, unbroken chain:

Kr.e. 7th sz.: Kastaritu "Sapardai" in the army of Prince Mada (König: Älteste Geschichte der Meder und Perser). Scythian royal names in Transcaucasia: Partatua and Madyas, which are "old Cappadocian-Subaric names" (King: im). The prophetic question of Sarhaddon around 670: "will Dusanni of Sapar" attack this and that city? (Soden: Der Nahe Osten im Altertum.) 6-2nd century BC: In the Behistun inscription of Darius I, the provinces of Media and Sapardlia are listed directly next to each other (around 520). In the Zagros region there was a Persian province called Matiene, which included the Sagartoi district.

Herodotus lists the Matieni, Sapiri and Makroni peoples in Transcaucasia right next to each other (around 440). Several Greek writers mention the Tibari and Moseri peoples in Transcaucasia, and according to the Bible, the Tubal and Mesek peoples are engaged in metalworking in Transcaucasia. These latter names are undoubtedly "subar" and "machar"

37

===== PAGE 35 =====

"corrupted forms of folk names," writes Hanéar in his work "Urgeschichte Kaukasicns."

Xenophon mentions the Makron and Machar peoples in the Erzurum area (around 400).

1st century AD: According to Pliny, the Machoron people were around Bayburt, the "Saspeir" people were in Transcaucasia.

2nd century AD: According to Ptolemy, Mazara and Siavara are cities in the area of the sources of the Tigris and Euphrates.

4th century AD: City of Magarra at the source of the Tigris (Markwart:

Southern Armenia and the Tigris Valley). According to several Syrian and Persian sources, Sabirs live in Dagestan (Haussig: Theophylakt's Exkurs über die skythischen Völker; Altheim: Geschichte der Hunnen, vol. II.)

5th century AD: The Sabirs appear in the Caucasus-Volga-Don arc (Priscus).

6.52 AD: Sabirs (Huns) with Kuban, their king's name is Muageris (Menander, 518).

Jordanes: the Sabirs or Hun-Ugors are a people, descendants of the Huns (around 550).

Mazar castle near Bayburt, Sabir troops in the Greek army there (Theophylaktos Simokatta, 583).

8th century AD: Kangvar (Kingivar) fortress at one of the sources of the Tigris, which is the nest of the Magerdan tribe (Markwart: 1st century).

9 AD: The city of Zubatra in southern Armenia is captured by the Byzantines (reported by the Monophysite patriarch Dionysius from 837).

10th century AD: Masoud mentions the Magerdan tribe in Azerbaijan. At the same time, he states that the real name of the Khazars is Sabir, only the Persians call them Khazars.

The list is far from complete, as it is not our task to discuss these data in more detail here. However, we considered it necessary to at least point out the significance of the issue, because in our opinion it may constitute a very important component of the ethnic dimension of Transcaucasian-Steppe1-Ural-Volga relations to be discussed further.

On the other hand, we were not only able to confirm Padányi's data, but we also succeeded in expanding it with additional sources.

Returning to the discussion of the early Transcaucasian civilization, based on the archaeological and written records outlined above, we can state that its creators and carriers could only have been the Hurrian people, who, in turn, are identical with the Subarians. They were in constant and close contact with the contemporary Sumerian-Akkadian culture, and were, as it were, provincial functions of it. The same-38

===== PAGE 36 =====

They also maintained lively contacts with the ancient Hattian peoples of Asia Minor, and apparently mixed with them (Alaca Hüyük). Their settlements are also found north of the Caucasus ridge. We have also seen that ceramics with 16565 decorations first appear at Ilani-Tepe in Azerbaijan at the beginning of the 4th millennium.

To these brief, general statements we can add a characteristic type of architecture, especially widespread in the eastern regions, from Dagestan to Lake Urmia, the round houses, in the middle of which we find the pillar supporting the roof, and next to it the usually portable hearth. This solution is surprisingly similar to the yurts of the later steppe peoples, write Burney and Lang in their cited work.

The third stage of early Transcaucasian civilization began around 2300 BC. What is striking in this period is, on the one hand, the disintegration of the previous unity, and on the other hand, the rapid development of the metal industry. The types of weapons and tools are identical to the Sumerian products of the same period. This is the result of a new, even more massive direct Mesopotamian influence, which is also manifested in the Maykop kurgans north of the Caucasus. In the Maykop princely tombs dating from 2300-2200 BC, the highly developed Mesopotamian-Sumerian metal culture appears without any transition, coupled with the types of bronze and precious metal finds found in Alaca Hüyük, also of Mesopotamian origin. For example, a map of Mesopotamia and Transcaucasia was engraved on one of the Maykop silver vessels. The Tigris and Euphrates rivers, the Persian Gulf, and the Caucasus mountains to the north — that is, above — are clearly visible. The master also indicated the characteristic flora and fauna of each region by depicting a tree and an animal. On several gold and silver animal figurines, we can clearly recognize the silage cattle that were bred in Hungary until recently. At the same time, undoubted connections can be established between the metalwork of the Kuban culture in Maykop and the metalwork of Transcaucasia and Transylvania: the characteristic hammer-headed needles, the pointed and broad axes, for example, are the same in all three places (yyyyyy: both 1.m., G.Childe 1.m., 1949.; Burney-Lang im). This circumstance is no longer surprising in light of the early metalworking centers in Transylvania and along the Danube — dating back to the 3rd millennium. We saw above that the knowledge of metalworking reached Transylvania directly through pre-Asian influences, and even through ethnic expansion. This earlier finding is further confirmed in the light of the newly introduced Cuban-Caucasian finds.

In the Kuban region, therefore, by all indications, two far-reaching branches of cultural influences, ultimately originating in Mesopotamia, once again converged and fertilized the development of the areas here,

If we take into account what was said in the first part about the development in Central and Southeastern Europe, then the following are now more clearly outlined:

39

===== PAGE 37 =====

who before our eyes the almost ubiquitous cultural circles of the 3rd millennium BC. We also notice that these Middle Eastern cultural seedlings, which have become more muscular in the meantime and in many places have already millennia-old traditions, are beginning to influence each other more and more strongly, while the direct Mesopotamian — already distinctly Sumerian in this period — radiations do not cease, but continue to influence development continuously.

The development of an independent metal industry in the Caucasus and the Pontic Kuban civilization in the second half of the 3rd millennium BC can therefore be traced back primarily to direct Sumerian-Mesopotamian, and secondarily to Anatolian (Alaca) and Transylvanian influences. The rich local ore deposits greatly facilitated and accelerated this process (Burney-Lang: 1.m.).

The Kuban civilization is the most advanced Pontic center in this period. It is far superior to the cultural development of the surrounding regions. In accordance with this high level of culture, the archaeologically palpable expansion of the Kuban civilization begins towards the north and northeast, and even to the more backward Georgian regions of Transcaucasia. This is indicated by the spread of kurgans here after the turn of the 3rd and 2nd millennium. Evidence of strong north-northeast connections, however, is the start of Ural ore mining already at the end of the 3rd millennium (yyyyyy: im 1956; Childe: im 1949.).

Before we go any further, however, we should briefly outline the antecedents of Eastern European development in the 4th and 3rd millennia, so that we can then recognize the overall picture more clearly.

At the beginning of the 4th millennium, we find descendants of the eastern branch of the Upper Paleolithic Late Gravettian culture living in the Mesolithic cultural stage in the territory of present-day Ukraine and Southern Russia. In the middle of the 4th millennium, in the area of the Dnieper rapids on the Igren Peninsula, as well as on the Sursky and Sulayev Islands, near Melitopol and near Zhitomir, comb-pitted ceramics appear, with characteristic pointed-bottomed vessels and clay thinned with shell powder. (Gy. László: The earliest stages of our history.)

As we saw above, this vessel decoration first appeared in Ilani-Tepe in Azerbaijan at the beginning of the 4th millennium, as a provincial version of the ancient local cultures around Lake Urmia (Dalma-Tepe, Yanik-Tepe, etc.). It is therefore very likely that it spread from there to the Pontic areas. This comb-shaped pottery is particularly interesting for us because in later times it gradually penetrated the entire area of the forest belt, from the Eastern Sea to the Urals, where both the comb-shaped and pitted decoration method and the mixing of clay with shell powder remained in the later Finno-Ugric regions for millennia. (László: im1961.)

The pit grave (kurgan) culture developed in the Pontic region around 2500 BC. However, its development cannot be explained by internal forces.

40

===== PAGE 38 =====

with its effects, because archaeological and anthropological data show that as a result of the appearance of new, short-headed peoples, they assimilated the Cro-Magnon-Proto-Europid indigenous population found here. The ceramics also change, the pottery types and decorative elements of the neighboring agricultural cultures appear. (Hanéar: im1956; Childe: im 1949; László im1961.). If we now take a closer look at these neighboring agricultural cultures, we find in the western neighborhood of the pit grave culture the Cucuteni-Tripolje culture, which arose in Western Ukraine at the end of the 4th millennium through the direct spread of the Balkan-Carpathian Basin cultures. The southeastern neighbor is the The area of the pit grave culture is wedged between these two secondary pre-Asian — Mesopotamian cultural regions. In accordance with this geographical situation, in its development and ethnicity, we can primarily register Balkan-Trypolina and "early Transcaucasian civilization", the northern extent of which is not yet known, due to the lack of adequate archaeological research.

Transcaucasian influences. The parallels of the shrunken burial, the spread of agriculture and pottery can be explained mainly by the east-northeast expansion of the Southeastern European cultures. (Hanéar: im 1956.) The descendants of the Paleolithic core population remained only in the more northern areas (Swedish culture), or a significant part of them migrated even further north and settled on the Paleolithic hunters of Kunda and Sigirya, in all probability playing a leading role in the ethnogenesis of the later Finno-Ugric peoples (László: 1.m.1961.). Archaeological signs of this migration can be seen in the appearance of comb ceramics in the Baltic, Upper Volga and Ural-Kama areas in the middle of the 3rd millennium.

Examining the further development of the Tripolje civilization, we can observe a striking phenomenon that undoubtedly played a very important role in the development of later nomadic societies. In the second half of the 3rd millennium, the importance of animal husbandry in the Tripolje civilization increases dramatically, namely in the direction of herding and pastoral animal husbandry requiring large areas. Undoubted signs of this are the increase in the proportion of horses and sheep from the previous 5% and 15% to 20% and 40%, and the simultaneous decrease in the proportion of pigs and cattle from 17% and 40% to 2% and 28%. The first bits also appear at the same time (Hanéar: Das Pferd... etc.).

This development towards nomadism was not, however, limited to the area of the Tripoli culture. In the same period, we find the same tendency in the Kärpät Basin, where animal husbandry was of much greater importance from the very beginning than in the in the area of "early Transcaucasian culture" — in its III. phase — and of course, to a particularly strong extent in the area of the pit grave culture,

41

===== PAGE 39 =====

elsewhere. The bound steppe lands could not yet be cultivated satisfactorily with the tools of the time; therefore, agriculture was initially limited to the looser soil of the riverbanks.

The reason for these peculiar economic changes can be seen primarily in the increase in the population of agricultural cultures and the gradual depletion of the areas that could be cultivated. New livelihood opportunities had to be sought. Especially in the steppe, but also in the mountainous regions of Transcaucasia, the extensive intensification of the already relatively developed animal husbandry almost spontaneously offered itself as a solution, since in both areas there was plenty of previously unused pastures available.

The new economic form naturally required much larger areas than the previous farming. Thus, the fight for possession of pastures began throughout the region. Archaeological data from this period also reflect this unrest, showing smaller and larger migrations. One branch of the pit-grave culture penetrated the Carpathian Basin and, reaching the Tisza River, mixed with the local population, while also influencing the development there towards semi-nomadism (I. Kalicz first).

Similarly, after 2300 the unity of the early Transcaucasian civilization disintegrates, and the focus of agriculture shifts demonstrably towards animal husbandry, in favor of sheep and goats, which are best bred on mountain pastures. The enormous deforestation necessitated by the population growth reduced the amount of precipitation and thus the yield of agriculture also declined. At the end of the 3rd millennium, semi-nomadism also came to an end here. Around the turn of the millennium, many ancient settlements ceased to exist or declined sharply. The population was on the move throughout the civilization. At first, there were large internal migrations, then the Hurrian expansion began, ending with the conquest of Northern Mesopotamia, Syria, Palestine and Central Anatolia, the most distant wave of which we can also consider the invasion of Egypt by the Hyksos. Meanwhile, as we have already mentioned, certain ethnic groups of the pit-grave Pontic-Maikop culture settled in the sparsely populated areas of Transcaucasia and introduced the kurgan burial method here as well (see the above in I. Burney-Lang: im).

It is striking that despite these large-scale population movements, there is almost no archaeological evidence of a decline in culture or major destruction in the affected areas.

On the contrary, in most places we find a cultural boom, especially in the metal industry. In this period, the Transylvanian, Pontic and Caucasian (MetSamor) (émfives) centers with high artistic and masterly demands are formed (Burney-Lang: im).

It is surprising that the faces of the Late Copper Age — Early Bronze Age cultures from the Carpathian Basin to Transcaucasia show a largely identical picture. This uniformity, which is evident from the findings of the excavated graves, is a common feature.

==== PAGE 40 ====

becomes clear, it takes us back to Mesopotamia, namely to the royal tombs of Ur between 2700-2500 BC. We have already mentioned that the weapons and tools of the Maykop kurgans are identical to the Sumerian forms of the same period. We can now add to this statement that the chariot burials found in Maykop are also exact copies of the burial order of the royal tombs of Ur (Childe: im 1949). The kurgans of the pit-grave origin population settled in Transcaucasia around 2000 — from the middle of the 2nd millennium — show the same wealth of forms. The four-wheeled chariots found here are also completely identical to the chariots of Ur. But we also find these parallels in our country: the ca. The construction of the Budakalász wagon model from 2000 and the Ur wagons are identical, and the wagon burials of the Bodrogkeresztúr culture also show an analogy to the burial order of the Ur royal tombs and the Maykop kurgans (Childe: 1.m. 1949. and 1952; Hanéar: im 1956.).

These similarities again only confirm that — as G. Childe stated in his work "Stufen der Kultur" — at the beginning of the 2nd millennium BC, Mesopotamian culture almost permeated the Central and Eastern European regions.

In our opinion, the most plausible explanation for the massive Mesopotamian parallels and coincidences presented above, which seem incomprehensible at first glance, is that Mesopotamia was inhabited for approximately We must look for decisive historical ethnic changes that took place between 2400 and 1700 BC. The Semitic Akkadians, who had been infiltrating in increasing numbers since the beginning of the 3rd millennium, had become so strong around 2450 that they were able to seize political power (the Sarru-Kenu — with Greek distortion: the "Sargon" dynasty). From the period of about 150 years of Akkadian rule, numerous written records tell of a series of uprisings by the subjugated Sumerian city-states, their bloodshed, the destruction of the cities and the enslavement or expulsion of the population. Each change of throne resulted in such general uprisings, and each new Akkadian ruler had to almost reconquer the empire of his predecessor after ascending the throne (Moortgat: Geschichte Vorderasiens bis zum Hellenismus).

All signs point to the fact that the migration of the Sumerians from Mesopotamia had already begun in these Akkadian times. This is evidenced, for example, by the sudden, almost unprecedented flowering of the Kuban civilization around 2300, but also by the equally rapid rise of Caucasian metalwork. As we have stated earlier, massive and direct Sumerian-Mesopotamian influences occur in both places, so to speak, without transition, around 2300-2200 BC. The form of the weapons and tools, the craftsmanship of their production, are completely identical to the Sumerian products of the same period. Research cannot provide a satisfactory explanation for these surprising parallels, write Burney and Lang in their cited work.

43

==== PAGE 41 ====

Now, from the point of view of the Sumerian migration, all these phenomena become understandable without difficulty. Moreover, starting from the assumption of migration, we almost involuntarily come to the conclusion that if our assumption is correct, we should also expect these symptoms in all the areas to which the Sumerian population migrated. We can place each such region in the Caucasus-Kuban region.

We can also associate with this first wave of Sumerian migration - at least yyyyyyyyyy - those Pre-Asian settlers who arrived in several waves in the Carpathian Basin around 2200 and, as we saw above, formed the Pécel culture, but also had a strong influence on the development of metalwork in Transylvania.

We must also point out the important circumstance that the immigrants did not go to unknown territories, neither in the case of the Caucasus region nor the Carpathian Basin, but to places with which they had been in constant contact, trade and other relations for a whole millennium. Let us think of the close Sumerian-Mesopotamian connections of the early Transcaucasian civilization, or the Sumerian pictographic tablets of Tatarla.

A third direction of emigration also seems to be proven: towards Turkmenistan (1. Jetmar et alii 1.m.), and then further north from there. The development of the Afanasievo culture may also be connected with such settler groups arriving from Turkmenistan. We do not want to rule out this latter possibility for the time being, but we cannot ignore it in further considerations.

The Akkadian rule in Mesopotamia was ended around 2300 by the invasion of the Gutians, who were, however, partly assimilated and partly driven out — after about 80-100 years. Thus, between about 2250 and 2120, a Late Sumerian empire rivaling the size of the Akkadian empire could have emerged under the III. Dynasty of Ur.

However, the Sumerians were unable to resist the two-sided pressure of the Semites (the Amorite tribes at this time) who continued to infiltrate continuously and the Elamites, who had meanwhile become stronger, and the independent Sumerian state finally fell around 2120.

In the following, so-called Isin-Larsa period, the Amorites gradually took over leadership everywhere in Mesopotamia, which had again broken up into small city-states.

We also have written records from the early Isin period, almost contemporaneous with the fall of the IIIrd Dynasty of Ur, about the migration of the Sumerians. The lament of Ibbisin, the last king of Ur, tells of the destruction of the city of Ur and the dispersion of the people. The lament of Ur also speaks of the depopulation of the city and its surroundings. The song says that the Sumerian gods departed from their cities. This expression is also used in the religious cults of that time

44

==== PAGE 42 ====

In the language of the Hebrews, it means that the people took their gods with them when they fled. The same concept is reflected in the Bible: when Jacob fled from his father-in-law's house, his wife Rachel took her father's household gods with her (Gen. 31:19).

In the case of the Sumerian emigration wave that occurred at this time, we can therefore start from concrete, nearly contemporary written records, and it is not necessary to rely solely on logical conclusions. We can again find traces of the Sumerians who moved in the light of archaeological finds.

New settlers appeared in the Carpathian Basin at the turn of the 3rd and 2nd millennium BC. They came from Asia Minor or Transcaucasia. They brought knowledge of bronze and established the outstanding Transylvanian bronze-working center, which had lively connections with the Pontic-Caucasian bronze industry (Kalicz, Kovács et al.).

Around 2000 BC, Caucasian metalworking shows another leap in development. Bronze also appears here, and a real bronze industry develops with the center at MetSamor. Researchers cannot understand how the knowledge of bronze making, which had previously been an exclusive Sumerian monopoly, could have reached such a great distance at such an early date (Burney-Lang: im).

In our opinion, these seemingly vague archaeological data — both in the Carpathian Basin and in the Caucasus — suddenly become clearer if we place the second wave of Sumerian migration after the fall of the Third Dynasty of Ur — for which, as we have seen, there is also written evidence from nearly the same time — as the starting point of our consideration.

In this study, we cannot explain these Sumerian migrations in detail; for the time being, we only wanted to indicate the eminent significance of the complex of questions. All that we would like to note in this regard is that previous research has very easily dealt with the apparent rapid disappearance of the Sumerians after 1700 years of pre-Asian hegemony, or their alleged absorption into the immigrated Semitic population. They have not taken into account the very important circumstance that in 1700 years (from about 3800 to 2100), the population of the Sumerian city-states, which had an extremely high level of culture, developed irrigated agriculture, industry, and long-distance trade covering large areas, had to multiply to such an enormous extent that this mass cannot simply be disappeared or declared absorbed in a short 2-300 years. We would like to illustrate the absurdity of this opinion with the following simple calculation. It is known that in the eras before the industrial and scientific development of the 18th-19th centuries, the population doubled on average every 200 years. Since the economic level of Sumerian civilization differed little from the level of European food production and industry before the 18th-19th centuries, we can safely extrapolate this rate of population growth to Sumerian Mesopotamia. So if we count only 20,000 Sumerian immigrants around 3800 BC, then by 2100 BC they must have numbered at least 5,000,000.

45

==== PAGE 43 ====

This mass of people can neither disappear nor be absorbed within 2-300 years. Especially not if it is a leading nation at the highest stage of development.

cultural ethnicity. The above-mentioned unfounded opinion of the research is eerily similar to the statement of the Nestorian Chronicle, according to which the Avars disappeared without even a trace of them. We believe that we do not need to elaborate on how much truth we can attribute to this statement of the Nestorian Chronicle in the light of recent archaeological and anthropological research. Well, this is exactly the case with the alleged disappearance of the Sumerians. We are also confirmed in this opinion by numerous archaeological and historical data from the 2nd and 1st millennia BC, which point to the migration of the Sumerians within East Asia in the 2nd millennium. This is confirmed, for example, by the surprisingly massive Sumerian parallels of the Hurrian cultural circle of the 2nd millennium, or the very striking similarities between the culture, state administration, and social organization of the Urtanian empire that emerged at the beginning of the 1st millennium with the former ancient Sumerian culture, state organization, and social situation of the 3rd millennium. Before these, the almost incredible scale

research is at a loss in the face of these agreements (Bittel: Basics of the Early and Early History of Asia Minor; Geotze: Hittites, Hurrians and Assyrians; Schmökel et alii: Cultural History

des Alten Orient; Strommenger: Fünf Jahrtausende Mesopotamien; Ungnad: Subartu, etc.). For now, we must be content with this seemingly enormous range of problems.

Based on the picture of culture and popular movements outlined so far, let us now attempt a closer examination of the cultural and ethnic influences coming from the south that can be detected in the Volga-Ural regions.

In the 3rd millennium BC, in the forested zone of Eastern Europe, we find only Paleolithic or Mesolithic cultural levels of gathering, fishing and hunting populations, in very small numbers, in accordance with their primitive economy. Only two or three families lived in the scattered settlements, usually located several dozen kilometers apart, and they also constantly changed their residence. Therefore, we cannot speak of a continuous, compact population in these early times, in these areas. The fundamental, theoretical significance of this fact, which can be verified archaeologically beyond all doubt, is that under such circumstances, of course, the common Finno-Ugric, but even more so the Uralic basic language, was not spoken in the 19th century. s. thesis, in the absence of a suitable coherent ethnicity, literally dissolves into nothingness. Around the middle of the 3rd millennium, the southern groups of forest dwellers had already learned the craft of pottery - as we have seen, they learned the art of pottery from the steppe peoples - but ceramics had not yet reached the more northern regions at this time.

These backward areas were penetrated by the explorers of the Kuban-Maykop culture at the end of the 3rd millennium. The discovery of the copper and gold mines in the Urals led to the discovery of the Urals.

===== PAGE 44 =====

Their discovery and first exploitation, as already mentioned, is attributed to them by research (Childe, Hanéar). This is when the 0156 metal tools appear here, for now only as fragments, and these metal objects are clearly of the Caucasian-Cuban type.

Due to the attractive influence of the Ural ores and the increased demand for pastures due to the transition to extensive animal husbandry, after the turn of the 3rd and 2nd millennia, more and more southern settlers migrated to the Ural-Volga region from both sides of the Ural River, along the Ob, Irtysh, Ufa, Volga, Oka and other smaller rivers. The immigration took place in several waves and continued uninterrupted until the middle of the 2nd millennium. The newcomers arrived in large numbers. They had a significant impact on the paleoethnography and culture of the Finno-Ugric peoples, including the Ugric-Hungarians. They brought with them knowledge of agriculture, animal husbandry and bronze production. In the middle Volga, lower Kama and western Ural regions, the Balanovo, Abasevo, Fatyanovo, Beam-frame and Andronovo cultures were developed. We cannot yet express a definite opinion about their language and their ethnic composition. However, it is conceivable that they were originally ancient Iranians. This is approximately how Antal Bartha (Hungarian Prehistoric Studies) describes this massive southern colonization. In his article "Society and Economy in Hungarian Prehistory"

After Bartha's rather brief summary, let us take a closer look at these southern immigrants. Let us try to uncover the roots of their culture and to find out their "possible" population, as Bartha writes, but rather their most probable not population based on the data available to us. For we — unlike Bartha — do not

We can be content with two commonplaces, thrown in as if by chance, that "they had a significant influence on the paleoethnography and culture of the Finno-Ugric peoples...", and "It is conceivable, however, that they were originally from Ösirän" (Bartha: im, pp. 29 and 30).

Not because both of Bartha's statements are shockingly superficial and at the same time extremely misleading. They are perfectly suited to glossing over and diverting attention from real problems.

First of all, we consider it necessary to emphasize most emphatically that it is hardly possible to speak of "ancient Iranians" at the turn of the 3rd and 2nd millennium BC, given our current knowledge, because we do not have any data from these times that refer to or can be referred to Iranians. Therefore, we cannot consider such assumptions to be anything other than anachronistic reflections left over from the theses of 19th century Indo-Germanic studies. Especially in cases such as the question of the population of these southern settlers. It was already clear more than 20 years ago that, for example, the Balanovo culture was closely connected with the

===== PAGE 45 =====

to the Caucasus and even to Mesopotamia. G. Childe, F. Handar and other renowned researchers established in the 50s-65s that the Balanov chariot models in their raw materials and structure perfectly correspond to the chariots used in Ur and Kis in the 3rd millennium BC, but also to the Maykop chariots from around 2300-2200 and the Transcaucasian chariots from the beginning of the 2nd millennium. The shape of the axes, axes, spearheads, arrowheads, jewelry, certain types of ceramics, and even folkloristic features point to Mesopotamia. The language of the present-day Chuvash, their legends, and folk customs are full of Caucasian similarities. Anthropologically, the closest affinities can be found in the Armenian and Eastern Mediterranean ancestral races. These connections clearly point to strong Transcaucasian and Pre-Asian connections, as well as immigration from the latter areas at the beginning of the 2nd millennium (Hanéar: Das Pferd... etc.; Childe: Vorgeschichte der europäischen Kultur). We emphasize that the opinion of these internationally recognized authorities in prehistoric research about the Early Bronze Age civilizations in the Ural-Volga region was already this way at a time when the archaeological traces of the mass and centuries-long, repeated southern immigrations described above were still largely unexcavated (also the work of IARanda cited). It is all the more incomprehensible that Bartha does not even mention these connections. Yet today, the migration route of these southern settlers and the geographical location of their original homeland can be outlined much more concretely.

We have already mentioned that large areas in the Transcaucasian region were depopulated at the beginning of the 2nd millennium. This process can be clearly confirmed archaeologically (Burney-Lang: im).

There is no doubt that the disappearance of the population can be attributed to a large extent to the southward Hurrian migration, but at the same time we must not forget that the sparsely populated, fertile areas in the immediate northern neighborhood of Transcaucasia, especially for the northern Transcaucasia peoples, were much easier to access and occupy than the densely populated southern, Mesopotamian, and Syrian regions. We have also seen that during this period Transcaucasia settlers reached as far as the Carpathian Basin. It is therefore inconceivable that Transcaucasia peoples seeking fertile land and pastures would not have settled on the South Russian plains directly in front of their gates.

However, we also have more specific data. For example, the surprising archaeological discovery that the advanced Cuban civilization that suddenly appeared around 2300, without any precedents — which, as we have seen, can be traced back to direct and massive Mesopotamian influences — after barely 300 years of flourishing, around 2000-1900 BC, fell back into gray mediocrity just as suddenly as it had arisen. This is most evident in the migration of the population that brought and maintained the culture to the present-day

===== PAGE 46 =====

We can explain. The ore miners of this culture were the first to explore the Ural mines, even at the end of the 3rd millennium, and their northern connections have not been broken since then. We can therefore look for the 6186 wave of southern colonists in them at the beginning of the 2nd millennium.

Here we would like to return to the anthropological question of the demography of the pit grave culture of southern Russia and the Afanasyevsky culture that developed around 2200 in the Yenisei-Altai-Sayan region, which we already mentioned in the discussion of Asia. We have established that the population of both cultures was anthropologically identical, despite the 3000 km distance and the lack of communication: broad-faced, low-foreheaded and rather short-headed, "plump" Euroid (Jettmar et alii: im). In the next — Andronovo — culture, we have already identified the same type of people as the dominant ethnic group spread throughout the steppe zone, from Eastern Europe to Altai. If we now add to these anthropological data that a large number of Mesopotamian connections can be found in the finds of both the Afanasyev and the Pit Grave cultures, then we can have little doubt about the anthropological affiliation of the populations of the two cultures. (Childe: im 1949.; Hancar: im. 1956.) During the development of the Pit Grave and Afanasyev cultures, and later, when these two cultures in the Andronovo culture, which arose from the interactions of the Andronovo culture, we can again register the phenomenon that we have already established in the examination of the origin of the Kuban-Caucasian metalworking cultures: from the mutual interaction of two secondary — or this time tertiary — cultures that arose from Pre-Asian influences, a new one, in this case the Andronovo culture, emerged, covering vast geographical spaces. In this newly formed cultural circle, the southern immigrants who arrived in the yyyj-Volga region in the first half of the 2nd millennium also played a significant role, about whom it can be established with the highest degree of probability that they originated from Transcaucasia and the area of the Maykop culture. From these times onwards, the cultures of the steppe and the adjoining forest-steppe areas became increasingly homogenized, and this unity remained until the beginning of historical times.

As a final conclusion, we can state that even with our current, still rather incomplete knowledge, the decisive role of the ancient civilizations of Pre-Asia, more specifically the Sumero-Mesopotamian, as well as the Transcaucasian, in the early Metal Age development of the Volga-Ural area is becoming increasingly evident. Moreover, we can demonstrate not only direct and indirect cultural radiations, but also massive, mass waves of Transcaucasian-Pontic colonization, among which the presence of smaller or larger Mesopotamian-Sumerian ethnic groups can be assumed with great probability.

In the light of these archaeological and historical data, we dared to state above that from a Hungarian prehistory and ethnogenetic point of view, there is no 49

===== PAGE 47 =====

We can rest assured with Antal Bartha's opinion about these southern settlers. Here we are not talking about "significant influences" on the paleoecogenesis and culture of the Finno-Ugric and Ugric-Hungarian peoples - as Bartha believes "par excellence". Taking into account the Paleolithic-Mesolithic level -, There is no evidence of the paleoethnogenesis of the so-called Finno-Ugric peoples, but primarily of the Hungarian people. of education of the local indigenous population and the resulting, but archaeologically verifiable, very low population density, the colonists, who had developed agriculture, animal husbandry and bronze culture, and who arrived en masse and in several waves over centuries, did not "have a significant impact" on the ethnogenesis of the indigenous population, but directly defined and determined it. This is - we believe - a very important difference. It is also difficult to imagine that the language of the highly educated, large number of settlers would have only "influenced" the language or languages of the indigenous population, which was probably still quite undeveloped at that time. Under the given circumstances, we can only expect that the previously primitive and sparse population of the area in question simply merged with the newcomers, both ethnically and linguistically, in a very short time - if they did not move to more northern regions. In contrast, Bartha goes on to say that the "Finno-Ugric" — by which he means the indigenous population — almost completely assimilated the southern colonists within a short time. We cannot follow this line of thought, even with the greatest good will, because it simply contradicts all common sense. In our opinion, such illogical conclusions stem from a convulsive adherence to the rigid language family theory formulated in the 19th century.

Another question is the situation of the native population of the surrounding nearer and more distant areas, who were not settled directly by the southern colonists, but only came into more or less contact with them. Considering the question from this point of view, Bartha's opinion regarding significant ethnogenetic and cultural influences is more acceptable, although for the sake of completeness we must also note here that linguistic influences were in all probability decisive and were not limited to the one or two "Iranian" loanwords that Bartha attributes to these southern colonists. The Iranian origin of these words is, by the way, very doubtful, despite the fact that dozens of

Péter Hajdú and János Harmatta take the same position on this issue, both in the aforementioned Pre-Uralic linguistic relations; In the volume "Hungarian Prehistoric Studies". (Hajdú, P.: Harmatta,J.: Irániak és Finno-Ugric, irániak és magyarok.) Although this is not strictly related to our subject, we cannot ignore the generally accepted opinion that the population that migrated from the south to the Volga-Ural region in the first half of the 2nd millennium BC was the Ősiráni. As we have already mentioned, in this period-50)

===== PAGE 48 =====

In the 19th century, not even the faintest traces of the Iranians can be found in archaeology, historical sources, or even in contemporary linguistic monuments. The Iranian Persians are first mentioned in an Assyrian inscription around 840 BC. On the other hand, we hope that in our present paper we have managed to show something of the kind of peoples we can take into account in these migrations. It would be more useful and expedient if, in cases where tangible archaeological finds and contemporary written monuments are available, prehistory and linguistic research were to start from these, rather than from the perspective of 19th century Indo-Germanic studies, which is now - by the way - mostly not accepted even by Indo-Germanic historians and linguists themselves.

Finally, we briefly draw the conclusions that almost naturally arise from the described pre-Asian cultural influences and connections. Archaeological research over the last 80-100 years has revealed ancient civilizations in the territory of East Asia that the science of the 19th century had no idea about. At that time, it was believed — mainly based on the Bible and the old Indo-Germanic language monuments — that the cultural development of humanity began with the appearance of the Semitic and Indo-Germanic peoples. On these seemingly solid foundations, European science in the 19th century built its own pre-historical and linguistic perspective, the starting point and basic pillar of which was naturally primarily the Indo-Germanic cultural priority.

In the light of our current knowledge, however, this 19th century view has long since become outdated and untenable, or rather, according to the fundamental, theoretical requirements of scientific methodology, it should have become so if, for various reasons — mostly non-scientific — a significant proportion of the representatives of European science had not clung to this outdated worldview and had not rigidly rejected almost any revision, the demand for which has been increasingly imperative for decades. This incomprehensible ignorance is particularly striking in the field of linguistics. Comparative linguistics has so far simply refused to acknowledge that we now know of a mass of linguistic relics that are 2000-2500 years older than the ancient Greek, Sanskrit and Avestan texts, and that the vast majority of these linguistic relics cannot be included in the system of Indo-Germanic languages, and in fact, in most cases they are diametrically opposed to their linguistic spirit.

The natural consequence of this situation is the gaping void that appears in the scientific literature in the field of the evaluation of the discoveries of archaeology in the pre-Asian region. This situation is eloquently exemplified by our prestigious bibliography, from which it can be seen how many works we had to browse through, while we were able to compile this study of a few pages from scattered notes and small-print references. However, for the sake of objectivity, we do not want to ignore the sometimes, as it were, white ravens,

===== PAGE 49 =====

We will now mention only the names of Gordon Childe, Franz Hanéar and Fritz Schachermeyr, whose works listed in the bibliography clearly show that they recognized the key role of ancient Proto-Asian civilizations in the cultural development of the peoples of the Eurasian region. However, their voice remained a voice shouted in the wilderness, and they had few followers.

Academic historical science is still largely a supporter of the 19th century approach. Linguistics, however, has not yet reached the point where at least one expert linguist has attempted to take into account the more than half a million ancient linguistic monuments of Mesopotamia and other Near Eastern countries from a historical and linguistic development perspective. Philologists still use Sanskrit, Avesta and other Indo-Germanic linguistic monuments exclusively to conduct etymology, compile hypothetical ancient languages, and classify peoples into this or that language family assumed in the 19th century, as if the half a million linguistic monuments mentioned, originating from much earlier times, did not even exist. It is not our task now to get involved in endless linguistic problems; we would like to deal with these on another occasion. This time we only wanted to briefly point out that the current linguistic approach, based on fundamental, theoretical and methodological considerations, is, in our opinion, outdated and untenable in every respect in the light of our current knowledge of archaeology, prehistory, and most importantly, linguistic heritage.

The circumstances outlined above prompted us to attempt, as far as our possibilities allowed, to collect the cultural influences and emanations of the Middle East — Mesopotamia — and to include them in a unified, historical perspective. We could not, of course, provide a synthesis covering every detail, and we do not want to make such a claim — not even remotely. The enormous amount of data on the topic and the fact that the relevant knowledge encompasses numerous fields of expertise, already exclude the possibility of the complex of questions being summarized — even approximately — in a single study. Our work can therefore be considered only as an introductory study of a new research direction.

First of all, we tried to show that our archaeological and historical knowledge can be examined with a different perspective, and interpreted differently - and in most cases with a much higher degree of probability - than the way historical science usually explains these questions,

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===== PAGE 53 =====

Chapter 2
INDOGERMAN LANGUAGE, INDOGERMAN CAKE MAKER
Indo-Germanic studies — and with it modern scientific linguistics — can be dated back to 1816-1820. At that time, F. Bopp's epoch-making treatise "Über das Konjugationssystem der Sanskritsprache in Vergleichung mit jenem der griechischen, lateinischen, persischen und germanischen Sprachen" was published, in which he scientifically demonstrated the kinship of these languages. This language family, called Indo-Germanic or Indo-European, soon included the Celtic and Slavic languages, and then, by the end of the century, Albanian and Armenian. In other words, from the westernmost regions of Europe to the Ganges, apart from one or two smaller islands, an unbroken chain of peoples speaking languages belonging to the same language family was discovered.

This enormously far-reaching realization, which had not even been suspected shortly before, naturally left an indelible mark on the European linguistic and historical perspective of the 19th century. All the more so because, in the world of historical knowledge at that time, it seemed an indisputable fact that the oldest civilizations of Western Eurasia, apart from Semitic culture, were everywhere formed and maintained by peoples speaking Indo-Germanic languages. At the same time, the oldest written records known at that time were also in Indo-Germanic languages. Thus, detailed historical and comparative linguistic studies were launched with full force to gain the best possible understanding of the connections and connections of Indo-Germanic cultures and languages.

When comparing dictionary materials, it soon became apparent that certain serially repeated, seemingly regular differences could be observed between the phonetic forms of words with the same or similar meaning in certain related languages. One such frequently occurring sound correspondence was, for example, that in the Indic and Iranian languages, but also in most Slavic languages, the sounds S, SZ, Z were very often found in place of the sounds G, K, H of other Indo-Germanic languages. The distinction emerged from this observation, (Indic "satem"=száz, Lat. "centum"=ua.)

The development of the sound change rules, which decisively determined the further direction and method of the research, is primarily attributed to J. Grimm. The so-called Grimm's sound laws (1848) were adopted by all the authoritative Indo-Germanist linguists of the 19th century without exception. The thesis of "regular sound changes" postulated by Grimm soon became the basic pillar, the be-all and end-all, so to speak, of modern linguistics.

56
===== PAGE 54 =====

One of the most important tasks of linguistic research was to deduce the basic words of the presumed common Indo-Germanic proto-language using Grimm's sound laws. On the other hand, they tried to determine the original level of literacy of the proto-Indo-Germanic peoples, who were still thought to be linguistically undivided, from the combined common Indo-Germanic words.

However, if we look closely at these efforts, it soon becomes apparent that there is no unified scientific method. Individual researchers grouped languages according to their own preconceived ideas, according to which language or people they considered more important. Under such circumstances, of course, countless, often directly contradictory, findings were made, which were impossible to reconcile.

We can get a vivid picture of this methodological confusion from O. Schrader's book "Sprachvergleichung und Urgeschichte" (1883). In the first part, the author gives a detailed and objective historical summary of the development of Indo-Germanic studies up to that point, while not skimping on criticism.
We can see that even in the embryonic stage of the formulation of sound laws, they were used without further ado in the composition of common cognate words (A. Kuhu, 1845), and even Grimm tried to grasp the individual stages of the separation of the Indo-Germanic languages with their help (1848). The work of A. Pictet had a significant impact on further research, although he used a fundamentally incorrect method. He mostly worked with word compounds and word divisions. For example, ind "ka-bhara" = (what food) corresponds to the Old German "habaro" = (Hafer), or ind "ka-rasa" = (what food) to the Old German "hirs" z (Hirse). With such methods, we can prove anything we want, Schrader notes in his cited work. Pictet's book in question (Les origines Indoeuropéennes on les Aryas primitifs, essai de paléonto-logie linguistique, 1859-63) was immediately and fiercely criticized (A. Weber, A. Kuhn and others), but many people, despite this, have been using it as a starting point for their research ever since. Among other things, Pictet's influence is clearly the widespread opinion that the still undivided Indo-Germans knew metals, although — Schrader states — apart from copper, there is no trace of knowledge of metals in the Indo-Germanic period. In fact, they could not even process copper, but only made primitive jewelry from the copper nuggets they found here and there by simply drilling through them or at most by rough hammering. The existence of a blacksmith or metal casting craft cannot be demonstrated linguistically.

Schrader therefore clearly stated in 1883 that Pictet's methods were fundamentally flawed and therefore no conclusions could be drawn from them.

57
===== PAGE 55 =====

It will be instructive to briefly interrupt our introduction to the history of science and take a look at the etymological and historical methods with which K. Zeuss (1837), W. Tomaschek (1887), K. Müllenhoff (1892) and their followers established the Iranian origin of the Scythians.
Apart from one or two common words, we know only mythological and other personal names from the Scythian language. Therefore, the few common words recorded by Herodotus are of primary importance in such circumstances. According to Herodotus, the ethnonym "arimasp" means "one-eyed" in Scythian, because in Scythian "arima" = "one" and "spu" = "Amazons", and among the Scythians the name "oiropata", i.e. "man-killer", because in Scythian "oiro" = "man" and "eye". The especially because they are contemporaries. However, since in Indo-Iranian languages , pata" = "to kill". These data, as we can see, are completely unambiguous,

It is not possible to draw The word "man" has a suitable analogy, the Zend and Sanskrit "viras" = , man", and for the other three they resorted to the well-tried method of "most circumstantial source criticism" a parallel with the "oiro" = tabb reasoning, the aforementioned researchers misunderstood the and simply stated that Herodotus Scythians with "certainly", therefore their statement is worthless from this point of view. Let us now pay close attention to this word "certainly", because in the following we will repeatedly experience that it occupies a very prominent place in the scientific evidentiary apparatus of historical research with an Indo-Germanic perspective.
In other words, translated into plain language: since the Zeusses assumed from the outset that the Scythians were Indo-Iranian, the only evidence they had for this hypothesis, drawing from their own experience, was The contemporary wellhead was simply declared worthless. We can safely leave it to the reader to decide how much such a procedure has to do with scientific method.

And after they had thrown the eyewitness and earwitness who disturbed their circles into the trash with the aforementioned fabricated argument, they continued their reasoning as follows: since the Arimaspi were horse-keeping nomads (according to the heroic poem of Aristeas, the »Arimaspeia«), their name, based on the Zend and Old Persian languages, is either "airiyama-acp" = "obedient horseman", or "airima-acp" = "owner of the desert horses". And the name of the Amazons "oiropata", considering that one of the adjectives of the Sarmatians, who descended from them, is "women conquered", "women conquered" (Skylaxnāl and Skymnos), the Indo-Iranian "vira-pata" = The contortion and "men's mistresses" is the same as the expression, strain of these explanations seem at first glance to be 060016. They refer to Greek works of fiction and the fabulous stories about the Amazons, which they consider to be conclusive without further ado, in contrast to Herodotus' clear statement, which is based on their own information, but which they reject.
In his treatise "On the Scythians", Géza Nagy rightly pointed out at the beginning of our century that the argumentation was fundamentally flawed.

===== PAGE 56 =====

because even if Herodotus were really mistaken, the important thing here is not what the people's names in question actually meant, but that these two compound words were communicated to Herodotus by the Scythians in their own language. That is, even if they were 0700001020 Indo-Iranian words, they still say nothing about the "one-eyed" and Scythian Indo-Iranian origin, because the further etymologies of Zeuses are also of a similar nature. The known Scythian names of gods are assembled in the from Iranian or Indic roots, which can never same way as a mosaic: "man-killer" does not have the meaning in Indo-Iranian languages, but in Scythian they undoubtedly meant these concepts. be found in original Indo-Iranian names of gods -- as Antal Csengery has already established in his study "The Nationality of the Scythians" (Budapest Review, 1859). It is on weak foundations and it is time to thoroughly review it (Keleti Szemle 5/3, 1904). We would like to note that this has been relevant for over 70 years.

revision is still pending today. Moreover, Hungarian historical science today assumes Iranian "Prescythians" on the Eurasian steppe as early as the turn of the 3rd/2nd millennium BC. (I. the previous chapter)

We can see that Zeuss's etymologies are completely in line with Pictet's method, which was recognized as fundamentally flawed by sober research in Schrader's time - but we can add: only in principle. But they also bear a ghostly resemblance to those word cognates that modern linguistics calls mirages. Here too, we can note that, apparently, they only come from representatives of the official linguistic school. You see, the methods used by Pictet, Zeuss, Tomaschek, Müllenhoff are not accepted by linguistics

condemns it in principle, however, the Indo-Iranianity of the Scythians, based on the unmethodical etymologies of these researchers, seems to be a definitively settled question in Hungarian linguistics and history. Yet we now know that Herodotus did not publish fantasy words. Géza Nagy already mentions that in Sumerian, *bad* = to kill (cf. , *pata*). Since then, other Scythian words and names that appear in Herodotus have also appeared in Sumerian linguistic monuments, such as, for example, *TAB* = "flackern, flammen, leuchten, blitzen" (flash, burn, light up, flash — cf. "Tabiti", the name of the Scythian fire ur" = "Mann, Herr" (man, gentleman — cf. , oior") or , goddess).

It is also worth highlighting in connection with the Scythian question that international historical scholarship has repeatedly and emphatically addressed this issue in recent decades.

However, from Schrader's criticism described above, we can also draw other lessons in addition to considerations related to the Scythian question. It can be shown step by step that the Indo-Germanic cultural priority

59
===== PAGE 57 =====

The wild followers of the nineteenth-century prehistory approach, based on the foundations of the 19th century, haunt not only linguistics and history, but also the most recent publications of archaeology, and often lead researchers to hopelessly confusing or contradictory statements. In our time, no one claims that the "ancient Indo-Germans" knew metals. We have seen that even Schrader considered this view absurd.

However, we can read with astonishment in the latest edition of "Propyläen Kunstgeschichte" (Frühe Stufen der Kunst, 1974) that the "Nordic", i.e., South Scandinavian - North German Early Bronze Age began around 1800 BC.

The author of the article, H. Thrane, notes a few lines earlier that the Northern Bronze Age began much later than the Southern European one. However, V. Dumitrescu, in his article on the Balkan and Carpathian Basin Bronze Ages, dates the beginning of the Bronze Age in these southern areas to 1800 BC. The confusion is completed a few pages later by J. Filip's article, which describes the Central European Bronze Age, with special attention to the Aunjetitz culture. y also states that the European Bronze Age began with southern, Cretan and Balkan influences; the further north we go, the later. After this, he dates the beginning of the Aunjetitz Bronze Age to 1700 BC, while in the Carpathian Basin he only speaks of a Bronze Age from the 1500s BC. Then comes the crash: the Northern Bronze Age, he writes, began "with some delay" - according to Montelius, at the end of the 19th century BC.

This confusion means in Hungarian that, in the spirit of the 19th century Indo-Germanic cultural priority theory, the "Nordic" Bronze Age in North Germany and South Scandinavia, considered to be one of the nests of the ancient Indo-Germans, and the beginnings of the Aunjetitz Bronze Age, also considered to be Indo-Germanic, are still dated significantly earlier than the Bronze Ages of those regions where the presence of Indo-Germans is by no means probable in the times in question. This is done despite the fact that they clearly see, and even state, that the knowledge of bronze spread from south to north in Europe, with considerable temporal differences, to the disadvantage of the northern regions. It is also particularly characteristic that the source of J. Filip's 1947 article can be undoubtedly established in the work of G. Kossinna. Kossinna places the beginnings of the Silesian and Bohemian Bronze Age (Aunjetitz) before 2000 BC, and then states that the Aunjetitz Bronze Age culture The Indo-European Question answered archaeologically" c. értekezése — 1902-ből! E expanded southwards towards the Balkans at the beginning of the 2nd millennium BC and that the Carpathian Basin Bronze Age developed under the influence of northern bronze art. The Carpathian Basin bronze finds are merely "Ableger der nordischen Bronzekultur" (in Hungarian; the branch deposits of the northern bronze culture) — he writes. Even in 1974, the authors of the article "Propyläen Kunstgeschichte" were apparently unable to free themselves from these 1902 reflections.

60
===== PAGE 58 =====

We could continue to list similar cases almost endlessly, but we hope that this example alone is sufficient to show how enormously the outdated views of the 19th century hinder even today's scientific research. It is long overdue that we thoroughly review these mostly long-disproven propositions, theories, and hypotheses and, based on our current knowledge, separate the vast amount of chaff from the much smaller amount of wheat.

Let us now return to our overview of the history of science, continuing in Schrader's footsteps.

Although the works of M. Müller (1869), Th. Benfey (1868, 1869) and A. Fick (1869) are on a much more solid foundation than Pictet, they also show a complete lack of method in their examination of the ancient common Indo-Germanic vocabulary. They constantly draw unfounded conclusions from the similarities or differences of certain words.

All of them claim that the ancient Indo-Germans knew agriculture and had castles and cities. They are very uncertain about the knowledge of metals, and in many cases we find several contradictory views in different publications by the same author (e.g. M. Müller or Th. Benfey). Such errors are the consequences of the inadequacy of their methods.

It was generally believed that the ancient Indo-Germanic civilization was at a relatively high level. The existence of organized family, community, and even state relations, the knowledge of agriculture and animal husbandry, and the regular use of the most important metals were taken for granted so much that the ever-increasing finds of European archaeology, which did not fit into this picture, and even stood in blatant contradiction to it, were simply declared, without any closer examination, to be "pre-Indo-Germanic" ("vorindo-germanisch") and the appearance of higher civilization in Europe was explained by the immigration of the Indo-Germans.

In the field of linguistic research in the narrower sense, we cannot speak of uniform or well-founded methods. Some a priori, without any evidence, divided the Indo-Germanic people into eastern and western groups, others into northern and southern groups, but their conclusions were drawn either by taking one or the other hypothetical division as certain a priori, each according to his own ideas. No negative conclusions were ever drawn regarding the Indo-Germanic culture from the lack of common word forms (we will note Schrader's later opinion on this issue, which is in direct opposition to his current criticism!), but they did not care about the original meaning of the words either.

The obvious errors of conclusions drawn from arbitrary, unmethodical word comparisons were first pointed out by V. Hehn in his work, "Cultural plants and domestic animals in their transition from Asia to Greece and Italy, as well as to the rest of Europe" (1870). Point by point

61
===== PAGE 59 =====

showed that the Indo-Germanic peoples took over their entire culture from the ancient pre-Asian cultures and that before the advent of these cultural influences, nothing more than primitive barbarism could be found among them. The biggest mistake of the methods used so far is that they project the modern meanings of words back to ancient times. Any word that occurs in at least 2-3 Indo-Germanic languages is simply considered to have an ancient Indo-Germanic root.

Hehn's book was sharply criticized by many researchers, but it received little recognition. Everyone held (sic!) — writes Schrader — that Hehn's primitive picture of the ancient Indo-Germanic people did not reflect reality. However, it was not refuted on the merits (!), the criticisms consisted of general objections, mostly in such a tone that if we accepted this or that statement of Hehn, then "we could not even assume this or that about the ancient Indo-Germans." Schrader sharply condemnsthese meaningless criticisms. He finds it incomprehensible that the vast majority of scientists do not take Hehn's work into account and that research continues in the old rut. It is almost unbelievable — he writes — that even years after Hehn's work, publications such as A. Fick's "Die ehemalige Sprachinheit der Indogermanen Europas" (1873) could be published. book — which do not mention Hehn's findings at all. Moreover, in this work, Fick fiercely opposes all those who insist on the brilliance of Indo-Germanic antiquity at all costs against such efforts, that in Indo-Germanic antiquity only All this, Fick believes, can be discovered in the vocabulary of ancient times "with an unparalleled boldness of imagination," writes Schrader.

They want to dirty it up.

"we can experience elevated spirit and a sound, thoughtful mind."

However, he immediately softens his criticism and emphasizes that in this work Fick works with a much more well-founded method than before, because he only considers words as ancient that occur in at least one European and one Asian Indo-Germanic language.

In this way, he actually unspeakably proves Fick right against Hehn, because — after all — Fick works methodically, so his results are correct. He has forgotten that Hehn also criticized the previous research methods, that words that occur together in 2-3 Indo-Germanic languages do not prove the existence of the respective language.

the Indo-Germanic origin of words. Schrader, therefore, having previously approved Hehn's criticism in this regard, now takes a surprising turn, using the same rejected method.

He praises Fick precisely for this method. Later we will see that Schrader fully accepts Fick's position, but he does not state it as openly as Fick.

These neither cold nor warm criticisms are very typical of many representatives of Indo-Germanic linguistics and history. At first, they were mostly ob-62

===== PAGE 60 =====

jekiv, with clear reasoning they demonstrate the work in question, making it obvious 668611 and reject it. Then in their further discussions they come closer and closer to the author

The researcher in question will thus begin to "understand" that undoubtedly positive" triggering causes (meaning: the higher the demonstration of Proto-Indo-Germanic culture), The inevitable result everything remains as it was, and in many respects the critic himself will silently continue to apply the methods rejected on principle, if he can use them to dispel the myths of existence. Later, we will observe this phenomenon again in the case of Schrader,

An excellent example of these side-talks, hair-splitting and attempts to prove the ancient Indo-Germanic culture at all costs is Th. Benfey's article on the Indo-Germanic razor. In it, the author explains the absence of razors among the various northern Indo-Germanic peoples appearing in history by saying that the latter had undergone arduous and lengthy migrations under harsh climates. Schrader describes and quotes Benfey's stilted and stilted arguments over pages (53-57), but does "they lost" the razor, which the Middle Indo-Germanic peoples undoubtedly already knew, because the word is found in several Indo-Germanic languages.

not point out the tangible absurdity of these arguments in a single word. The consequence of this is that the idea of the ancient Indo-Germanic razor is elevated to the pole of scientific findings and, e.g. Kossinnánál but as we have seen, even much later = these "Nordic" bronze razors will flood the Carpathian Basin and even the Balkans, bringing an "Indo-Germanic" bronze culture to the "barbarian" peoples there,

The methods of research of Indo-Germanic were soon adopted by scholars of other languages. A. Ahlquist (Die Culturwörter der westninnischen Sprachen, 1875) established that the Linnaean words referring to a higher level of culture than the fishing and hunting way of life are, without exception, Germanic or Bult-Slavic loanwords. The same was the case with the Hungarians when they appeared in Europe. They also note that all words indicating a higher level of culture are borrowed words.

Ahlquist's method consisted in first selecting and discarding all those Finnish words that had cognates in Germanic and Baltic Slavic languages. Then, from the remainder, he took into account only those words that had cognates in the eastern Finno-Ugric languages (Vogul, Ostyak, Cheremis, Mordvin, etc.) in order to paint a picture of the Öslninn culture.

Now we only point out the one-sidedness of Ahlquist's modsverénck, with which the ptoni considered all Finnish words found in German, Slavic, Leuven, and Lithuanian as foreign words,

Indo-Germanic linguistic research also attempted to divide the period after the breakup of the common proto-language into linguistic stages, but a unified method was not possible.

63

===== PAGE 61 =====

They did not apply serletcknel either. The cgycs researchers started from different languages and compared the others to the language they arbitrarily took as a basis. The only success of the many grouping attempts was that it became clear: it is not 1061 simply to talk about a family tree in the case of the Indo-Germanic languages. The relationships are much more complicated. From certain points of view, sometimes Greek and Iranian, sometimes Iranian and Slavic, and then again Slavic and Germanic or Greek and Latin show a closer relationship. Taking other criteria as a basis, the situation may be reversed. For example, in terms of words related to agriculture, Greek and Latin are completely different from each other. As a result of this recognition, they were content with examining the ancient relations of certain languages that were definitely related (Germanic, Latin-Romance, Slavic, Indo-Iranian, etc.)." JEVocel researched Slavic primitive culture (1868). Since no bronze finds had been found in the region of the presumed Slavic ancestral homeland, north and northeast of the Carpathians, between the Oder and the Dnieper, Vocel concluded that while the peoples of the Carpathian Basin were still in the Bronze Age, the Slavs — under the influence of Greco-Pontic culture — were already living in the Iron Age.

G. Kreck (1874) also gives a rather high rating to the Proto-Slavic culture. In contrast to them, Hehn, in his cited work, showed that, for example, all Slavic words for stone construction were borrowed from foreign sources. He also proved with a whole series of reliable sources that even in much later historical times, the Slavs lived in huts made of wickerwork and migrated here and there. They had no permanent settlements.

However, as we have seen, Hehn's work was not taken into account, although it could not be refuted either. Thus, we cannot be surprised that, in accordance with the opinions of Ahlquist, as well as Vocel and Kreck, more than a thousand Hungarian words were declared to be of Slavic origin, including countless ones in which the historical sources clearly indicated that the borrowing took place in the opposite direction. Let us consider, for example, "szablya" and Schrader deals with the issue of loanwords summarily. He recognizes only a few dozen Semitic "bite" to our words. loanwords in the Indo-Germanic languages. However, a passing remark about the common Semitic-Indo-Germanic words is noteworthy. Fr. Müller - he writes - judged these words in one of his papers as follows: "These words give the impression that we would have in them the sounds of a culture of a much higher age than we could ascribe to the Semitic or Indo-Germanic." (In Hungarian: Rám all these words make Everyone is making a fuss about me the impression that-

* The relationships between the individual language groups were tried to be explained with the so-called "wave theory" (J. Schmidt, 1872), from which later, in the 1930s, the thesis of "areal languages" grew out. (Trubetzky)

64

===== PAGE 62 =====

if they echoed a much older culture than the antiquity we can attribute to Semitic or Indo-Germanic culture.) With our present-day knowledge, we need not prove how well Fr. Müller guessed - already at the earliest period of the discovery of the Sumerian language - the basic principle of the ancient connection of languages. problem. With the assumption of a unified Proto-Indo-Germanic language and people, the question of the ancestral homeland naturally arose. IGRhode (1820) placed the Indo-Germanic homeland in the Pamir region, AWWSchlegel between the Caspian Sea and the Central Asian mountains, JvKlapproth (1830) between the Himalayas and the Caucasus, and FAPott (1833, 1840) in the area of the Oxus and the Jaxartes.

The assumption of a Central Asian homeland is also related to the fact that the folk names "yüe-ü", "yüe-csi" or "yeta", "sze" or "SZai" and "szü", "wuszun" occurring in Chinese sources were interpreted as "Usun" or "Getae" Asian Indo-Germanic, and some even did not hesitate (e.g. Klapproth and Ritter) to interpret the "yeta" as "the Goths" - writes Schrader. J. Grimm also considered the Getae to be Goths and thus gave fuel to such ideas with his authority. In connection with this unambiguous statement of Schrader, we must again point out emphatically: however unfounded these assumptions are and however much one of the most renowned Indo-Germanists of the turn of the century has been scorning such fantastic opinions for almost a hundred years, the 20th century descendants of these Indo-Germanic "Yue-chis", the "Tochärs", are still trotting merrily across the Eurasian steppe today, together with their other Indo-Iranian horse-nomadic relatives, the "Sai"-Sakas, "Usun"-Azos, Scythians, Massagetae, etc. — in historiography with an Indo-Germanic perspective. Despite the fact that, for example, all known Chinese sources call the "yüe-chi" a people of Tibetan race and language (AvGutschmid: Geschichte Irans und seiner Nachbarländer von Alexander bis zum Untergang der Arsaciden; O.Franke: Beiträge aus chinesi-schen Quellen zur Kenntniss der Türkvölker und Skythen Zentralasiens; JvMde Groot: Chinesische Urkunden zur Geschichte Asiens 1.; W.Eberhard: Kultur und Siedlung der Randvölker Chinas; i.e.: Die Kultur der alten zentral-und westasiatischen Völker nach chinesische Quellen.)

Th. Mommsen also took a position on the question of the 6shaza (Römische Geschichte, 1854). He considered Mesopotamia to be the Ööshazajä of the Indo-Germanic peoples. This statement can be found unchanged in later editions of his work, even in the 7th edition published in 1881 — writes Schrader in a hidden footnote. And we can add from our own experience: even in the 1932 edition, and without any publisher's comments. This is again a textbook example of the scientific methods of Indo-Germanic historiography. Not only in 1881, but also in 1854, it was clearly stated that

===== PAGE 63 =====

The excavations in Mesopotamia have clearly shown that this region was inhabited by Semites and other ancient, but certainly not Indo-Germanic, peoples from the 4th millennium BC onwards. Not even the faintest traces of Indo-Germans have been found. Mommsen was apparently not interested in all this; his hypothesis was Mesopotamia and no archaeological data could have swayed him from this. If the archaeological facts said otherwise, then it is worth revealing such backstage secrets, because only in this way can we learn the true nature of the "the worse it was for the facts." spirit that prevails in Indo-Germanic studies. But we can also learn from this example what a huge role the principle of authority had - and still has - in historical and linguistic sciences: even Schrader did not dare to openly state that in 1881 Mommsen's opinion had long been anachronistic.

In the second part of his book, Schrader presents his own views on the issues of the Indo-Germanic proto-language and cultural status. From the very first lines, we can see how much the author is addicted to the views and unfounded methods that he apparently objectively criticized in the first part. He defines his own working method as follows (page 155): Indo-Germanic language comparison is based on two fundamental propositions: 1.) The relationship of the Indo-Germanic languages can only be explained by assuming a common ancestral language. 2.) The assumption of this Indo-Germanic proto-language necessarily proves (sic!) the existence of an Indo-Germanic ancestral people. In our humble opinion, no assumption proves anything. Not even in general, let alone necessarily. The relationship of the Indo-Germanic languages is also not a can only be explained by a common ancestral language. Linguistic affinities can be imagined in other ways. The two "fundamental" "fundamental" assumption. Both from theoretical considerations, but also logically. As the beginning, so is the continuation. When studying the second part, it becomes increasingly clear that Schrader himself works with similar phrases! as those, for example, in the case of Fick, he explicitly rejected in a mocking tone; "The enormous spread of the Indo-Germanic languages - despite the great amount of mixing - can only be explained by higher culture" - he writes in the introductory lines. Then he devotes an entire chapter to the scourging of some "undisciplined" researchers who in practice do not always adhere to the "uniform agreement of principles" that "from the lack of common words one cannot draw negative conclusions regarding the culture of the ancient Indo-Germanic people", and thus attribute a much lower level of educationto the ancient Indo-Germanics than they actually are.

66

===== PAGE 64 =====

They stood for "certainly". This cut is for Hchnnck. Let us just turn back and read again what Schrader wrote about Hchnnck's work. So the naked reality once again emerges from behind the scientific mask: if some data is advantageous from an Indo-Germanic point of view, all the better. If, however, it is disadvantageous, it should not be taken into account. This is the true meaning of the above "uniform agreement" disguised as a "principle requirement" but in reality can only be called a common deception. What can we say about this procedure? This is the behavior of selfish schoolchildren who always want to win the game.

The arguments in which Schrader — at the end of his book, in the chapter "Sprach" — describes the superiority of the Indo-Germanic languages over the Ural-Altaic languages are also on a similar level. Among other things, grammatical genders are of course among the infallible signs of superiority. As he writes, only the Semitic languages can be compared with the Indo-Germanic languages in this respect, but even these do not reach the "height" of distinguishing the three genders: they stop at two. Finally, he allows that in their most ancient state the Indo-Germanic languages could have been as low as the Ural-Altaic languages today. Let it be according to his word. It is natural that everyone considers their own mother tongue to be the most developed and logical, since it is the language in which they can express themselves most easily and with the most nuance. However, we do not understand in any way what such subjective beliefs have to do with a scientific work. For any sober-minded person, these extracted data already make it obvious that much of 19th-century Indo-Germanic studies was not so much about scientific linguistics as it was about the stubborn defense of the theory of Indo-Germanic cultural priority. The older the civilizations that archaeology uncovered in East Asia and the more certain it became that the Indo-Germanics had nothing to do with these ancient civilizations, the louder and more hysterically the champions of Indo-Germanic cultural supremacy asserted their supposed truth. _

This atmosphere of panic was largely the result of Indo-Germanic studies becoming increasingly self-contained towards the end of the 19th century, as if the elite He assumed a posture of isolation and vehemently protested against any attempt to examine possible more distant linguistic relationships within the Indo-Germanic language family. Schrader also states in the chapter "Sprach" of his book that the Indo-Germanic languages are not related to either the Semitic or the Ural-Altaic languages.

The signs indicate that the clearer minds saw it already at the end of the 19th century: working with the laws of sound and regular sound changes

67

===== PAGE 65 =====

Language comparative methods can only be applied satisfactorily as long as they remain within the framework of the Indo-Germanic languages, which are surrounded by libraries of specialized literature, and are thoroughly developed. In the case of more extensive language comparative studies, the limitations of these methods immediately became apparent. This was first demonstrated when

was noticed when hundreds of researchers (Delitzsch, Möller and others) pointed out Indo-Germanic parallels in the Semitic languages. These similarities could not be explained by any sound law. There was no other way out than to turn inward, the result of depressing experiences, and to isolate the Indo-Germanic languages - references to "coincidental" coincidences or other similar superficial objections. Such from all other language families.

At the same time, they also built up their defensive positions against the expected competition from the ever-increasing and increasingly ancient Proto-Asian linguistic monuments. One very effective way of defense was the absolutization of the sound laws, which was represented by the neo-grammatical school (Junggrammatiker) that emerged at the end of the 19th century. This trend proclaimed that there were no exceptions to the sound laws and that only completely regular sound correspondences counted. Their view increasingly gained ground in the authoritative circles of Indo-Germanic studies.

In this initiative we can clearly see the influence of the Sumerian language, which was slowly becoming known. More and more Sumerian words came to light that were eerily similar to many basic words that had previously been considered to be Proto-Indo-Germanic. Pl. "ag"=, tun, machen" (do, make), , agar"—, Feld, Flur, Steppe" (arable land, field, steppe), etc. They tried to exclude these Sumerian words from linguistic historical studies by strictly applying the phonetic laws.

We have formed this opinion after carefully considering the components and direction of 20th century Indo-Germanic linguistic research. To this day, 20th century Indo-Germanic studies do not take Sumerian or other ancient Proto-Asian languages into account in their linguistic historical studies. They are meticulous in their efforts to avoid even mentioning the name of the Sumerians. We will experience this incomprehensible ignorance at every turn in the presentation of the history of 20th century research. Even the latest Indo-Germanic etymological dictionaries are full of words with alleged Indo-Germanic origins, which at first glance can be said to be derived from the Sumerian language.

Before concluding our presentation of 19th-century Indo-Germanic studies, we would like to introduce two — generally applied — very effective psychological tricks, with the help of which unfounded assumptions, improbable conclusions, and unproven statements can be presented in such a way that the very shaky foundations mostly remain unnoticed.

68
===== PAGE 66 =====

One such method is also used by Schrader in his work, so we can demonstrate this with specific examples. We have already mentioned that Schrader repeatedly emphasized: those idyllic views of the high level of education, orderly and even law-governed community life of the ancient Indo-Germans, which are almost ineradicably widespread among researchers and in the public consciousness, cannot be maintained any longer. They are based solely on linguistic data, and the evidentiary value of these linguistic data is very doubtful (pp. 191 and 410-411). However, on the next page (412) we can read something completely different: "As the community for the ancient times had existed, so may (emphasis added) early municipal courts such as Tacitus with judicial competence for the welfare of the community Incidentally, since we are on a certain degree political have existed." (In Hungarian: Since we have acknowledged a certain degree of political community in ancient times, it is possible that in early times there may have been community courts similar to Tacitus' "concilium", which had jurisdiction in cases of crimes affecting the good of the community.) So, after explaining that we have no evidence whatsoever of the communal life of the ancient Indo-Germans, let alone laws, he now simply states that such "may have existed" (, mögen...bestanden haben). This is based on a piece of information from Tacitus, who wrote in the 1st century AD, which he unhesitatingly projects back to Indo-Germanic antiquity. The most important thing, however, is that he makes this statement in the last, summary part of his book, as a carefully considered conclusion. Such final conclusions are naturally much more memorable in the reader's memory, and they significantly strengthen the formation of his opinion. influence, as the data scattered here and there previously reported.

Let us not think that this phenomenon is accidental: there is a method behind it. Let us recall the very positive evaluation of Hehn's book in the first part, on the history of research, and then the condemnation of his method, based on a fundamentally unscientific, even almost ridiculously primitive "principle requirement". This method has several advantages. On the one hand, it creates a feeling of absolute objectivity in the reader, on the other hand, it influences his opinion in the way outlined above, and thirdly, it also blunts the edge of any possible criticism, since it can always be referred to as the fact that the author only drew his conclusions conditionally (" , mögen...bestanden haben").

However, there are quite a few who use another method. This is not as balanced and finely thought out as the previous one. As a result, we find it mainly in authors of lesser capacity. We could call it the overwhelming method 15 because its main strength lies in the fact that it discusses in full the various chains of hypotheses that exalt the Indo-Germans, while

69
===== PAGE 67 =====

with countless literary references. In the meantime, he exaggerates even the most insignificant, but positively interpretable, data, while tactfully skimming over sources unfavorable to the Indo-Germans - perhaps even just mentioning them. Thus, the real picture of the given historical situation is naturally miserably distorted, but - and this is the essence of the method - always in favor of the Indo-Germans without exception.

The main "advantage" of this method is that it destroys in many people the sense of proportion, which is indispensable for the correct evaluation of historical, linguistic sources and data, and for the sober consideration of their importance. After studying a sufficient number of publications written in this way, a significant part of researchers - especially young people, the scientific generation - increasingly lose their natural sense of distinguishing the important from the unimportant.

The excavations in Mesopotamia, which began in the second half of the 19th century, fundamentally shook the historical and cultural worldview that had developed at the beginning of the century. By the beginning of the 20th century, tens of thousands of cuneiform language relics were available for research, some of them dating back 1000-2000 years to the time when the oldest Indo-Germanic texts were written. It also turned out that the oldest language was the agglutinative Sumerian, which, using the methods of modern linguistics, could not be related to either the Semitic or the Indo-Germanic languages.

These archaeological results created a fundamentally new situation in linguistics. The opportunity arose to compare the previous results of Indo-Germanic linguistics with these new, much older linguistic monuments and to check their accuracy in the light of these, and to correct any errors or mistakes. However, this methodologically indispensable work process is still pending today. Not only in Indo-Germanic studies, but also in the research of other language families.

The Sumerian language was initially classified by many renowned scholars as part of the Ural-Altaic language family, but by the beginning of our century the general opinion had developed that Sumerian was a completely unrelated language, not related to any modern language or language family. This view still prevails today. We cannot now analyze the gradual development and spread of this scientific position, nor the deeper, real reasons for which, it seems, had primarily and most importantly non-scientific roots. Based only on the concrete archaeological, cultural-historical and ethnogenetic data presented in the first chapter, we can conclude that the linguistics idea that the Sumerian language was an island language seems incomprehensible in light of our current knowledge.

It is inconceivable that the language of the first true culture on Earth, which existed throughout East Asia for a good 1500 years, 70
===== PAGE 68 =====

dominated and whose bearers, the Sumerians, maintained regular trade and cultural relations with a considerable part of the western half of Eurasia, would have remained without any impact on the surrounding peoples, who were then still very primitive, at the Paleolithic or at most Neolithic level of culture. Let us think of the English world empire, which existed for barely 200 years, to what an enormous extent it influenced the languages of the colonial peoples. We will now only refer to the linguistic phenomenon of pidginization, which can be found in a considerable part of the former European colonies6.*

Well, the language of the Sumerian cultural and commercial empire influenced, not for 200, but for 1,500 years, in exactly the same way, the languages of the primitive indigenous peoples of the vast region stretching from the Indus to the Danube and from the Red Sea to Turkmenistan, who, moreover, were much less developed in those ancient times than the people living in the European colonies. in the languages of the 18th-19th centuries.

But it is not necessary to prove our point with theoretical arguments. It is enough to consult an Indo-Germanic or Finno-Ugric etymological dictionary, where — as already mentioned — we can find hundreds of clearly recognizable Sumerian words recorded as Indo-Germanic and Finno-Ugric roots.

Let us take an arbitrary compilation as an example. In his article "Pre-Uralic linguistic relations" (Hungarian archaic studies, 1977), Péter Hajdú lists seven ancient, non-borrowed words (page 158) Five of these, without any special examination, can be determined at first glance to be of undoubted Sumerian origin:

Indo-European urálí-finn Hungarian	*NOMN-	*NIME	Sumerian
name clean enclose,	NEV		
stable, cage live make, do bring			
*GREETING- *GREETING MOSQUE *COURT-	"DH3K	*CITY	HAZ
*TEKE	TESZ 'Dodw,-+DO	*TOKE *	TO (Finnish: TUO)

Pidginization occurs in almost every case when a developed, dominant cultural language comes into closer and longer-lasting contact with the languages of undeveloped, backward societies. In such cases, the natives either adopt the cultural language in question, for better or worse, more or less distorted, or they massively incorporate its vocabulary, grammatical and language structure elements into their own language. The mixed languages thus formed, forms of the original cultural languages often distorted beyond recognition, are called pidgin or creole languages in linguistics.

71
And the 111

===== PAGE 69 =====

See, maybe Sumerian isn't as isolated a language as they claim. Indo-Germanic linguistics at the beginning of this century is characterized by the recognition by some prominent researchers that the methods used up to that time had completely isolated the Indo-Germanic language family from other languages and that broader linguistic research had thus reached a dead end. They therefore tried to break out of the vicious circle that 19th-century linguistics had drawn around the Indo-Germanic language family.

H. Möller, in his work "Etymological Dictionary of Indo-European and Semitic Languages" (1907), describes this state as follows: Indo-European linguistic research has closed itself off within its own walls and has reached a complete standstill. "Sie muss wenigstens zunächst an einer Stelle die Wand zu sprengen suchen." (English: "We must first try to break the wall in at least one place.") Bär Möller's dictionary contained such a huge mass of Indo-Germanic-Semitic synonyms that it constitutes a considerable part of the vocabulary of the most important European and ancient Near Eastern languages.

embraced and opened up previously unimaginable perspectives for exploring the pre-Asian-European linguistic connections of prehistoric times. The relevant professional circles, far from exploiting the opportunities he showed, did not even understand the significance of Möller's pioneering work.

A. Trombetti was the first to attempt a broad comparison of different language families using modern linguistic methods. However, being a pioneer in this field, he overshot the mark. In his works we find numerous incorrect, unfounded word derivations. On the other hand, he could not get rid of the usual dogmas and tried to establish various sound laws between the individual language families. Thus, his work could not achieve lasting results. However, his undoubted merit is that he emphatically pointed out the interrelationships between the language families and thereby set the direction for future research.

In 1912, Fr. von den Velden's work "On the Origin and Origin of Indogermanic Languages and Anarische Language Remnants in Europe" was published. He examined the ancient, non-Indo-Germanic words of the Alpine dialects and came to the conclusion that the Indo-Germanic languages developed relatively late and, before their separation, belonged to a larger, ancient linguistic community that formed the basis of other language families besides Indo-Germanic. The Indo-Germanic languages took over many things from this earlier linguistic state: word roots, basic words, word-forming elements. But even after their development, they remained in contact with neighboring language families and later borrowed some things from them. He states that Indo-Germanic linguistics should also take neighboring language families into account in its etymological studies.

72
===== PAGE 70 =====

because it is often much easier to find explanations of many Indo-Germanic words in these than in the Indo-Germanic languages themselves.

But Velden saw even more. The ethnic basis of the Indo-Germanic language-speaking peoples is the same mixed race of people - the so-called Caucasian-Europid element - from which the Semites, the Caucasian and the Ural-Altai peoples also originate - he writes. He argues that before the separation of the present-day language families, the ancestors of these language families belonged to the same ancestral language community. This is proven not only by the numerous linguistic parallels, but also by the far-reaching similarities in myths, tales, legends and religious images that extend almost throughout Eurasia.

Velden already suspected the close relationship of the West Eurasian language families in 1912. It did indeed exist during the Pre-Asian Neolithic, "ancient linguistic state" which he is talking about, based on our current knowledge, is Chalcolithic and Early Bronze Ages. The most important dialect of this ancient language was Sumerian. This is why we consider it indispensable to include the Sumerian language in any broader comparative or evolutionary study of languages that goes beyond the boundaries of language families. We will discuss the massive effects of the Sumerian language on the Indo-Germanic, Semitic and Ural-Altai languages in detail later.

Velden thus pointed to the core of unresolved linguistic questions, so to speak. He demanded an explanation of the obscure words of origin that are found everywhere in Indo-Germanic languages and that cannot be interpreted on an Indo-Germanic basis. This question has remained on the agenda ever since, although Velden's work was not taken into account at the time.

However, the studies of H. Schuchardt, Paasonen, Setälä, Thomsen and Jacobsen have in the meantime made it clear that closer connections can be demonstrated between the Indo-Germanic, Hamito-Semitic, South Caucasian and Finno-Ugric language families, in the knowledge of which they could no longer speak of the complete separation of the Indo-Germanic languages.

Questions similar to Velden's doubts also occupied French linguistics. In his book "Linguistique historique et linguistique générale" (1921), A. Meillet resignedly states that although the Indo-Germanic languages have been researched with the most meticulous care, Indo-Germanic equivalents have been found for only slightly more than half of the vocabulary of the Greek language, and the remaining Greek words cannot be interpreted from the Indo-Germanic languages. In the same work — probably as a result of this negative experience — he allows the possibility that there may have been a genetic relationship between the ancestral languages of the West Eurasian language families. He also sees and appreciates the eminent importance of this question. However, in his opinion, the time is not yet ripe for its solution, because we do not have such certain criteria with which the question could be decided. He does not consider the numerous vocabulary similarities that have been demonstrated to be sufficient.

73
===== PAGE 71 =====

to prove linguistic kinship, grammatical correspondences are also indispensable for this - he writes. However, he also sees that requiring grammatical parallels can lead to a dead end, and even in the case of comparisons of monosyllabic languages, for example, it is impossible to fulfill it outright. Therefore, he demands the development of criteria that would enable the reliable separation of similar words of two language families into loanwords and words that are closely related to each other.

Meillet's demand for a reliable method in the question of loanwords was long overdue. In this area, there was just as much confusion in Indo-Germanic linguistics as in other methodological issues. We have already seen that this methodological confusion extended to almost all areas of 19th-century Indo-Germanic studies. We cannot discover any uniform guideline in the assessment of loanwords either. Everyone classified loanwords according to their own ideas or theories, with a preference and first and foremost, of course, for those of Indo-Germanic origin. The only methodological rule that can be called a general aspect was the one we could already register in connection with the work cited by Schrader: the ancient Indo-Germanic people had such a high level of education that they did not need loanwords. Meillet, for example, in his work mentioned above, he claims that the number of Semitic loanwords in Greek does not reach a dozen. On the other hand, H. Lewy lists hundreds of Greek words borrowed from Semitic languages in his work "Die semitischen Fremdwörter im griechischen", but let us also think of Möller's work mentioned above.

Scheffelowitz, on the other hand ("Arisches im Alten Testament") claims that a great many Hebrew words are borrowed from Indo-Germanic languages, namely Greek and Persian.

Researchers representing these diametrically opposed opinions very often claim reverse borrowing of the same words. There is therefore no trace of method here, only individual opinion confronting individual opinion. This can hardly solve the often very complicated question of the origin of loanwords. It is not possible to decide from which language family vocabulary a commonly occurring word originates by majority vote or on the basis of unfounded cultural priority hypotheses. Here, the only thing that helps is to carefully examine all the circumstances that can be taken into account, such as the first occurrence in linguistic monuments, historical and cultural history data, the branches of the word in question, its word family, its basic meaning in the languages in question, etc. |

It would be indispensable, above all, to clarify the concept of the loanword itself in principle. Indo-Germanic linguistics has not completed this task satisfactorily either.

They only got so far as to establish, on the one hand, that there are foreign words that have not yet been integrated into the sound system of the receiving language and loanwords that have not yet been integrated into the sound system of the receiving language.

===== PAGE 72 =====

this integration has already taken place. On the other hand, they also agreed that if a loanword was introduced before the sound shifts in the receiving language, the person language, then its being a loanword cannot be recognized afterwards 101, because this word also participated in the characteristic sound changes.

We cannot be satisfied with these two statements, because they only touch the surface of the question, although we can also find much more essential, deeper differences between loanwords and borrowed words. The most important such fundamental difference lies in whether a new basic concept, a new word stem, has been introduced into the receiving language with a loanword, or whether a corresponding root with parallel phonetic and meaning can be detected in this word independently of the borrowed word. That is, whether we are talking about a loanword stem or just a borrowed word form — the adoption of a new phonetic variation of the already present basic theme. Such loanwords occur much more often than we might think.

But let's return to our review of research history.

The issue of Semitic-Indo-Germanic word cognates was resolved by compromise. In 1929, in the re-issued Indogermanischen Altertumskunde (O. Schrader), edited by A. Nehring, in the light of the new Real-lexicon of research of that time, he evaluates the problem as follows: "Perhaps it is a borrowing of Indogermans and Semites from a third language or language group." (In Hungarian: , we must think about it.)

Then, from the mass of identical or largely similar Semitic-Indo-Germanic words, the question is raised by presenting a single example, as if it were only one or presents the correspondences of the word "bull", implicitly so two such parallels:

German | Greek Scandinavian Old Slavonic Lithuanian (Edda)
STIER TAUR-os TAUR-us THIOR-r TUR-u TAUR-as (bull) (bull) (bull) (buffalo) (buffalo)
Old Prussian Assyrian Hebrew | Ethiopian Phoenician Aramaic Arabic
TAUR-is SUR-u SOR SOR THOR TOR TAUR (buffalo) (bull) (bull) | (bull) (bull) (bull)
for "bull"? The Semites and Indo-Germans borrowed it from a "third" language. What is their word

75
===== PAGE 73 =====

Sumerian: SUR or SUR = "Stier, stechen, druchdringen" (bull, pierce, ätðof) TAR = "Spitze, Stachel, stechen" (peak, peak, thorn, spike, cf. also dagger and crest).

This fact was available Reallexikon" carefully omits it. Rather, it speaks of some unknown third language. It is worth noting that in 1929, a detailed Sumerian dictionary had already been available for quite some time.

to the research in dictionaries (Brünnow, Meissner), and even the corresponding volume of A. Deime's monumental "Sumerisches Lexikon" — from which we took the above data — was published as early as 1928 (Vollständige Ideogramm-Sammlung, Band I).

It is not necessary to elaborate on the scientific stance of the linguistic school that does not take newly discovered proto-language words into account if they do not fit into its own worldview of language development and cultural history, which it had formed a hundred years earlier. More than 50 years have passed since 1929, but this has been systematically and consistently adhered to ever since: the Sumerian language is simply ignored. method"

This ignorance — along with its inevitable negative consequences — can be experienced at every turn in the latest representative publication of Hungarian linguistics, "The Historical-Etymological Dictionary of the Hungarian Language", which follows the linguistic research methods of Indo-Germanic studies in almost every respect. We present a typical example:

"tunic": of Latin origin... The Latin word with the Greek "chiton" (ua) form together go back to the same ultimate source; cf. Phoenician "ktn", "garment made of linen".

Well, this Phoenician "ktn" is not even the "final source" within the Semitic languages, because the original Semitic form is the Akkadian-Assyrian "KITU" = Flachs, Linnengewand, which in turn is a takeover of the Sumerian word GAD"; "GADA", "GADU" with the same meaning.

The form "ktn" is the result of a long development and stem expansion in later Semitic languages. Why is the Phoenician needed, despite the to stop at the word "ktn" and declare that this is the "ultimate source", fact that the corresponding Sumerian word has been known for decades? Perhaps because this word is strikingly reminiscent of the Hungarian word "gatyá" in both its sound and meaning, which was declared to be of Slavic origin in the 19th century? We cannot know. However, it is certain that by omitting the corresponding Sumerian word, the real proto-linguistic connections of "tunika" are not revealed. For the time being, we cannot follow this extremely interesting proto-linguistic trace any further. For now, we will only note that "GAD"

The wedge sign has both the readings "KAD" and "KAD," 65 this "KAD," = "binden", i.e. in Hungarian, "KÖT"; and in the compound "AL-KAD 0" it means "Reuse, eine Art Netz" (varsa, a kind of net, aza7, in Hungarian "KATA", the bag of the planing net. We will deal with these questions in detail in the appropriate place.

76

===== PAGE 74 =====

The further development of Indo-Germanic studies in the 20th century was determined by two shocks - of paramount importance in their consequences. One was linguistic: the classification of the Hittite language among the Indo-Germanic languages (B. Hrozny). The anomalies, phonetic and grammatical peculiarities of this language overturned the previous lexical and grammatical concepts and formulations related to the proto-language, since they did not want to fit into the straitjacket of the deduced proto-language word forms and grammatical formulas. From this point on, the division of Indo-Germanic linguistics into two camps can be observed. One school denies the inference of the proto-language, the uniform origin of the Indo-Germanic languages, and understands by the term "Indo-Germanic" exclusively a certain linguistic state, which, however, says nothing about the circumstances of the development of these languages, neither in terms of people, race, nor history. In parallel with this, an increasingly completely ahistorical trend emerged, which absolutized the evidentiary power of the laws of sound (Reallexikon der Assyriologie und vorderasiatischen Archäologie, vol. V, under the heading "Indo-germanen").

The second shock came to Indo-Germanic studies in the historical-political field; after witnessing the rapid spread of the fantastic Indo-Germanic race theory of German National Socialism, many researchers suddenly realized where the uninhibited heroization of the Indo-Germans for a hundred years had led them. The race theory and the theory of the Indo-Germanic "Herrenrasse" were by no means invented by Hitler, but were the direct, logical consequences of the Indo-Germanic-inspired approach to history writing in the 19th and early 20th centuries. This historiography, from elementary schools to universities, but also outside of school: in newspapers, magazines, books, plays, musical works, etc., taught the Germans that in the western half of Eurasia, almost since the beginning of the world, all cultural achievements were the work of the various migrating Indo-Germanic, ruling classes.

("Herrenschicht"), and that the Indo-Germans were the teachers of all other peoples. In the interests of this thesis, they did not shy away from the most absurd distortions and conscious misinterpretations. We consider it sufficient to familiarize ourselves with two of the countless similar "statements" in more detail.

One is the question of the alleged Aryan ruling class of the Hurrian state called Mitanni, which emerged in the middle of the 2nd millennium BC in northern Mesopotamia, Syria and the adjoining north-northeastern areas, roughly between the Mediterranean Sea and the Zagros Mountains. This thesis is based on the knowledge of a treaty text — dating from around 1350 BC — at the end of which the king of Mitanni swears by the gods of his country to keep the agreement. Among the numerous names of gods listed for testimony, , in a less prominent place, near the end of the Mitanni-Hurrian gods-77

===== PAGE 75 =====

gods - as is known - also "in a less prominent place, near the end of the Mitanni-Hurrian list of gods" - writes the aforementioned "Reallexikon der Assyriologie" (under the heading "Indra") - the names of the gods Indra, Mitra, Varuna and Nasatya also occur, which appear in the Vedas. The Indo-Germanists did not need more than this: they immediately established that the Hurrians of Mitanni had an Aryan ruling class, since these "Old Indic gods" 15 were worshipped in Mitanni. The fact that the four god names in question are found almost at the very end of the list of dozens of gods did not bother them. But neither did they have to prove this", which, however, were first described only a few hundred years after the birth of Christ. This Indo-Germanic linguistics, from the archaic nature of the language of the Vedas, had long established that these "undoubtedly" belong to the 1st millennium BC. They reflect the linguistic state between 2000-1500 BC. The Mitanni Aryan god The antiquity of the "ancient Indus gods" could only be established through the Vedas. names came in very handy for this dating, which was obtained exclusively by linguistic methods, because now they could also be used to support the antiquity of "ancillary circumstance" could not be considered, simply because the Vedas. Thus, the cat bit its own tail nicely: the shaky linguistic dating of the antiquity of the Vedas was supported in Mitanni, and the "Aryan origin" of the questionable Mitanni gods was proven by the antiquity of the Vedas. The discovery of the ancient Indus civilization in the 1920s and 1930s then acted like a thorough cold shower. It was proven that the Aryans had nothing to do with this culture and that the Aryan immigration, which had previously also been placed around 2000-1800 BC in close connection with the age of the Vedas, could have been no later than 1000 BC. but it could be done even later than that. "ancient Aryan god names"

At the same time, as if a benevolent fairy of eternal youth had waved her magic wand at the Vedas, they too were rejuvenated by a thousand years overnight. The aforementioned "archaic linguistic character" no longer pointed to between 2000-1500 BC, but rather between 1000 and 500 BC, and undoubtedly began to reflect the linguistic conditions of that time.* It should be noted, however, that according to several renowned researchers, even this dating is too high, because there is no trace of the migration or conquest of the Aryans in the Vedas; they could only have been created a considerable time after their arrival in India. (L. Petech: Indien. — Propyläen Weltgeschichte, vol. II, 1976.). However, the date around 1000 BC was generally accepted, thanks to Indra and the other Mitanni-Aryan gods, who could be referred to even after the excavation of Mohenjo-Daro. That these gods were no longer "ancient Aryan gods" under the changed circumstances, i.e.

* This 1,000-year rejuvenation that occurred overnight beautifully illustrates the reliability of linguistic age determinations based on phonological laws.

78

===== PAGE 76 =====

they could not have originated from the Rigveda, which according to the new data was written no later than 1000 BC, and they were rather tactfully silent about it. Thus, the "Aryan ruling class of Mitanni" still appears as a fact in several scientific works today, of course, almost exclusively in the publications of the ahistorical school that was already discussed above.

A considerable part of the representatives of Hungarian linguistics and history - it seems - have joined this ahistorical trend, which is represented in today's Indo-Germanic studies only by a stubborn minority, who still dream of the 19th century Indo-Germanic cultural priority and ignore any new linguistic or historical insights.

This view is reflected from beginning to end, for example. According to László Antal, Barnabás Csongor, István Fodor, they discuss "Languages of the World" (1970). The authors still adhere to the rigid family tree theory languages and speak about the common ancestral languages of language families as self-evidently as if they were definitively proven facts. We must assume that a long time ago there lived somewhere... a people speaking a more or less unified language. It must be up. These were the ancient Indo-Germans." (p. 20) The results of linguistics conclusively prove, for example, that the Hungarian, Finnish, Lappish, Estonian, etc. languages are just as much a descendant of a former common linguistic state as the Indo-Germanic languages" (p. 7). For "In the Kassite dynasty ruling in Babylon we find a great many Indic personal names" (p. 23). this latter quote, it is necessary to note that, similarly to the Hurrians of Mitanni, the "Aryan ruling class", at one time - especially before 1945, in Hitler's Germany - they also tried to Aryanize certain Kassu names and gods. However, this was only a temporary experiment, because most researchers soon realized the unfoundedness of the assumption and rejected it. (K. Balkan: Kassitenstudien I. — Die Sprache der Kassiten; A. Ungnad: Subartu) Today, only one or two veterans of the aforementioned ahistorical trend occasionally warm up to this hypothesis. (Instead of the older "Kassite" designation, the use of the name "Kassu" based on the original cuneiform texts is becoming more and more common.)

Likewise, the authors testify to the almost complete ignorance of the new archaeological and historical findings with their following two statements: "They penetrated India from Asia... in the first half of the second millennium." (p. The Aryan tribes of the Pre- and Middle 29) And: "The oldest written records of the Indo-Iranian branch... were created among the Indics, presumably in the middle of the 1st-9th millennium. These written records are religious songs, hymns, the so-called Vedas" (p. 30). Knowing the real situation presented above, it is not necessary to refute these views floating in a vacuum.

79

===== PAGE 77 =====

Our authors still consider the so-called linguistic paleontology to be a solid methodological basis for inferring the level of culture and homeland of the ancient peoples of the language families (p. 22). Here too, they represent opinions from about 50 years ago. In recent decades, a number of authoritative Indo-Germanist researchers have shown with very thorough analysis that this method was fundamentally flawed and useless for determining any kind of ancient culture or homeland. (N.Strubitzky: Gedanken über das Indogermanenproblem, 1936/1939.; E.Pulgram: Indoeuropäisch und Indoeuropäer", 1958.; H.Kronasser: Vorgeschichte und Indogermanistik, 1961.)

We find equally anachronistic statements by János Harmatta. Since the Indics appeared in India and Mesopotamia around Also in the article "Iranians and Finno-Ugric peoples, Iranians and Hungarians" (Hungarian archeology studies 1977): "1500 BC, and their presence in the latter area may have been as early as the 3rd century BC. ...we can also count at the end of the 1st millennium..." — writes Harmatta on page 169, unfortunately without any literary references. However, if someone makes such claims, it would be very necessary to name their sources. Modern research places the appearance of the Aryans in India at the latest around 1000 BC, but rather somewhat later. According to archaeological finds, the Indus civilization ceased in several places around 1500 BC, but in considerable areas — in the Lothal region, as well as in the northwestern and northeastern regions — it still existed until 1200 BC. However, finds that could be associated with the Aryans are nowhere to be found directly above the finds of the final phase of the Indus culture — not even in the places that flourished until 1200 BC.

—, but everywhere wide " hiatuses" can be identified and only above a very thick layer do the remains that can be considered the legacy of the Aryans appear: the so-called gray ceramics, in the company of other very primitive finds. The archaeological find complexes everywhere clearly testify that from 1200 BC a few hundred years passed before the Aryans appeared in India (M.Semper: Rassen und Religionen im alten Vorderasien; M.Taddei: Indien, 1970 - Archaeologia mundi; L.Petech: Indien — Propyl. Weltgesch. Volume II, 1976). In addition to citing these newer works, we cannot fail to mention that even before the discovery of the Indus Valley Civilization, unbiased researchers saw the absurdity of the Indus migration and, in close connection with it, the high dating of the Vedas.

One such broad-minded objective scientist was G. Hüsing, who in a Hungarian scientific journal, Keleti Szemle, in 1901, demanded a drastic reduction in the fantastically high dating of the Indian immigration, because — as he writes — the first cultural monuments in India only appear in the 5th century BC, and that under Iranian influence (G. Hüsing: Die Urbewölkerung Irans). Behold, time has fully vindicated Hüsing, although no one in his time took his argument seriously, and the lesson is

80

===== PAGE 78 =====

This is because archaeological finds and historical circumstances provide a more reliable basis than linguistic reconstructions devised independently of everything else. The presence of the Aryans in Mesopotamia around 1500 BC has already been discussed in more detail in connection with the thesis of the "Aryan ruling class of the Mitanni". Let us now add to our findings there the summary opinion of the latest representative work of modern Near Eastern research on this issue: "Since no (Indo-)Aryans can be proven in Fore-Asia, the contact between a small, presumably noble layer of Indians or Indo-Iranians migrating to their later residences obviously took place in the Caucasus or southeast of it." In Hungarian: , can therefore be proven, thus the contact between a small, presumably noble layer of Indians or Indo-Iranians migrating to their later residences and the later Mitanni-Hurrians

"It obviously took place in the Caucasus region or to the southeast of it." (Reallexikon der Assyriologie und vorderasiatischen Archäologie, Volume V. 1976, under the heading "Indogermanen", p. 94).

The internationally renowned Soviet researcher IMDjakonov holds the same opinion (IMDiakonoff: Die Arier im Vorderen Orient: Ende eines Mythos. — Orientalia, NS 41. (1972) pp. 91-121). It is therefore obvious that Harmatta is also a follower of the ahistorical trend which is becoming increasingly hopelessly isolated within Indo-Germanic studies itself.

We consider it important to mention that this school has already taken the absolutization of the sound laws to the point that — again according to him — these The "Reallexikon der Assyriologie" (Real Lexicon of Assyriology) Etruscan, the Karic states that seemingly exact Indo-European sound laws, on the other hand, also cope with insufficient language remnants and definitely non-Indo-European speech, such as the so-called Pelasgian, which (and even the Ainu from Japan, against OGTailleur KZ 77 /1961/ 1-30) als'idg. Sprechen'." (Reallex.d.Assyr.Band V. 1976. under the heading "Indogermanen" p. 95). In Hungarian: , with the help of apparently exact Indo-Germanic sound laws, however, even slight linguistic remnants and proven non-Indo-Germanic languages can be declared Indo-Germanic, such as e.g. the so-called Pelasgian, Etruscan, Carian (and even the Japanese Ainu, against which OGTailleur KZ 77 — 1961 — 1-30) speaks."

We are now looking forward to Professor Harmatta's literary, archaeological or other references to support his above opinion, especially regarding the possible presence of the Indus in Mesopotamia at the end of the 3rd millennium. This was last considered by Mommsen, who, as we have seen, sought the ancestral homeland of the Indo-Germans in Mesopotamia. Since then, this assumption has not even been raised in a serious form, so we do not need to deal with it in more detail.

81

===== PAGE 79 =====

We cannot know what considerations guided a significant proportion of the authoritative scholars of Hungarian linguistics and history when they joined this fossil trend, which also Indo-Germanized the Ainu. However, we can undoubtedly state that anyone who works with the methods of such a school can certainly expect little result in solving the many open questions of Hungarian linguistics and prehistory.

A In conclusion of the "Mitanni question", we would like to highlight another important circumstance from the history of intellectual and cultural history to show how absurd it was to even assume that the Hurrians had adopted Aryan gods. From our archaeological chapter, we already know that the presence of the Hurrians in Northern Mesopotamia and the adjacent areas can be proven by written documents since the Akkadian period (around 2400-2300 BC), and archaeologically it can be assumed with great probability throughout the entire 3rd millennium (Burney-Lang: Die Bergvölker Vorderasiens. — The ethnicity of early Transcaucasian civilization). It can also be proven that these northern areas were in constant and close cultural relations with the Sumerian and then Akkadian cultures, with which they also shared a border (Ungnad: Subartu; Burney-Lang: im).

What boundless ignorance and cultural-historical illiteracy is required in the knowledge of such conditions to declare: these Hurrians were just waiting for more than a thousand years to adopt god names from some wandering, barbaric Aryans? It is worth noting that the Aryans were completely uncultured barbarians even at the time of their immigration to India (L. above the primitiveness of the archaeological finds) and, according to the testimony of the much-mentioned Vedas, even after their arrival in India they had not developed any ideas about the personification of various natural forces as gods. Moreover, most of their god names are not even of Indo-Germanic origin (L.Petech: im; W.Semper: im; A.Ungnad: im; M.Taddei: im).

Our second example of presenting the methods of Indo-Germanic historiography is related to the Medes. We chose the Medes question because it interests us, Hungarians, more closely. The question of the ethnicity and language of the Scythians, Sarmatians, Sakas, Parthians and other steppe horse-riding peoples is organically connected to the nationality of the Medes and the belonging of the Medes language.

Before we turn to the discussion of the actual Median complex, we will briefly outline the historical perspective of research on the steppe peoples in order to better understand the connections.

We must begin by saying that the generally accepted view in the 19th century placed the Indo-Germanic homeland in Central Asia -- Turkmenistan.

As a natural consequence of this opinion, only Indo-Germans could be imagined in this region - and in later times, Indo-Iranians. Thus the Yuks, Sakas, Massagetae, Scythians,

82

===== PAGE 80 =====

Sarmatians etc. without any further investigation Indo-Iranian. (L. O.Schrader's description). In the case of the Scythians we have already seen what methods were used. Although the Indo-Germanic homeland was moved to Europe in the meantime, these steppe peoples implicitly remained Indo-Iranian.

Recently, however, the Scythian question has arisen again. According to T. Talbot-Rice (Die Skythen), the Iranian origin of the Scythians is not yet a settled question. Recent archaeological finds — e.g. Pazyryk — rather, they point to the Altaic ethnicity of the Scythians. But other connections have also emerged, he writes. The great fertility goddess of the Scythians, for example — the "Magna Mater" — appears in southern Russia as early as the Bronze Age and bears a striking resemblance to the Mesopotamian and Elamite goddess statues of a few centuries earlier. It is even possible that the Scythian animal style was inspired by Mesopotamian seal cylinders. On the other hand, in the West, the style and way of life of the Hallstatt civilization are in many ways identical to those of the Scythians.

At the same time, many of the Pazyryk finds show close Urartian parallels.

But the Indo-Iranianity of the other steppe peoples is also sometimes very emphatically questioned by researchers. E. Franke's treatise "Beiträge aus chinesischen Quellen zur Kenntnis der Türkvölker und Scythen Zentralasiens (Abhandl. d.königl. preussischen Akademie d.Wissenschaften, 1904)" is very instructive in this regard, from which we learn that the various Inner and East Asian Indo-Germanic peoples were "discovered" by Klaproth at the beginning of the 19th century and have been uncritically pursued ever since. Yet Klaproth never claims or proves it, but always only assumes it. Regarding the Szakák, for example, in his work "Asia Polyglotta" he literally writes: "dass man es mit ziemlicher Gewissheit behaupten könne". In Hungarian: "that we can assert with considerable certainty" - that they are Iranians.

W. Eberhard, in his two works, repeatedly takes a very skeptical position on the "Yue-Chi" and "Tochär" question (Culture and Settlement of the Border Peoples of China and: The Culture of the Old Central and West Asian Peoples Based on Chinese Sources).

The linguistic monuments in question date back only to the 7th century AD, and on the other hand, it can be proven that the Tocharians acquired the language from which our linguistic monuments originate through language exchange, writes Eberhard. We can see that in both cases we are dealing with scholars who, due to their special field of research — Sino-Central Asian relations — are best placed to form and express authoritative opinions on these issues.

G. Hüsing, in his already mentioned article on the dating of the Vedas (Die Urbevölkerung Irans; Keleti Szemle, 1901), states that the etymologies of the various Iranian names are so artificial and convoluted that they prove nothing.

83

===== PAGE 81 =====

K. Jettmar, HW. Haussig, B. Spuler and L. Petech in their joint work "Geschichte Mittelasi- cns (Handbuch der Orientalistik, 1966.)" state that the thesis of the alleged "Iranian" languages of the Persian peripheral regions is based on very shaky foundations. On the one hand, the few written records interpreted as Iranian are no more ethnic or linguistic evidence than, for example, the Latin language of the 12th century European chronicles. On the other hand, Arab sources repeatedly call the Khorezmian, Sogdian, Bactrian and Fergana languages Turkish languages. Similarly, the Iranian-ness of the Scythians and Sarmatians is not certain. Almost the only evidence is the Europic skulls in the grave finds, they write.

However, it is necessary to know that anthropology does not mean "Indo-Germanic" by "Europic", but this term includes Caucasians, Finno-Ugric peoples, Altai Turks who did not mix with Mongols, Mediterranean and Pre-Asian races, and even Malays and Australian natives (Lajos Bartucz: Race Question, Race Research; István Kiszely: Tombs, Bones, People).

It is therefore a common deception to argue with the anthropological concept of "Europic" to prove the "Indo-Germanic" nature of certain peoples.

The ancient authors unanimously attest that the Medes were relatives of the Scythians, and also that the later Old Persian Empire was originally organized by the Medes and that the Persian Kuras (Greek distortion Kyros), one of the vassals of the great king of the Medes, seized power in a palace coup, claiming that his mother was the daughter of the king of the Medes. Later, during the existence of the Old Persian Empire, the Medes were always considered equal to the Persians. Moreover, research in the 19th century showed that the Persians had adopted their culture, art, and methods of state organization and governance from the Medes. There could be no doubt about such a common view.

This opinion, formulated about 150 years ago, has remained unchanged ever since and has become so ingrained in the scientific and non-scientific public consciousness that no one even thinks of asking about any scientific evidence for it.

The information and data that have survived about the Medes were collected by FWKönig in his work entitled "Älteste Geschichte der Meder und Perser". Let us also -- following König's lead -- get to know the Medes more closely.

We learn in the introduction that we know almost nothing about the language of the Medes. Their people were mostly descended from the indigenous Subartui, Gutí, Lullí, Chaldean (Urartui) and Kassites. Only their leading class was Iranian,

Here we consider it necessary to emphasize that the mentioned peoples each spoke agglutinative languages and, according to the unanimous statement of the international scientific world, were neither Indo-Germanic nor Semitic.

84

===== PAGE 82 =====

Immediately after the above, König states that most of the princely names are not Iranian. He then mentions the monogamy of the Medes in contrast to the polygamy of the indigenous population of Subartu, emphatically underlining that this is a sign of Indo-Germanic superiority. A few pages later, however, in describing an Assyrian campaign against Media, he narrates that the Assyrians only took captive harem women (Weiber") from the province of the chieftain Ramateja, who bore the Iranian name, while the Assyrian source only reports wives (Frauen") from the territories of the other chieftains named Subatu.

Finally, Kynig summarizes the known Median language monuments. No Median common words have survived to us, we only know proper names from two Assyrian war reports. The first one — around 800 BC — lists 28 names. Of these, 19 are certainly non-Iranian, 4 are Chaldean (Urartui-Hurrian), 4 are unknown and 1 "could correspond to an Avestan name". The second monument — around 700 BC — lists 26 names. Of these, 5 are definitely non-Iranian, 4 are Chaldean (Urartui-Hurrian), 4 are unknown and 1 "could correspond to an Avestan name". After this, we will only mention as an addition that "Iranian-sounding" (sic) the rest are different — Kynig states. the ancestor and founder of the Median royal family, Dajaukku, bears the name Subartui, the founder of the Old Persian Achaemenid dynasty, and the name of Teispes is Urartui-Hurrian (ct. the Hurrian names of gods TeSap and TeSeba, and the name of the capital of Urartu, Tushpa).

The names of the two first kings of the Scythians, first mentioned in Eio-Azsia at the turn of the 8th/7th century BC, Bartatua (Partatua) and Madyas, are ancient Cappadocian-Subaric names, writes Kynig. He also notes that according to these names, the Scythians must have been in Cappadocia long before their first mention.

"It is also extremely interesting that among the names of the Median nobles, ancient names such as Zutirna or Uzitar often occur, which are only simple variations of the names Zimutar and Uziwatar in the III. Kiš dynasty." is a sacredly believed thesis. Finally, we would just like to note that Professor König is one of the most internationally recognized experts on the history of Armenia, Transcaucasia and Iran in the 1st millennium BC, and in many cases he presents the results of his own research in his work. The apparent contradictions therefore do not arise from the fact that König's abilities are being misused.

* In connection with these striking name parallels pointing to the 3rd millennium, we are reminded that, in the context of a legendary narrative, the sources mention the continuous close presence of various phonetic variations of the words "mada" and "szubar" (szubir) in written records for more than two millennia. (Chapter 1)

85
===== PAGE 83 =====

The topic would have been beyond the scope of the text, but rather because the author was not willing to conceal or falsify the results of his own investigations in the interest of the "only saving doctrine". Although he never explicitly refutes the typical "Indo-Germanic" statements taken from the literature, he does, within the shortest possible time - sometimes in the next paragraph - dryly present the contradictory data without any commentary. Other, less capable researchers, however, usually slip on such slippery ground, and their works often drown in outright muddle. For example, C.F. Lehman-Haupt, who also wants to - at least partially - Indo-Germanize the Urartians, on the grounds that we find small and tall Urartians among the Urartians appearing on Assyrian reliefs. Well, these "larger" ones, according to Lehmann, are Indo-Germanic (Lehmann-Haupt: Armenien Einst und Jetzt). And we can again observe a typical example of the almost inexhaustible ingenuity of Indo-Germanic studies when it is necessary to conjure up "evidence".

Or J. Markwart In his work "Südarmenien und die Tigrisquellen" (1930), he even "Indo-Germanizes" the Tigris River, saying that its Persian name, "Diglat", specifically an Indo-Iranian word ("durchaus indo-iranisch"). However, it is worth noting — and this was already known at the time of the publication of Markwart's book — that the Sumerian name for the Tigris, which was adopted by the Babylonians, Assyrians, and then the Persians, was "Idiglat". This example also eloquently illustrates how much credence we can give to the "Indo-Germanic" etymologies of various geographical and personal names, which are legion in number and occupy a very prominent place in the scientific evidentiary apparatus of Indo-Germanic studies, and very often are the only "evidence" for declaring a people to be Indo-Germanic (1. above, the etymologies of Zeuss on pages 58-59 of this chapter).

We consider it necessary to explain in more detail the question of the connections between Urartian-Medo-Persian art, because these are integral to the development of the Scythian animal style, on the one hand, and to the question of the antecedents and actual roots of the later, so-called Sassanid-Iranian style, on the other. We can state at the outset that, according to the unanimous opinion of the research, both Medo-Persian and Scythian art can be traced primarily to the art of northern Mesopotamia-Urartian art, and secondarily to the artistic style of the Luristan bronzes (R. Ghirshman: Iran I-Protoiranien, Meder und Achämeniden; F. Altheim: Das alte Iran; E. Porada: Alt-Iran; K. Jettmar: Die frühen Steppenvölker; J. L. Huot: Persien I. - Archäol. mundi; B. Pjotrowsky: Urartu. -- Archäol. mundi; E. Strommenger: Fünf Jahrtausende Mesopotamien; Schmökel, Otten, Maag, Beran: Kulturgeschichte des Alten Orient).

86
===== PAGE 84 =====

Urartu art, however, is closely related to the Assyrian-Mesopotamian style. It is also worth noting that this Assyrian-Urartian style grew directly out of the Hurrian art of the 2nd millennium BC. This is not surprising, since Assyria was a Hurrian-Mitanni province for centuries in the 2nd millennium, and there was even a strong Hurrian-Assyrian mixture of peoples during this period. Urartu was a Hurrian successor state; the Urartian language itself is a later version of Hurrian. This 2nd millennium Hurrian art has already caused many puzzles for researchers. They cannot explain why the Sumerian stylistic elements, motifs, and even religious-mythological images of the middle of the 3rd millennium reappear in the Hurrians — after a thousand-year break — in a massive and almost unchanged form. Even in Urartu, which flourished in the first half of the 1st millennium BC, these close Sumerian parallels can be established, to the point that many researchers, for example, consider the Urartian state organization to be a direct successor to the theocratic Sumerian city-states of the 3rd millennium (Lehman-Haupt: im; A. Ungnad: im; Burney-Lang: 1. m.; R. Ghirshman: im; A. Geotze: Hethiter, Hurriter und Assyrer). Moreover, Ghirshman emphasizes that the layout and division of the Scythian princely tombs are the same as those of the 3rd millennium Urkirdlys.*

In this regard, we refer to the similar observations of T. Talbot-Rice already described, as well as the corresponding observations of the Maykop and Transcaucasian kurgans discussed in Chapter I. But let us also remember Kyriak's above statement that the Scythian royal names Bartatua and Madyas are ancient Cappadocian-Subar names. The second most important antecedent of the Scythian style is the Luristan bronzes, which, according to most researchers, are the legacy of the Kassos, who were expelled from Babylonia around 1150 BC and retreated to their ancestral homeland, the Zagros Mountains. Parallels to the Luristan bronzes are found throughout the Eurasian steppe: in the Minusinsk ancient style bronzes from around 1000 BC (Karaskul culture), in the Ordos bronzes in the Great Bend of the Hoang-Ho, but also in southern Russia, in the earliest Scythian kurgans.** After these antecedents, in the regions between Káspi and Urmia Lakes (Kalardast, Amla5, Hasanlu, etc.) around 1100-1000 BC, a characteristic, * A Scythian burial horse grave dating from the 9th-8th century was found in Hasanlu. Typical Scythian sculpture also appears in Amlas in the 9th-8th century, a good 100 years before the appearance of Scythian finds in southern Russia. This suggests — notes Ghirshman The Scythians have been active in the Transcaucasia-Northern Iran region since at least the 8th century. ** (B. Strommenger, F. Altheim, K. Jettmar, R. Ghirshman cited works.) Moreover, the early Scythian finds in Hungary also show clear Luristan parallels (Gy. László: The art of the period of migration in Hungary).

87
===== PAGE 85 =====

A mixed Hurrian-Mitanni and Lurian-Clāmi style appears, with animal-shaped pottery, artistically crafted gold and brass goblets and deer-shaped belt ornaments; with a peak on the southern shore of Lake Káspi. The so-called cloisonné inlay also appears for the first time in this cultural circle, on the handle of a Hasanlu knife. Here we can first sense the direct antecedents of the Scythian animal style. The Zivjé hoard from the end of the 8th century BC or the middle of the 7th century BC belongs to the same cultural circle — also in terms of territory. The ivory objects show a strong Assyrian and Syrian influence, but on one plate a double-braided palmette pattern appears that has no parallel in Proto-Asia (E. Porada: im). Let us note this circumstance, because here the antecedents of the so-called Sasanian and late Sasanian art first appear, to which the art of the conquering Hungarians also belongs. The metal objects of the treasure show mainly Urartian influence, but strongly mixed with later characteristic Scythian style elements. Cloisonné can also be found here. So the antecedents of Scythian art are present in the Zivjé find as well. But the pieces of the Oxus treasure also represent the same style — with strong Altaic features (Pazirik). Here, enamel inlay appears at a mature, artistic level, on several beautiful bracelets (E. Porada: im).

This art — or rather only a part of it — was later transmitted by the Medes to the Persians. Thus, the common basis of Old Persian and Scythian art is this Hurrian-Urartian and Luristan style — ultimately ancient Mesopotamian-Sumerian in both its components (the works of Porada, Ghirshman, Altheim, Jettmar, Pjotrowsky et al.). The Persians did not add anything to this borrowed art; on the contrary: among them it degenerated into a rigid, schematic, court art devoid of any developmental tendency, which served only to satisfy the representational needs of the royal family and could not take deeper roots (Ghirshman: im).

We can only observe a revival with the appearance of the Parthians, who, with the fortunate inclusion of Hellenistic stylistic elements, renewed and further developed the ancient Urartian-Luristanian-Scythian traditions. However, the strong Hellenistic influences of the early Parthian period later completely faded away and a powerful, rich, independent art emerged, with numerous stylistic variations. One of the stylistic variations of this Parthian art was later adopted by the Sassanids and made it — similarly to the Old Persian rulers — imperial art (E. Porada: im; R. Ghirshman: Iran II.- Parther und Sasaniden; W. Lukonin: Iran II. — Archäol. mundi; H. H. Osten: Die Welt der Perser.) In Lukonin's words: "Demzufolge haben sämtliche, für die früh Sassanidische Zeit charakteristische Züge schon in spät Parthischer Zeit bestan nur mit dem Unterschied, dass dieser Kunststiel eben nicht der einzige war, den es im Partherreich gab Als nämlich die Sassaniden in Fars zur Macht kamen, erhoben sie eine der Richtungen inner-88

===== PAGE 86 =====

halb der parthischer Kunst zum Reichsstiel." (Im182. p.) In Hungarian: "Accordingly, all the characteristic features of the early Sasanian times can already be found in the K&Sö-Pártus period — with the only difference that this artistic style was not the only style in the Parthian Empire. When the Sasaniens came to power in the province of Fars, they elevated only one artistic trend of the Parthian Empire to an imperial artistic style." The Parthians also took completely new paths in construction. Their monumental arched buildings (Ivan) reached the Roman Empire, and are even astonishingly accurate prototypes of the medieval Romanesque style — writes H. H. Osten in his cited work. In Hungarian terms, for example, the forward arch in front of the main gate of the Gyulafehérvár Cathedral, which extends to the entire height of the main nave, is a typical Parthian, Ivan solution. However, recent research has also established that the Parthian Empire maintained close and far-reaching relations with the steppe regions. These relations reached as far as the East Asian Hun (Hiun-Gnu) Empire, which flourished in the 2nd and 1st centuries BC. Ghirshman, for example (Iran II.), considers many of the finds from the Hun princely tombs at Noin-Ula to be of Parthian origin.

However, the ingrained reflections of the 19th century view of Indo-Germanic cultural priority still appear in these newer works and lead the authors to almost incredibly illogical conclusions. For example, in Ghirshman's aforementioned book (Iran I.), after a detailed explanation of the above-mentioned origin of the Scythian style — let us recall his statement regarding Persian art — Ghirshman himself provides evidence of this Hurrian-Urartian-Luristan art, which, according to his own words, was later adopted by the Iranians. At the end of the chapter in question, as a final conclusion, he writes that the Scythian art of the Eurasian steppe, although it has common roots with Persian art, nevertheless adopted a begins to call it "Iranian-Urartian Koiné", characteristically placing "Iranian" in front. And that this is not a slander on our part, lot from the Persians. He then lists some objects from Scythian kurgans of the 5th and 4th centuries, the motifs of which he considers to be of Persian origin in several cases, even if they cannot be traced back to Persia itself from the period in question (im pp. 358-359). From this conclusion it is clear that Ghirshman had good reason to have previously consistently called Pre-Scythian art "Iranian-Urartian".

Similar phenomena can be observed in several other relevant works. Jettmar, in his work "Die frühen Steppenvölker", gets into the same contradiction when he also indicates the origin of steppe art in Old Persian, Achaemenid art, although he had previously stated that the Scythian style developed from Hurrian-Urartian-Luristan art.

89

===== PAGE 87 =====

And T. Talbot-Rice, in his already mentioned — by the way very valuable — book, directly writes that the style of the Scythian kurgans in Cuba is very close to the Achaemenid style of the 8th-7th centuries BC (sic!) (Mixing error?)

According to common knowledge, we can only speak of Achaemenid-Persian motifs from the formation of the Persian Empire, i.e. from the second half of the 6th century BC, while the Scythian style appears on the steppe as early as the 7th century.
Such astonishing misapprehensions can hardly be explained by anything other than the mass inertia of the vast amount of literature that has accumulated since the 19th century.
After this historical excursion, we return to linguistic issues. We briefly review the views and debates of the two camps that emerged after the "Hittite trauma". Of course, we cannot give a comprehensive, detailed description within the framework of this work, but can only confine ourselves to a summary overview of the main trends. Yes, it is good and exhaustive.
A historical overview of science is provided by András Róna-Tas, with particular attention to ideas and theses on language relatedness, to which we refer for detailed questions. His book "Language Affinity"

Examining the development of Indo-Germanic studies further, we can first of all state that several researchers strongly criticized and even did not accept Hrozny's evidence for the Indo-Germanic nature of the Hittite language. (M. Semper: Rassen und Religionen im alten Vorderasien; W.I.Awdjiew: Geschichte des Alten Orients; CF Lehmann-Haupt: Armenien Einst und Jetzt; F.Altheim: Geschichte der lateinischen Sprache.) It is also characteristic that B.Hrozny himself, in his book "Über die Völker und Sprachen des alten Chatti-Landes" written in the 1920s, In his treatise, he only states that Hittite is about as much an Indo-Germanic language as Ancient Egyptian is Semitic. This opinion corresponds best to the real situation.

In the case of the Luwian and Palaian languages, which are also considered Indo-Germanic, we have so few linguistic relics that they are far from sufficient for a definitive language comparison. (E. Dobhofer: Zeichen und Wunder; M. Semper: im) In view of the massive "abnormalities" of the Hittite language — behind which name is hidden the fact that this most ancient Indo-Germanic language does not adapt to the hypothetical Indo-Germanic basic words deduced with the help of sound laws, nor to the assumed Proto-Indo-Germanic grammatical system — the recognition began to dawn in quite a few researchers that perhaps something was wrong with these laws, and that it was also conceivable that it was not the written relics of the Hittite language, but the words of the deduced basic language that showed "massive irregularities".

H. Schuchardt was the first to state openly, without any further ado, that the concept of phonetic laws had once been introduced into linguistics rather irresponsibly ("ziemlich skrupellos") and that it needed to be revised.

===== PAGE 88 =====

their burden was left to posterity. (Das Baskische und die Sprachwissenschaft; 1925. p. 28). Research beyond the Indo-Germanic language family also became more active.
In 1934 (Die Ursprung der Germanen), H. Güntert finally raised the long-overdue question of whether the Indo-Germanic languages were truly isolated from other language families. It demands extensive investigations in this direction, including those of neighboring language families, because: the very idea is The very idea is unlikely, indeed, downright improbable. without any connection." (In Hungarian: , it is untenable that the Indo-Germanic languages should stand in isolation, without any ~~isolierung~~ ~~isolation~~ ~~isolation~~ downright unhalbar, that the Indo-Germanic languages should stand in isolation, 51-52.) In fact, he goes even further. He does not even recognize the thesis, which was previously considered indisputable, that word agreements play only a secondary role in addition to the evidentiary power of grammatical agreements. "Blosse Grammatik, auch vergleichende Grammatik ist keine Sprachwissenschaft" — he writes on page 53. (In Hungarian: , Pure grammar, or even comparative grammar, is not yet a science of language.") Indo-Germanic inflection is a historical phenomenon; it is not a rigid system that has existed since the beginning, but has only developed gradually and is subject to changes. (pp. 54-55) These are also obviously conclusions drawn from the lessons of the Hittite language. Hittite inflection — as far as it can be called — cannot be compared with other Indo-Germanic languages.

Despite all these eminently significant insights, Güntert still does not see the most obvious way out, but instead addresses the key to the whole issue, the Sumerian-Akkadian sources, which are much older than the Indo-Germanic language monuments, in half a sentence: "...in der Nordsüdrichtung sind solche Beziehungen nicht nachzuweisen". (p. 60) (In Hungarian: "...no such connections can be demonstrated in the north-south direction.") Erti applies this to the connections between language families, in the Indo-Germanic and Semitic-Proto-Asiatic relationship. He does not even mention the name Sumerian. However, he finds striking Indo-Germanic connections in the Finno-Ugric, Altaic, Chinese and Korean languages, and even in Eskimo.

The procedure presented above — how Güntert deals with the entire Sumerian proto-language and the pre-Asian culture — is again very typical of the deeply rooted, seemingly ineradicable one-sidedness of Indo-Germanic studies, as a result of which only Indo-Germanic influences can be imagined or assumed everywhere from the outset. This approach also turned Güntert's attention towards the East and Northeast, where it is a much more rewarding and easier task to search for Indo-Germanic linguistic and cultural influences among various backward peoples than in ancient Pre-Asia, which preceded the Indo-Germans in cultural development by thousands of years, where — horrible dictu — it could even happen that the Indo-Germans were culturally and linguistically influenced by the Indo-Germanic languages.

===== PAGE 89 =====

vi would encounter influences. The reason for the otherwise incomprehensible ignoring of ancient Proto-Asian languages and cultures can be sought primarily in this circumstance.
Yet, if, for example, the Hittite language had been thoroughly examined, embedded in its geographical, linguistic and cultural-historical environment, and with the inclusion of other ancient Proto-Asiatic linguistic monuments, they would have been able to observe, as it were, "in statu nascendi," the very first phase of the development of an Indo-Germanic language, and the linguistic and cultural-historical circumstances of the so-called "indogermanizalódás." Hrozny in his book "Die älteste Geschichte Vorderasiens" (1940) emphasizes that the Hittite language shows the most similarities with Latin. And J. Friedrich, one of the most renowned experts on the Urartian and Hurrian languages, in his work "Einführung ins Urartäische" emphatically points out the many striking similarities between Urartian-Hurrian and Latin.
parallels and explains these directly with the Hurrian-Urartian origin of the Etruscans.
However, Indo-Germanic studies have never dealt with these obviously paramount connections, nor have they recognized their significance. The mysterious threads leading from Latin through Etruscan to the pre-Asian Hurrian languages of the 2nd millennium BC have never interested anyone, and they still do not.
H. Schuchardt and most other authoritative researchers in the first decades of our century sharply opposed the neo-grammatical school that proclaimed the absolute validity of sound laws.
However, their criticism was of little use. The methods of the neo-grammarians remained in practice, almost unchanged. This is not a new phenomenon in the history of Indo-Germanic studies: in the 19th century, we have repeatedly experienced in the history of science that, despite all well-founded, objective criticism, the condemned and rejected methods were tacitly continued to be used. And yet, without the objections being refuted in the slightest degree. Instead, they were simply ignored. Well, the same thing happened in the 20th century. H. Schuchardt expresses this in his work mentioned earlier: "Now more and more has been dismantled, but only in the areas of secondary importance and too cautiously: the old framework has remained standing, if only for the sake of convenience." (Im. p. 4) In Hungarian: Well, although more and more has been dismantled, but only in the areas of secondary importance and too cautiously; the old framework has remained standing, if only for the sake of convenience." And yet, with the help of the laws of sound, we cannot in any case gain a clear picture of the inner life of languages — he continues to write, and then concludes: "...they are not laws inherent in the languages" (Im. p. 4), in Hungarian: "...they are not laws inherent in the inner essence of languages") - i.e. the laws of sound.

However, the real situation is reflected in the book mentioned by Güntert: "Especially striking from the standpoint of modern dialect geography"

92

===== PAGE 90 =====

is the almost complete exception of the Germanic sound shift." (pp. 35-36). In Hungarian: "From the point of view of modern dialectography, the almost complete exception of the Germanic sound shift is particularly striking."

Also typical of the general view is L. Wolff's discussion against Wirth (Hermann Wirth und die deutsche Wissenschaft, 1932). Wirth denied the Germanic sound shift and showed that consonants with related articulations can pass into each other almost without limit. Wolff tries to refute these statements, which Wirth demonstrated beyond all doubt by comparing the specific vocabulary of the Germanic languages, by saying that under such circumstances we should give up our previous conviction about the linguistic phenomenon of sound changes; consequently, either Wirth is wrong, or the entire science of language is wrong. For Wolff, of course, there is no doubt that linguistics is right. - "schon der Bequemlichkeit halber" referring to the weight of literature measured in jars - against the demonstrated linguistic facts. In other words, the same procedure as, for example, At that time, Semmelweis was dismissed from the Vienna obstetrics clinic because, contrary to the mass of --, as we saw earlier with Schuchardt. This way of reasoning is nothing other than simply the literature, the number of deaths from puerperal fever in his hospital wards was 1/10 of that in other hospital wards. Let us remember: the lives of hundreds of young mothers were at stake here, yet the literature was the deciding factor, not the facts. For several more years, otherwise healthy young women continued to die en masse, simply could only refer to the fact that after the introduction of carbolic handwashing in his because of the principle of "anonymous snout".

What do you think of this?

Well, in linguistics, no one dies as a result of forcibly upholding false theories, so it is incomparably simpler to argue for all sorts of different theories.
to push down opinions with the help of the principle of authority and literary references. This question has much greater significance in the history of science than we might think; primarily in the so-called humanities, which also include linguistics and historical research.
Wolff's second argument against Wirth is of similar weight. He criticizes him for not going back to Indo-Germanic proto-forms — that is, to the inferred, hypothetical Indo-Germanic root words, the uselessness of which was long ago exposed by the Hittite language — but for using the words as they are found in the individual languages. In other words, he criticizes Wirth for starting from the real words of the languages, documented by written records, and not from the phantom words of the supposed proto-language of a hypothetical primitive people. We could hardly find better evidence of his complete departure from reality.

93

===== PAGE 91 =====

Meanwhile, it became increasingly clear from the increasing archaeological findings that the European Neolithic, Copper Age and Early Bronze Age development was a direct consequence of pre-Asian cultural influences and even partly of colonization (see Chapter I). However, all this was still not taken into account in linguistic research. Like Güntert, H. Krahe

He also looked for connections with other language families in the east, namely with the Altaic ones. According to him, archaeological, anthropological and linguistic data all point to the Altaic peoples and the focus of further research — primarily linguistic investigations — should be placed in this area. (O. Schrader: Die Indogermanen; — new edition, edited by H. Krahe, 1935.)

J. Pokorný (Substrattheorie und Urheimat der Indogermanen, 1936) strongly rejects the view of the archaeologist C. Schuchardt that the geographical names found in East Germany, which show Balkan Illyrian parallels, can be explained by the northward expansion of the Neolithic banded pottery population. He does this with the justification that most of the East German names in question can be easily etymologized from Indo-Germanic. However, in the following lines he himself admits that the Aunjetitz culture — in the area of which these names occur — arose from a complicated mixture of Indo-Germanic and non-Indo-Germanic elements, and that non-Indo-Germanic factors, including the banded pottery people, played a significant role in this process, and that many Illyrian names are also attributed to the banded pottery people.

We could continue listing similar illogical, explicitly one-sided conclusions for long pages. However, let us suffice with mentioning one more glaring example.

Even in 1935, G. Neckel firmly stated that the spoked chariot was an Indo-Germanic invention. (Die Frage nach der Urheimat der Indogermanen, 1935.) Despite the fact that the depiction of the spoked chariot already appears on a piece of Tell Halaf pottery dating back to the 6th-5th millennium BC. (MvOppenheim: Der Tell Halaf, 1931.) N.S. Trubetzkoy's treatise "Gedanken über das Indogermanenproblem" (Acta linguistica 1. 1939.), which he read out in the Prague linguistics circle as early as 1936, acted like a bomb in the atmosphere. Trubetzkoy systematically takes up almost all the methodological contradictions that had been latent until then and had never been systematically summarized and expressed. He shows that the designation "Indogerman" is exclusively a linguistic concept. It says nothing about ethnological questions. The assumption of a common proto-language is not at all necessary to explain the similarities of the Indo-Germanic languages. They could just as well have developed among the different but closely related neighboring languages of certain geographical regions (theorizing about affine languages, linguistic alliances). It is therefore methodologically inadmissible to start from the assumption of a common proto-language.

94
===== PAGE 92 =====

It would be much more important to finally clearly define the criteria by which a language can be called Indo-Germanic, since no unified position has yet emerged in this regard. According to him, the question of lexical agreements is secondary, because on the one hand there is practically no word that occurs in common in all Indo-Germanic languages, and on the other hand many Indo-Germanic lexical and morphological elements are also found in other languages - and indeed the most widespread ones. Moreover, these most common Indo-Germanic words also show such a strong phonological anomaly that their basic forms can only be reconstructed by falsifying the actual data. Normal sound correspondences very often cease to be useful, forcing linguistics to invent various special, sometimes very stilted, sound laws "ad hoc". For these reasons, Trubetzkoy considers structural features to be much more important. According to him, the combined presence of six such features determines the Indo-Germanic languages:

1. Lack of vowel harmony.
 2. Consonant stacking is possible at the beginning, end, or middle of the word.
- The word does not always begin with the stem.
In addition to suffixes, form formation can also occur by changing the root vowel (Ablaut).
Consonant changes also play a role in morphology.
There is no morphological distinction in the sentence between the subjects of transitive and intransitive verbs.
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Based on these criteria, he writes, we must imagine the development of the Indo-Germanic languages in an environment where these six characteristics are found in neighboring language families. He places this region at the intersection of the Ural-Altaic, Caucasian, and Semitic languages—roughly between the Eastern Sea and the Caspian Sea—since all six of the above criteria occur in these three language families—considered as a whole.

His opinion about inflection is that it is a more primitive linguistic state than agglutination, because the development of Indo-Germanic languages demonstrably proceeds from hyperinflecting Caucasian languages towards agglutination. But this is also proven by the fact that the ideal stem put together by Indo-Germanic linguists, Esperanto, is nothing other than a pure agglutinative language built from Indo-Germanic elements,

Trubetzkoy undoubtedly saw clearly most of the open, insufficiently explained and proven questions. However, he could not free himself from the general belief in the field of sound laws. Yet in his criticism he himself shows the failure of the sound laws: precisely in the

95
===== PAGE 93 =====

cannot be applied to the most important, most common words. However, he only draws the following evidence from this: under such circumstances, lexical and morphological similarities are secondary in deciding where a language belongs. It does not occur to him that this statement of his means the castration of comparative linguistics in the strict sense of the word, but apart from that, it directly contradicts his own views. Regarding the structural characteristics he considers to be the most important, he himself further states that they are not unchanging, eternal factors, but only characterize a certain, temporally determined state of the language: the development of the Indo-Germanic languages demonstrably progresses from the characteristics of the Caucasian languages towards the structure of the Ural-Altaic languages, he writes. And if these criteria are constantly changing, they cannot determine the first and most important place of a certain language either. Trubetzkoy therefore apparently avoids the confrontation of general language comparison and is content with a much narrower examination of special, ephemeral, "ad hoc" language states.

But he also exhibits a peculiar, one-sided approach to Indo-Germanic studies: Uralic, Altaic, Semitic, and Caucasian all play a role in the development of Indo-Germanic languages, and he does not even mention the oldest language, Sumerian.

Although Trubetzkoy was heavily criticized by many, his influence has left its mark on Indo-Germanic linguistic research ever since — except, of course, for the school that already Indo-Germanized the Ainu. A follower of this trend is, for example, E. Meyer (Die Indogermanfrage, 1949), who still in 1949 paraded Aryan war chariots in East Asia. In his view, the Hurrians, Kassocks, and Hittites were all

full-blooded "Indo-Germans" who, according to him, shook the whole thing to its foundations.

ancient Near Eastern world. In fact, these Indo-Germanic charioteers traveled as far as China. According to Meyer, the Tocharians were the "Indo-Germans" of the Tripoli culture (no one knew in 1949 what language and race the population of the Tripoli culture had, because we did not have any skulls at that time), who migrated east and formed the Yang-Shao culture. He bases this opinion on the undoubtedly established parallels between the Tripoli culture and the Yang-Shao culture. However, in our archaeological chapter we have already seen how the Yang-Shao culture developed under the influence of the Neolithic-Chalcolithic cultures of Proto-Asia and Turkmenistan. At the same time, we have also shown that the parallels between the Turkmen and Tripoli cultures can be traced back to a common Proto-Asiatic origin. It is therefore not surprising that in the Yang-Shao culture

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tripole elements are also found. And if the Yang-Shao culture |,
Ethnic migration played a role in its formation, and it could only have come from the Central Asian region of Turkmenistan. However, this question is still open today.
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was not yet ripe enough to serve as the basis for any conclusions,
96
===== PAGE 94 =====

J. Pokorný (Die Träger der Kultur der Jungsteinzeit und die Indogermanenfrage, 1949.) and A. Scherer (Das Problem der Indogermanischen Urheimat vom Standpunkt der Sprachwissenschaft, 1950.) partly agree with Trubetzkoy's views, and see the Indogerman languages as a fusion of Finno-Ugric, Semitic and possibly some Caucasian languages. Scherer's article is particularly interesting for us because he mentions that There have been attempts to explain some Indo-Germanic words from Sumerian. The Old Norse word "raudá" = "rotes Eisenerz" (red iron ore) was, for example, attributed to the Sumerian "urud" by J. Schmidt (1890). =,Bronze" (bronze), and G. Ipsen (1924) connected the Old Indic words "gau" and the Old German "chuo" =,Rind" (cattle) with the Sumerian words „ÿÿ/y)y", Rind, Stier" (cattle, bull). Scherer adds the following comment to this: "Doch ist 'urud' vielleicht eher aus einer idg.Sprache entlehnt als umge- kehrt." (In Hungarian: "But 'urud' talán inkább was borrowed from an Indo-Germanic language rather than translated.") He has no comment on the word „,gu/ d", but simply passes over it, and the following In the sentence he is talking about . What does Scherer want us to believe? It would be good if we could state the linguistic facts of the time, but also the cultural historical ones. To a certain extent — but something else. koránt- not entirely — a language and people that can be called Indo-Germanic first appear around 1700 BC in Asia Minor: the Hittites. Then follows a long break, and then in the 8th century BC the first authentic Indo-Germanic language monuments appear in Greece. But even if we take the language of the Linear B tablets as Ancient Greek — which, despite all the hype in this direction, is still far from proven — we still only get to 1300 BC. Sumerian language monuments, on the other hand, can be dated from the second half of the 4th millennium BC. On the other hand, it can also be clearly proven that the Sumerians were already around Bronze casting has been known since 3200 BC, while in Europe, with the exception of Crete, where it also demonstrably arrived from East Asia, bronze appears around 1800 BC, here too as a result of East Asiatic -- Mesopotamian influences.

After this, we can hardly consider Scherer's opinion quoted above as anything other than a bad joke. So, from the only mention of the Sumerians so far, we only learn about them that the blessings of Indo-Germanic culture reached them, and that in such ancient times when the future Indo-Germans had not yet developed. It is worth noting that modern Indo-Germanic studies place the earliest beginnings of the development of Indo-Germanic languages and peoples in the first half of the 2nd millennium BC.

E. Forrer, for example, sees Hittite, Luwian and Palaian as early stages of development towards Indo-Germanic languages. G. Solta, in his treatise "Gedanken zum Indogermanenproblem" (1952), also following in Trubetzkoy's footsteps, states that the Indo-Germanic proto-language

97
===== PAGE 95 =====

and primitive people are exclusively hypothetical concepts, their proof has never even been attempted. In deducing the primitive language, the relationship between Latin and Romance languages was mainly projected back to ancient times. However, the relationship between Latin and Romance languages is not as simple as it was imagined in the 19th century. Today, it is no longer acceptable to assume such a uniform

the existence of a vulgar Latin language, from which the Romance languages would have developed. Latin was originally divided into dialects everywhere and only the influence of the imperial administration and the schools gives us the false idea of unity. If we only knew the Romance languages, but knew nothing about Latin, then we would never be able to deduce Latin even approximately correctly from a comparison of the individual Romance languages. However, the situation is the same - and even more difficult - when they want to reconstruct the ancient Indo-Germanic language, because the memories of the individual languages come from the most different ages. The reconstruction of the ancient language is therefore theoretically impossible, Solta states.

Franz Altheim (Geschichte der lateinischen Sprache, 1951) says the same thing, adding that our historical knowledge consistently indicates that in older times, uniform languages only developed over larger areas where organized countries and empires emerged. We must undoubtedly give Altheim the truth. We merely extend his examples — he mentions the Hellenistic Koine and the Roman Empire — in time, when we emphatically point out that these conditions for the development of a linguistic Koine were already present thousands of years before the Hellenistic-Roman world, and in the 4th and 3rd millennia BC in Mesopotamia and neighboring areas, when the Sumerian cultural Koine dominated the development of the culture of all of East Asia for more than 1500 years. But the repeated formation of large empires can also be documented from the turn of the 4th/3rd millennium: Eridu, Uruk, 118, AkSak, one after the other. The hegemony of the Mesopotamian Empire, and then the empire of Lugalzagezi, were the very first such formations in the history of mankind (Strommenger, E.: Fünf Jahrtausende Mesopotamien), moreover in such ancient times and — in comparison with the surrounding areas — with such a huge cultural dominance that the administrative, commercial, literary, scientific and — not least — cultic, sanctified liturgical language that developed here had to have an almost unimaginable impact - not only within the cultural borders in the narrower sense, but also in all those nearer and more distant barbarian areas with which there were regular commercial or other, for example colonial, relations - on the languages of various primitive indigenous groups. We emphasize again: this developed state and cultural language existed continuously for more than 1500 years and was decisive throughout.

influenced the languages of the surrounding barbarian peoples, who were then still only fishermen and hunters. Dominant-98

==== PAGE 96 ====
There is undoubted evidence that even in the Semitic (Assyrian and Babylonian) states of the 1st millennium BC, it was the language of cult and science, similar to the Latin of medieval Europe.

Under such circumstances, this ancient cultural language cannot be excluded from linguistic research with any excuse. However, for reasons that are incomprehensible to us, modern linguistics, despite the ever-increasing evidence of archaeological research that clearly points in the above direction, does not reach this logical conclusion. Almost all reputable researchers now agree that we cannot speak of a common Indo-Germanic proto-language or proto-people, but that the Indo-Germanic languages developed through the mutual influence of two or more language groups, and that relatively late, according to the evidence of the Hittite language, sometime after the turn of the 6th to the 1 3/2nd millennium BC. In this connection, they consider all kinds of languages: Uralic, Altaic, Caucasian, Semitic and Hamitic; Pelasgian, Ligurian, Iberian, Basque and Berber, but the language of the people who laid the foundations of our entire modern civilization and culture, and who determined and catalyzed the cultural development of almost all of Western Eurasia for more than a millennium and a half in the 4th-3rd millennium BC, is not even mentioned.

We can therefore conclude that although the debates surrounding the Hittite language, and then Trubetzkoy's thorough criticism, have shaken Indo-Germanic linguistics out of its previous self-confident calm, this "productive restlessness" has not yet led to truly positive results. Although the theses of the primitive language and primitive people have been largely abandoned, they are still far from truly applying complex methods in comparative linguistics.

One trend is immersed in the detailed questions of the various so-called "substrate languages", but never looks further than the immediate geographical environment. This research method is typical of H. Krahe's work "Indogermanisch und Alteuropäisch" (1957), in which he analyzes ancient European water names. Some people consider these names to be Ligurian or Tyrrhenian words, he writes, but he argues: why shouldn't these words be Indo-Germanic, when they can be easily interpreted from Indo-Germanic languages. He then states: It seems that the ancient "water words" prefer the short, deep "á" sound. He immediately gives this phenomenon a name: "á-Mode". However, the undoubtedly very scientific-sounding expression "4-fashion" is certainly accompanied by the comment made by A. Deimel in his "Sumerisch-Akkadian Glossar" in 1957 to the entry "a", "aa", "Wasser", "Dunst" (water vapor): "Zu a'a vgl. idg. aha, ahwa, aqua. (To a'a cf. Indogermán aha, ahwa, aqua.)" — And already in 1934!

The second trend works with larger contexts and notices, for example, the undeniable Finno-Ugric, Semitic and 99
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He draws his conclusions in a vacuum, as it were, without taking into account the known historical and cultural-historical contexts of the given eras.

A. Nchring, for example, postulates an imaginary horse-riding people who penetrated from Asia to Southern Russia and then from there to Central Europe in the 3rd millennium BC, who, mixing with the local peoples, would have formed the nucleus of the Indo-Germans (Die Problematik der Indogermanenforschung, 1954.) However, in his opinion, the data can also be interpreted in such a way that the formation of the ancient Indo-Germans involved the fusion of a Uralic or Inner Asian people and a Caucasian or Mediterranean people. Finally, it is also possible - he writes - that all these languages (i.e. Indo-Germanic, Finno-Ugric, Caucasian and Semitic languages) have taken over elements of another, unknown language and their mutual parallels are signs of the influence of this unknown language. Again, the ominous, mysterious, unknown language. How significant and dominant is it that it was able to influence the development of the languages of half of Eurasia to such a great extent that its effects can "unknown language" could have been in the period in question — at the turn of the 3rd/2nd millennium BC — such still be detected in all the languages of Western Eurasia today? In the light of our current historical knowledge, this language could not have been other than Sumerian. Or is it conceivable that archaeology has not found a single memory of this culture to this day, even though this people was so significant that it was able to influence the languages of continents with an effect that lasted for millennia? "unknown" language people

What is, therefore, something obscure in such circumstances? The number of to speak of an "unknown language"? Why can't we say that this language was Sumerian? All the more so because words of Sumerian origin in the vocabularies of the aforementioned language families runs into the hundreds. (WVSöden: Der nahe Osten im Altertum; H.Schmökel: Ur, Assur, Babylon.) Probably not, because at the beginning of the 20th century, linguistics was unfounded and hastily stated that Sumerian was an unrelated, isolated island language. Well, this opinion can no longer be maintained with our current knowledge. Today, it is all - :
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It becomes increasingly clear that the Sumerian-Mesopotamian culture was decisively | influenced the cultures of the western half of Eurasia. These facts do not allow the perception of the Sumerian language as an island language. Finally, we highlight two more insights from Indo-Germanic studies that can be very useful for Hungarian linguistic and historical research. The authoritative Indo-Germanic linguistic and prehistory publications of the last 25-30 years, apart from the already known "ahistorical" trend, almost without exception, agree that the reconstruction of the ancient language is in principle impossible. The view that the so-called "linguistic paleontology", i.e. the determination of the place of the ancient homeland or the level of education of the ancient people by linguistic means, can also be said to be general.

100
==== PAGE 98 ====
attempted definition, a fundamentally flawed method. (F.Altheim: im; G.Solta: im; J.Pokorny: Die indogermanische Sprachenheit, 1954.; A.Nehring: im; E.Pulgram: Indocuropäisch und "Indoeuropäc", 1958.; H.Kronasser: Vorgeschichte und Indogermanistik, 1961.; P.Bosch-Gimpera: Die Indocuropäer, 1961.) However, as we have already seen from one or two examples, a considerable part of the representatives of Hungarian linguistics and history seem to be unwilling to acknowledge these new findings. Hungarian Finno-Ugric studies still insist that linguistics has "undoubtedly and definitively established" that the Finno-Ugric languages, together with Hungarian, derive from a common — inferred, but by no means documented — Finno-Ugric or Uralic ancestral language. Linguistic palontology is also invariably used as a methodological basis in attempts to determine the location of the presumed common ancestral homeland and the level of education of the also hypothetical ancestral people, which, however, have remained unsuccessful so far for obvious reasons.

Perhaps it would not be a completely useless pastime if Hungarian linguistics and history, which enthusiastically adopted and applied the countless unproven and unmethodical hypotheses of 19th-century Indo-Germanic studies, would at least reflect on these much more well-founded insights.
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102
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Chapter 3
A FEW WORDS ABOUT THE GRIMM-FELIE SOUND LAWS AND REGULAR SOUND CHANGES

By the term "sound laws" modern linguists mean the changes in speech sounds that occur in languages and are considered to be regular and lawful. According to the generally prevailing view in linguistics, these sound changes occur almost exclusively in languages and determine their historical development.

Since R. Rask and especially J. Grimm (1848), Indo-Germanic linguistics, and indeed, we can say, the entire modern linguistics, has been based on these sound laws. The example of Indo-Germanic linguistics was soon followed by researchers of other language families and — as we have seen in our previous chapter — they adopted this so-called historical-critical method of language study, which works with sound laws and regular sound changes. Within each language family — and even within individual languages — they developed the respective sound laws valid for a language family or language, regular sound correspondences, and in the future, all kinds of language comparison or language history-language development research were based exclusively on these sound changes observed over time, which were considered to be legal and regular.

In contemporary Indo-Germanic studies, these sound laws have lost much of their dogmatic character - several renowned researchers have already emphatically questioned them - but in general terms they still belong to the fundamental tenets of modern linguistics. Especially in Ural-Finno-Ugric linguistics and in the already described school of Indo-Germanic studies, which absolutizes sound laws, they are still considered the first and most important axiom of scientific linguistics today.

However, the linguistics of the 19th and early 20th centuries was characterized by a general and firm belief in the validity of sound laws. Until the 1920s, there was not a single authoritative linguist who doubted even the slightest bit the decisive role of sound laws and regular sound changes in the development of languages. They could not imagine any other method for discovering the kinship relations of languages. They saw the strict application of sound laws as the only sure barrier to any arbitrary, unsystematic comparison of languages. It was a general belief that only those languages could be genetically related to each other in whose words the sound changes laid down in the sound laws were regularly found. A consequence of this view was the opinion that words with the same meaning in two languages should not even have the same sound form in order to be related, but should have regular sound changes. 105

===== PAGE 103 =====

They have a lot to show. The identical sound already raises the suspicion that we are dealing with a loanword or a coincidence, because closely related words can only have the same sound in very, very rare, special cases, they claim.

Let us examine these sound laws more closely. Let us look at the circumstances of their origin, let us try to find out what they owed their rapid and pervasive success to, why they could soon become the most important axiom of modern linguistics.

J. Grimm collected the words of parallel meaning of the German dialects with meticulous care and laborious work and saw that a general and regular gradual sound change can be observed between German and other Germanic dialects, which consists in the fact that the voiced consonants of the older language monuments have in many cases become voiceless or have faded away over time. Based on this observation, Grimm established the sound laws named after him. He distinguished two temporal sound shifts:

1. Germanic, which presumably occurred in the 1st millennium BC and would express the separation of Germanic (Gothic) articulation from other Indo-Germanic languages; and
2. High German, which would have emerged between 500 and 800 AD and would indicate the sound differences that separate the High German dialects from the other Germanic languages.

Based on these sound shifts, he distinguished three sound stages:
I. the Indo-Germanic basic tone, II. the Germanic (Gothic) tone (after the 1st tone shift), III. the High German tone (after the 2nd tone shift).
Grimm only considered simple labial, dental, and velar consonants in his laws, more specifically the sounds B, D, and G, and their related variants. These sound changes can be represented as follows:

labials dentals velars
I. Indo-Germans
II. Germanic (Gothic) II. High German
Grimm maga így jellemezte felfedezését (History of the German Language, I-II, 1848): "One may aptly compare the sound shift to wagons that run in a circle; as soon as one wheel reaches the place of the previous one, its sound is already changed by a following one."
106
===== PAGE 104 =====

= I
Classification of the most common consonants according to their place of formation, method of formation, and sound quality."
Front articulatory (labial sounds or labials)
voiceless v. hard
Explosion Shang
Frictional sound
Rezgýhang
Middle articulation Posterior articulation circle
round (dental sounds v. (airy palate sounds)
dentals) v. velar and laryngeal sounds v. laryngeal)
voiceless voiceless v. hard
In Table X, we did not aim for completeness; we only provided an accessible overview, for non-specialists.
to facilitate the reader's orientation.
===== PAGE 105 =====

taken, but none of the others. In their movement there can be no space left that would not be immediately filled" (Volume I, page 393) In Hungarian: "The hangeltolódás are best compared to carts that run in a circle; when one wheel has reached the place of the previous one, the next wheel takes its place, but neither reaches the other. In their movement there can be no space left that would not be immediately filled."

German Indo-Germanic studies immediately recognized that Grimm's rules of sound shift corresponded well to linguistic phenomena and were suitable for placing the comparison of Germanic languages on a uniform methodological basis. However, they soon noticed that in other Indo-Germanic languages there were also a large number of words with the same or similar meaning that corresponded exactly to Grimm's sound stages. From this empirical fact, they concluded that these sound changes belonged, so to speak, to the inner essence of languages, similar to the formulas and laws of mathematics, physics or chemistry. This is why they were called "sound laws".

Let us not forget to mention here that we are in the middle of the 19th century, when the unprecedented, almost unimaginable boom and discoveries of the natural sciences had such a huge impact on the entire educated humanity that even the representatives of the humanities, linguists, and historians could not escape them. Thus, the reaction that occurred in linguistics upon seeing the wide applicability of Grimm's sound-shift rules is undoubtedly understandable and explainable from a psychological perspective: in their effort to place linguistics — similarly to the natural sciences — on exact foundations, they made these regularities, "ex juvantibus," unshakable laws, the basic pillars of comparative linguistics, without any further investigation.

It is incomprehensible, but also inexcusable, that they went much further than this: they declared that only linguistic research and language comparison based on Grimm's sound laws can be called scientific linguistics, and that all other methods and opinions are to be considered unscientific from the outset. In other words, Grimm's rules of sound shifts became not only a law, but a dogma.

We would like to emphasize emphatically: these "sound laws" have never been examined by modern linguistics on a theoretical or methodological basis. They have not even attempted to find out the reasons for the sound changes or to explain them on a logical basis. They have simply been accepted as empirical results. (G. Solta: Gedanken zum Indogermanenproblem, 1952.; Schuchardt, H.: Das Baskische und die Sprachwissenschaft, 1925.) As we have said, their elevation to the force of law is still understandable to a certain extent.
Alone, savior
108
===== PAGE 106 =====

However, to call such unproven, unexplained absurdities dogmatic meant disregarding the most fundamental principles of systematic science. Let us think carefully: all kinds of methods, with a future effect, were called unscientific from the very beginning and, we might add, are still called so today. Moreover, in the name of a dogma that cannot even be proven, and which even contradicts common sense.

No one has asked the fundamental, theoretical question that almost arises by itself: what kind of occult power is it that forces the most diverse languages, from Ireland to India, over thousands of years, with constant force and irresistible force, to translate Grimm's schematic sound changes - eerily similar to children's puzzle games? What is language, actually? A Kantian "Ding an sich" in itself, a being with an end in itself and independent of people? Or a sound system consciously created by people, which serves to facilitate communication between them? - Undoubtedly the latter; and as a result, there are no, and indeed cannot be, any occult "laws" independent of our consciousness. To assume such a thing is the same as, for example, believing that a composition that has just been started becomes independent and writes itself independently of the composer.

Even based on these simple logical considerations, it is unbelievable that Grimm's sound change rules would be universally valid linguistic laws. However, there are also practical, empirical facts — and a good number of them — that refute the validity of these laws.

If we look at the examples that linguists usually give to prove the laws of sound shift without suspicion, then at first glance we really have the impression that these are sound changes that occur with admirable precision.

PL.:	Grade I	Grade II	Grade III Latin	heel	Old High German
LAB-i (gilde, HLAUP-an (run, LOUF-an (run, siklik)			run)		
FRANG-o (break, BRIK-an (break, PREHH-an(break, door)				dagger)	
TEC-tum (Dach, THEAC (Dach, tetö) DAH (Dach, tetö) tetö)			break) (Anglo-Saxon)		

However, upon closer examination of these examples, some important inconsistencies soon become apparent. In the first example, the Latin LAB-i is closer to the Old High German form than to the Gothic, phonetically-109

===== PAGE 107 =====
"leg" - using the terminology of linguistics - the Latin sound form is younger than the Gothic: the initial H here is just as "worn out" as in Old High German. The original, complete form is proven not only by the Gothic HLAUP-an, but also by the word GALOPP, whose meaning is largely similar. The Gothic HLAUP-an is therefore - at least in this respect - a more ancient, more original form than the Latin LAB-i.

In the second example, only the Old High German PREHH-an form is compared with the Latin word FRANG-o, because this fits into the scheme. However, they should pay close attention to the very important circumstance that in High German texts, in addition to PREHH-an, BREHH-an occurs at least as often, and moreover, that in Old High German linguistic monuments, the mixing of B and P is generally very widespread: BURG and PURG (castle), BERG and PERG (mountain), BERACHT and PERACHT (Pracht, splendor), they freely alternate with each other. They also do not usually mention that for most words beginning with B, there is no difference even between Gothic and Old High German: geBAR-en (to give birth) is Gothic and gaBAIR-an.

In almost all comparative linguistic work, and especially — by the nature of the matter — in various etymological dictionaries, this peculiar method can be followed and demonstrated point by point: the systematic silencing of a mass of words with phonetic forms incompatible with the phonetic laws, very often even in cases of complete agreement in meaning. Let us look at a couple of such examples.

A The German word "Garten" (garden) originally meant "hedge", "braid", "enclosed place", "fence". F.Kluge (Etymologisches Wörterbuch der deutschen Sprache, 21st, several times expanded and revised edition, 1975) compares it with the following Indo-Germanic words:

Old Slavonic
GRAD-u (enclosed (enclosed place, protected place)
Let's expand our horizons a bit: the German GYR-o (braid)
KRT KRATT-o KURTT-# (to weave) (GARD) (garden) (pen):
HURD (to pack) | (enclosed, 110
city)
GURT-il
With GÜRT (coil, (twist, (belt) screw) [coil, (braid) (basket) (basket) CRT | CHORT-os] HORT-us HURD protected place)

===== PAGE 108 =====

All these Indo-Germanic words, which are obviously organically coherent in terms of phonetics and meaning, cannot be fitted into the straitjacket of sound laws, so Kluge highlights one word each beginning with the sounds G, CH and H, which correspond to Grimm's sound development sequence, and he does not take the rest into account. They belong to "other roots".

Let us note the simultaneous, side-by-side, G, K, H(CH) sound variations within the same languages, in words that are semantically very closely related: in Greek GYR-os, KYRT-e, CHÖRT-os; in Latin GYR-us, CRAT-is, HORT-us; in German GURT-il, KRATT-o, HURD (6th High German); and GÜRT-el, KRATT- 6 and HURD-e (modern German).

Here, then, is the "secret" of the apparent validity of the laws of sound. As a result of this phenomenon, it is possible to demonstrate "regular sound development" in countless cases. All that is needed is to find the words with the appropriate sound form among the numerous variations found in almost every language, place them side by side, and ignore all other words with related meanings and sound forms.

In reality, however, there is no temporal "tone shift" here, but something completely different: by varying consonants with related articulations, languages apparently form new, similar-sounding words to distinguish related concepts. In other words, the tones correspond to shades of meaning. Let us present some examples of this:

German: GUGEL (hood), KUGEL (hood) and öfein. HACHUL (coat) GIPFEL v. GIEBEL, KIPFEL and HÜBEL (peak) from BRECH (break), PRAG (engrave, mark) and FURCH (groove, furrow) " tweed, weave)) / BLITZ-en (lightning), PLOTZ-lich (sudden) and FLITZ-en (gliding) WERG (tow, drop), WIRK-en (weaving) and Old Norse WERAH (tow,

Greek: ERG-o (weave), ARK-ys (web) 65 ARACH-ne (spider)

Latin: BULL-io (boil, bubble), PL-uo (fall) and FL-uo (flow)

Hebrew: GDR (bekerit), KTR (surround) and HTR (surround)

Hungarian: GYrY-r-y, KOR and HUR-s

In relation to the above, we only need to know that the words presented above constitute only an insignificant fraction of the words found in Indo-Germanic languages that are closely related to the word "Garten" both semantically and phonetically. We will get to know such word families in more detail later,

11

===== PAGE 109 =====

Incidentally, to note on the side of this question, the comparison HORT-us - GART-en itself is not correct, because — after all — in Indo-Germanic, in this case Latin, 11 could only correspond to Old German K. However, since the Gothic G in this case scm became Old German K - as it usually does not -, Kluge himself implicitly admits with this etymology that the so-called 2nd or Old German sound shift existed only in Grimm's imagination.

There is also no trace of this High German sound shift in our next example, which is also a model of the astonishing linguistic analogy based on sound laws.

also for its side effects.

Look up the German öfelgen in Kluge's dictionary | The Indo-Germanic cognate of the verb "haben" = "to have, to have". We find the following composition:

Anglo-Saxon | Old Norse

HAB-en | HABB-an HAF-a HAB-an | CAP-ere (one, what | (one, what (one, what van by) by)

Yes, our eyes are not dazzled: the ancestor of the German HAB-en is the Latin CAP-ere. To avoid mistakes, Kluge also notes: "Keine Verwandtschaft mit lat. HAB-ere". (No relationship with the Latin HAB-ere.) But this is natural.

According to the phonetic laws, only the Indo-Germanic basic (here Latin) K can correspond to the Göt H. Thus, the German haben" = "to have, to have" cannot be related to the Latin verb "habere" = "to have, to have". Grimm's domino game rules forbid this. In such cases, linguistics proceeds to the stereotypical magic verb, which is expressed as: "phonologically impossible" — i.e. the relationship of the two words.

The unimaginable illogicality of this way of thinking, even in itself, is further emphasized in our case by the fact that follows clearly and indisputably from the above Old Germanic language records, that the H of Gothic HAB-an has again not changed its sound form, neither in High German nor in the other Germanic languages. Not even to this day. Therefore, the so-called 2nd sound shift cannot be demonstrated in this case either.

S. Feist így ír erről a felnémet hangeltolódásról: "Compared to the uniform common Germanic scheme of the sound shift, the picture of the High German shift is manifold and becomes even more confusing when one considers the fluctuating spelling of the sounds in the various Old High German written works. A hangeltolódás egységes közger- mán sé májával ellentétben a felnémet etolódás képe sokrétű, és ha a han-faszt." (Die deutsche Sprache, 36, old.) Magyarul: "112

===== PAGE 110 =====

"If we consider the fluctuating spelling of the various Old German written records, it becomes even more confusing." The effects of this 2nd sound shift are not uniform; Grimm at the time paid more attention to letters than to sounds, he states further.

Even Germanists themselves acknowledge that many Germanic (Gothic) words, to use their expression, "have not shifted."

All linguistic facts that can be counted prove that the German HAB-en and the Latin HAB-ere are the same word. However, to acknowledge this would mean to admit the inapplicability of the sound laws in the case of a word that undoubtedly belongs to the basic layer of languages. For "haben" refers not only to the 2nd (High German) sound shift, but also to the first, Germanic, thus refuting Feist's statement quoted above about the uniformity of the 1st (Germanic) sound shift.

But there is another, much more important aspect of this "haben"- "capere" correspondence, which modern linguistics does not pay any attention to. The apparently "regular" sound correspondence can only be demonstrated in this case because "capere" is undoubtedly in close semantic connection with the word "habere" and is also its nuanced sound variation. Exactly as the Old High German Latin

In the case of "Garten" we saw:

German
Gothic HAB-eo (haben; has, / HAB-an (haben; has, | HAB-en (haben; has, | HAB-en he has) he has) he has) he has) can

CAP-io (seize), GAB-ei (wealth; GEB-an (give; ad), ! GAB-e

GIB-an (geben) (geben; ad)

economy) but Old Bavarian CEP-an donation get) bekommen; take,

GEBS (ad)

KAUP-o

KAUP-onv. KOUF-on | KOUF-o (Händler; | KAUF-en (Schenk-wirt; (kaufen; buys)

trader)

(buys) innkeeper)

All these words again form an organic semantic unity, and their phonemes differ from each other only in nuances. There is no doubt, therefore, that they are closely related and should only be examined together.

However, with the help of the method of "regular phonetic changes", we can only grasp some details of this unity, and even these few detectable connections will be distorted, because the starting point is the obviously completely one-sided assumption that phonetic changes are exclusively a sign of the temporal development of languages. As a result of this approach — characteristically — the words Gothic KAUP-on, Old High German KOUF-o and German KAUF-en are considered to be Latin loanwords, because the K of KOUF-o cannot be reconciled with the same K according to the phonetic laws.

113

===== PAGE 111 =====

with the Latin word KAUP-o, which has a G sound. The Old High German KOUF-o "is based on a former borrowing from Latin ,caupo', ,Schenk-wirt, Höker', which is probably an early borrowing from the Latin languages, Germanic peoples from the Roman soldiers in the Middle and Lower Rhine region." (Hungarian: ,caupo', ,Schenk-wirt, Höker'; from the word "innkeeper, market vendor", but around 100 AD it could have reached the which is absent from the Romance languages, but around 100 AD it could have reached the Germanic peoples from the Roman soldiers in the Middle and Lower Rhine region.) — we can read in Kluge's etymological dictionary, and then he states: "Aslav. (Old Slav) kupiti = ,kaufen' comes from Got. ,koupon." (In Hungarian: Old Slavic "kupiti"—"kaufen = to buy" comes from the Gothic "koupon"—"to buy.")

G. Kossinna, the renowned archaeologist, rejected this derivation for prehistoric-archaeological reasons at the beginning of our century, saying: archaeological finds indisputably prove that the peoples of Northern Europe had been engaged in extensive trade centuries before their contacts with the Romans. To this, the linguist O. Schrader gives the following, very characteristic, answered: "A It is self-evident that the Germans were already buying and selling long before they formed their word 'kaufen' (to buy) from the Latin 'caupo'. And yet, it is precisely this that opens up new possibilities for us. borrowing is such a vivid picture of Roman-Germanic trade, with no excavation or report from an ancient writer." (It goes without saying that the Germans, even before they formed their word 'kaufen' from the Latin 'caupo', bought and sold. And yet, it is precisely this borrowing that gives us such a vivid picture of Roman-Germanic trade that no excavation or report from an ancient writer can give us."

Let us note carefully the typical linguistic "proof": Kossinna demanded a re-examination of the correctness of linguistic methods based on prehistoric facts, but Schrader's response was nothing more than a side-talk spiced with romantic flute sounds.

Yet a whole series of linguistic monuments testify against the borrowing of "kaufen" from "caupo": Gothic "kaupon", Old Anglo-Saxon "ceap" (Kauf, Preis; purchase, price), English "cheap" (Kauf, billig; purchase, cheap), "chap-man" (Kaufmann; merchant), "chop" (handeln, ändern, wechseln, tauschen; trade, change, exchange, exchange). These words testify that the Germans knew this root even before their separation — that is, before Roman times — in various phonetic and semantic variations, especially

===== PAGE 112 =====

===== PAGE 113 =====

116

117

===== PAGE 116 =====

So here the situation is the same as "kapros", *apcr* (wild boar) or e.g. "öind" is just not uniform. . kam-a" (love) and the Latin . In the case of "am-or" (love): the Indo-Germanic base degree is again

To briefly summarize our discussions so far, we can state that if we take into account all the semantically closely related words of the individual Indo-Germanic languages, with identical or related phonemes, then words emerge en masse that modern linguistics cannot explain with the help of phonetic laws, but can only silence. At the same time, we have been able to draw attention to a semantic phenomenon that seems to be of primary importance, relying on which we can observe the real connections of languages in much greater proportions and examine them in a much more complex way than with the previous regular phonetic development method.

This has pointed out a linguistic circumstance that Grimm and his followers did not take into account. They paid only one-sided attention to temporal sound changes and did not notice the main point: the primary role of sound changes in the semantic development of languages. They were deceived by the undoubtedly detectable tendency in the Germanic languages, which is directed towards the voiced sounds becoming voiceless and then fading out and often leads to the complete elimination of the initial sound. Their greatest mistake was to see in this tendency a strict regularity belonging to the inner essence of languages.

However, the elevation of Grimm's sound laws to dogma and their exclusive application would not have prevented arbitrary, unsystematic etymologies, but on the contrary, led to the arbitrary fragmentation of semantically organically related word families in the interests of a linguistic phenomenon that, to all appearances, plays only a completely subordinate role in the development of languages.

This is how these sound laws look, in the name of which modern linguistics declares in an incontrovertible tone that they are not
119
===== PAGE 117 =====

It is difficult to make a linguistic comparison based on the similarity of words, because it is precisely the similarity in phonetics that is "most suspicious" regarding the belonging of words of two languages. So far, the sound law cannot be applied to the Germanic languages themselves. A brief glance at a Gothic dictionary is enough to establish that the second (High German) sound shift is even more problematic than the first (Germanic or Gothic). Many German words today do not sound any different from what they did in Gothic: Regen — rign (rain), Gras = gras (grass), bringen = bringan (to bring), binden = bindan (tie), Gibel = gibla (ridge), Hahn = hana (rooster), dröhnen = drunjug (rumble), Dunst = dauns (pára), trauen = trauan (trust someone), Knie = kniu (knee), etc.

This list could go on for a long time. In all these words, the laws of sound cease to be of service. Short-sighted fanatics, however, would rather give up on comparative linguistics in the true sense, with a broad perspective, than on Grimm's carousel of sounds, which can certainly be played with very nicely, but which consigns semantics to the rubbish heap of linguistics. This is most clearly seen in the absurd argument that the infallible sign of true word-relatedness is precisely the difference in sounds.

For this reason, for example, the cognation of the German Floh" (flea) with the Latin word "pulex" — which also means flea — is phonetically impossible. There is therefore no other way out than to completely isolate the Germanic flea from the other Indo-Germanic fleas. The Russian "pulex" dances out of the line. The word "pulex" (flea) is not a Germanic word, but a Latin word. The word "pulex" (flea) is not a Germanic word, but a Latin word. The word "pulex" (flea) is not a Germanic word, but a Latin word.

However, Grimm's laws have other shortcomings. They do not take into account important consonants such as M., ŷ L, R, ŷ and their Consequently, the questions related to these sounds are also not explained. Difficulties arise constantly in connection with these sounds. Inexplicable sound phenomena are encountered: the occurrence of the consonant combinations SK, SP, ST, for example, cannot be explained on the basis of sound laws. This incapacity is clearly demonstrated, for example, in the German "Nest" where the phonetic laws do not apply. Kluge assumes that the ST ending is a corrupt form of the Indo-Germanic "SED (to sit) 186. The word "nida", which also means "Nest". He derives the word "nest" from the Old Indic verb "nisad" (to sit), although he himself mentions the word "Gind" As research deepened, however, the weaknesses of the sound theory became apparent to Indo-Germanic linguists. Unfortunately, they did not help by examining the causes of sound changes more thoroughly or by trying to systematically approach the real-world situation.

===== PAGE 118 =====
their role in language development. Instead, they postulated various "ad hoc" auxiliary and spot-rules, special "exception laws" in such glaring cases when the Grimm sound carousel had finally given up its service and the horse's foot had hung out from under the scientific robe for too long. (Verner's law, etc.) For example, they invented the concept of "loudness increase" (Lautsteigerung), by which they meant that the "warlike, courageous, energetic spirit" of the "young, vigorous" Germanic peoples would have triggered sound forms that differed from Grimm's laws. Others again called them "tellers", the harbingers of the great historical deeds that the Germanic tribes were called to accomplish. Let there be no misunderstanding: these expressions were youthful vomiting", the "overwhelming self-confidence" was seen in the irregular voices, or even on that intended with deadly seriousness as scientific arguments. Some have compared the significance of Grimm's discovery to the discovery of America or the invention of gunpowder. If we were being sarcastic, we might add: it's a shame they forgot about Spanish wax. As for the gunpowder analogy, it's really apt: the laws of sound really did explode the semantic connections of words.

Linguistics has done everything in its power to maintain at least the appearance of the validity of the sound laws. Therefore, special attention was paid to removing the most glaring contradictions. The mass exceptions were reduced by, among other things, referring to onomatopoeic and onomatopoeic words. Since in almost all Indo-Germanic languages an extraordinary number of words — and moreover very characteristic, mostly belonging to the basic vocabulary — were found whose sounds ignored the sound laws and did not change over thousands of years, the postulate of onomatopoeic and onomatopoeic words was established. It was stated that we cannot of course apply the sound laws to such words, but we cannot use them in scientific language comparison either, because they developed separately in each language and their identical sound forms only prove that, for example, the cuckoo cuckoos in China as well as in Rome. It is therefore not worth dealing with such words with identical phonemes. The same is said about so-called "child language" words (Lallworte).

After these confident statements, the common man might expect that scientific proof is now forthcoming. However, we look for such a thing in vain. They have not even satisfactorily defined what can be used to recognize an onomatopoeic, onomatopoeic word. This has been left to the individual discretion of the researchers. Therefore, a well-established method — as we have already established repeatedly — has not been applied here either.

We do not want to argue for a moment that onomatopoeic and onomatopoeic words do indeed occur in large numbers in every language. It is only the
121
===== PAGE 119 =====

We reject such misuses of words. Things like the German "Rabc" (raven) and (raven) are found in the Old High German form The case of "Krähc" (sowing crow) in German studies. Since the Latin "corvus" cornix" (sowing crow) is "hraban" (raven), and these words are of course related. But since they are Latin, they have become onomatopoeic words to which the laws of sound cannot be still "Krähe" in German, "corvus" means "crow" just as "cornix", applied. Yet in the title, it is stated that "urverwandt sind lat. 'crepo' = 'knarre, krache'..." (Hungarian: , the Latin 'crepo' = 561 Kluge also notes the "Rabe" "knarre, krache" = 'cluck, crackle, crackle' ..."). Here, therefore, the onomatopoeic word can still be compared miraculously based on the laws of sound, although according to the thesis it should not show any sound changes.

This example perfectly highlights the real purpose and motivations of the onomatopoeic word postulate: with its help, more than one unpleasant exception could be eliminated, and thus the general validity of the sound laws was placed on a more solid foundation. It should also be noted that several new bird names were created in the languages in question by phonetic variations of "Kranich" (crane) also means a bird, so in this case it is also a the Germanic root concept, which is phonetically exactly equivalent to the Latin word "cornix" — onomatopoeic or not, which is irrelevant in this context. This again only supports our view that in the case of so-called "regular sound changes" we are not dealing with a temporal, but a semantic phenomenon. It would be advisable to look at the issue of onomatopoeic and onomatopoeic words a little more closely and objectively assess how much truth there is in the aforementioned explanation. Let us first consider how well founded the opinion is that these words are not subject to the laws of sound, as they are said to be, because they arose separately in each Indo-Germanic language. We will allow this. However, the question immediately arises: when? Undoubtedly, already in the most ancient, most primitive initial stage of languages, when man was still, in the strict sense of the word, an organic part of nature and largely learned the articulation of his speech sounds from the rhythms of nature. These words therefore belong to the most ancient basic layer of languages. The primary importance of root words, however, is emphatically emphasized by linguistics in all kinds of comparative linguistic studies and requires that language affinities must start from the root words. Now, on the basis of what criteria can we decide whether an ancient word is comparable or should be rejected as an onomatopoeic word? Perhaps it is true that Does it follow Grimm's scheme, conceived in the 19th century but never proven by scientific methods? Indo-Germanic linguistics and with it the other linguistic sciences, 122

===== PAGE 120 =====

The linguistics of the lads also tacitly accept this simple solution. They give up on examining a considerable proportion of the words belonging to the most ancient layer of languages, solely because these words have not changed "regularly". Should we rather throw everything away, leaving only the phonetic laws? However, we can also ask what they actually mean by the fact that these ancient, primitive words arose in the separate lives of the individual languages? According to this, there was not a common Indo-Germanic or Uralic-Finno-Ugric basic language? Did each language develop separately from the beginning? We do not get answers to these questions from modern linguistics. Our next thought: if the question of onomatopoeia is so simple and clear, why does it happen — not infrequently — even in very closely related languages that an onomatopoeia word takes on a completely different meaning? Dutch, for example, is so close to German that the Rhineland dialect of "Plattdeutsch" is understood better by the Dutch than by other Germans. Yet, a German visitor may be surprised to read on the door of a Dutch acquaintance the inscription: "bellen" (German: to bark). However, in Dutch, "bellen" not "to bark", but "to ring" with the meaning bir. Similar cases are found many times in the Indo-Germanic languages. What is the reason for these striking changes in meaning? Don't they admit that these words may have originally had a general, not yet specialized, meaning "noise in general" and that the individual languages later, in their separate lives, began to use the ancient, common "onomatopoeic" root to denote different, but already specific, noises? The second aspect of the question concerns words with the same sound and meaning occurring in different language families, which are considered onomatopoeias or onomatopoeias. The general belief in these also is that, having arisen separately, they say nothing about the possible relationships between the individual language families. This position is nothing more than a retreat from the problem, which is in fact much more nuanced than the above superficial statement could ever address. Here too, we can immediately ask: where is the boundary that separates the so-called onomatopoeic and onomatopoeic words from other words in the vocabulary? We have already mentioned that we do not have any scientifically based method for the reliable recognition of onomatopoeic words, but we also do not have the possibility to reliably rule out the onomatopoeic origin of a given word. Under such circumstances, it is only a matter of perspective and individual consideration as to what we consider to be onomatopoeic words and what not. Just as, for example,

In an abstract picture, most people see only a few spots of color, while critics explain half the universe in it. The transition between these two extreme positions is continuous. As a result, in the field of evaluating sound-imitating and onomatopoeic words, linguistics is in a desperate state.

123
===== PAGE 121 =====

confusion reigns. On the one hand, individual people are free to decide which words are pronounced as consonants and which are not, and on the other hand, in many cases they also use words that have already been declared consonants for language comparison. If this fits their ideas (1. the Latin "corvus" brush). In fact, much stranger things also occur. In the latest Hungarian historical-etymological dictionary of Benk 3, for example, we can find such words in a series. declared Hungarian words to be "Slavic loanwords", the roots of which are onomatopoeic words in both Slavic and Hungarian. It is therefore essential to create order in the field of onomatopoeic and onomatopoeic words - at least a relative one. This is hardly conceivable in any other way than to logically continue the idea raised earlier about the origin of onomatopoeic words. The overwhelming majority of researchers agree that these words belong to the lowest, most primitive layer of languages and that primitive man created them by imitating the sounds of nature. Everything therefore indicates that the overwhelming majority of our basic words originated in this way, but in most cases their original onomatopoeic character is no longer known.

Consequently, we have two options: either we give up all kinds of comparative linguistic research, since the majority of our words are ultimately of onomatopoeic origin and, as such, are unsuitable for linguistic affinities and linguistic history studies, or we do not exclude the remaining words, which are still recognizably onomatopoeic, from the research of possible linguistic connections.

Further results can only be achieved in the second way. Of course, a suitable method is first and foremost needed. We believe that this can be developed by conscientiously comparing the semantic connections, branches, and possible changes in meaning found in related languages.

Whether we accept this line of thought or not, one thing is certain: there is an urgent need for some kind of method, because the current state of affairs is unsustainable.

The situation is similar for the "child language words" (Lallworte). According to the opinion of linguistics, parents enthusiastically use the first words of the baby, "naturally" does not apply to such words either.

formation of so-called most diverse words. And since babies all over the world babble in the same way, they claim that these are again phonetic laws, and

therefore cannot be used for language relatedness studies.

This would be fine in theory. However, the problem begins when the Czechs' vocalizations are very limited and, according to observations, barely exceed 12-15 words.

meene", , In his work on child psychology "Die Seele des Kindes", he registers only 13 baby words. These are the following: "nja", "njan", "dada", , "atta", "attar", , "mama", , W.Preyer, "papai", "na na-na", "hatta", "meene-mecne-momm", "momma", "ao-u".
124
===== PAGE 122 =====

In contrast, in various etymological dictionaries, the number of words declared to be of child language origin is well over a hundred. It therefore seems very likely that references to child language words occur primarily where the number of exceptions to the phonetic laws needs to be reduced.

After this digression, let us continue to analyze the sound laws. We have already briefly mentioned the sound correspondence of the Latin words "corvus" and the German "Rabe". In this case, "regular"

we cannot be helped by listing these three words, because the high points of comparative linguistics for us is not to stop at the complacent, navel-gazing view of the detailed parallels collected to justify the sound laws, but to try to uncover other semantic connections by searching further. Our efforts were not in vain here, because we came across the following connections — not "sound-historically" regular, but semantically all the more interesting:

Greek	latin	Old High German	German			
KORAB-os	CORV-us	HRAB-an	RAB-e (raven)	(raven)	(raven)	(raven)
ORPH-naios	CREP-usculum	ERPFS	RAPP-e (dark)	(darkness)	(black 16)	(gloomy)

(In German we see again the phenomenon that we have already seen in the word "Führer" we observed: not only the initial consonant, but also the following vowel disappeared.)
In the first thing we can establish, however, is the "black raven" and the semantic parallelism of darkness, which is actually not surprising, because it is undoubtedly a reasonable connection. The
is no longer so natural — at least not for linguistics working with sound laws — that words carrying two meanings are phonetically closely related in each language. They show that in the development of languages, in the formation of new words, we are faced with conscious and logical human activity.
And this is precisely the circumstance that phonological linguistics does not take into account. For it, sound changes are a kind of subconscious or instinctive process that moves at the lower limit of consciousness.
"language development trends" results. (Róna-Tas, A.: Language affinity.)

After this basic, theoretical statement, let's go further and see whether such semantic-phonetic parallels are only peculiar to Indo-Germanic languages, or whether we can find them in other language families as well:

125
===== PAGE 123 =====

akkād heber		Arabic ERIB-u	'OREB	GH-R-B		
(raven)	(raven)	(raven) EREB-u	"ARAB"	GH-RB (supplementation)	(darkness)	(lemeretel)

So, in Semitic languages, the two concepts are the same, both in associative and phonological terms. Much more important, however, is that the Indo-Germanic and Semitic words show close phonetic parallels in both meanings. How can this double meaning similarity and the undoubted phonetic relationship be explained? Postponing the discussion of this eminently important question, we will now only draw lessons concerning the laws of sound.

In Semitic languages, the consonants that carry meaning, but also the word-initial sounds, are the same in both meanings from language to language. In contrast, in Indo-Germanic languages, the consonants of the two meanings differ in every language, and the word-initial sound is the same only in Latin and modern German, not in the other two languages. These changes are completely irregular from the perspective of sound laws. We already know why: because they are not signs of the temporal development of languages, but serve to make semantic distinctions. However, if we evaluate these sound changes strictly adhering to the basic principles of modern linguistics, we must conclude that, due to the irregularity of "sound development", the Indo-Germanic languages are not suitable as starting points and standards for the sound shifts laid down in Grimm's sound laws.

But we can also learn another lesson from the German "Rabe" series:
be upper class Akkadian Greek. As can Hebrew arab K H 0 0 "(H) GH
seen, the "sound development" in Indo-Germanic is the reverse of Semitic. The oldest Semitic language, Akkadian, shows the "most developed" Indo-Germanic sound form, while Young Arabic corresponds to the sound form believed to be the most ancient of the Indo-Germanic basic stages.
This phenomenon cannot be explained by Grimm's laws of sound either, unless we want to assume that the occult sound-goblin operating in languages, in his playful mood, played the film of "sound development" in reverse in the Semitic languages, thinking: if the Semites write in the opposite direction, why shouldn't they have reversed sound laws as well?
126
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===== PAGE 124 =====

However, the linguist has no need for such mechanical laws. In spite of all the loud statements, the real situation is becoming clearer and clearer: these "sound laws" not only smothered the true meaning of linguistics, semantics, in its infancy, but they could not even prevent arbitrary, unsystematic word comparisons; on the contrary, they directly provided food for the most fantastic of all comparisons, because the meaning of words played only a subordinate role in the face of the "regular sound correspondences" that dominated everything (1. "haben" = "capio", "Herz" = "šrad-dha")

That is why even the most representative works can be adorned with such amazing etymologies, such as the derivation of the German word "Schraube" (screw) from the Latin expression "sus scrofa" (sow), saying: the pig's tail also curls like a screw thread. (Kluge's etym. dictionary.) If they had had even a spark of sense for semantics, they would not have had to concoct such hair-raising vegetables, since the Latin parallel they were looking for almost suggests itself: "serpens" (snake) and the international European word "serpentine" derived from it.

The Latin "sus scrofa" literally means "swollen pig", being "scrofulus", "scrofulus" = , tumor, lump, nodule. From this comes the medical term "swollen", the technical term for skin tuberculosis: , scrofulosis. But we also find this German, of course not "goiter", i.e. also "tumor", "lump, lump", "screw", but in the meaning: , Kropf".

"Schraube" — In connection with the etymology of "scrofa", we cannot leave without mentioning the very characteristic fact that this wise statement of Indo-Germanic studies gives the meaning of the word "scrofa" simply nowadays may have even gone back to the Latin language itself. The newer Latin dictionaries mean "sow" together with as "sow", while "scrofa" means "lump", "nodule", "tumor" from the "newer" Latin the word "disznó", but never by itself. At the same time, the words "scrofulus", "scrofulus" only have the meaning of "sus", that is, their meanings are missing vocabulary, including, for example, from Alajos Györkösy's "Latin-Magyar szótár" published in 1978. We do not wish to draw any conclusions from this for the time being, but we consider it necessary to state the fact itself.

The unimaginably primitive etymology of the word "Schraube" is also a direct consequence of the laws of sound. Under their spell, modern linguistics cannot see the A from the trees. forest. We must take this expression here in the strict sense of the word. For we are really faced with a forest, the forest of semantics, which we have already touched upon briefly, in a snapshot manner, in some of our examples in the course of our previous discussions. However, modern linguistics, with its method of searching for "regular" sound correspondences, sees only those trees in this forest that correspond to its prefabricated scheme. Like the woodcutter who, walking through the forest, sees only the

===== PAGE 125 =====

He looks out for crooked trees to be cut, which he then marks with chalk. These may be oaks, beech trees, elms or others, but it is unimportant to him, because he only selects according to one criterion: curvature.

But let's look at a specific example:
Latin: | GLOR-ia KLAR-us HILAR-is (fame, brilliance) | (bright, shiny) | (clear, cheerful)

German:] GLAN-z HELL (light, shine), CHIR-ance (bright, clear) choice. The meanings are undoubtedly (bright, shiny) Well, according to what "sound law" should we group these words? It is free to close to each other, so all three words can be paired arbitrarily, depending on what prefabricated "sound law" we approach them with in our pocket. (The N of "Glanz" can be easily explained by the RL - N transition.)

In the following table — which is, however, far from complete — we can see in an illustrative way that linguistics based on the methodology of regular sound development can capture only an insignificant proportion of semantically closely related words.

GYR-o* (turn, twist, | KYRT-e (braid)
coil, screw) protected place)
GORYT-os (quiver) KYRT-ia (basket) CHORD-os (cord, KART-alos (basket) | rope)
GYR-us* (winding, coiling) CRAT-is (braid) | CHORD-ia (cord, garden) varás, ring)
GORYT-us (tegez) rope)
GART (city) HURD (braid) 6th High German | GURT-il (belt) KRATT-o (basket) CHREZZ-o (cart-
ART-o kas, cart-kas) GÜRT-el (belt) KRATT-e (basket) HÜRD-e (pen) GART-en (garden) KRATZ-e (hāti- HORT (ment-severe,
(garden) basket) shelter)

* The root of all the words in the table is the Greek-Latin "gyr-", which in turn is a derivative of the Sumerian words GUR (wenden, drehen; ein Massgefäß; to turn, rotate, a kind of unit of measure) and GAR (rings umschliessen, Einfassung; to enclose, frame, fence).

128
===== PAGE 126 =====

This also includes the words 6ind KRT (spinnen; fon) and CRT (verknüpfen; összefűz). According to Indo-Germanic studies, only the four framed words from the 6 sz6- family are in a Öösrokonssägi relationship, while the rest are etymologized separately, based on different roots.

This semantic richness of languages therefore makes it possible to include the changes of consonants with related articulations in several arbitrary systems and to establish different so-called "sound laws".

However, there is also a distinctly comical side to this question of sound laws, which would actually be enough on its own to prove the unfoundedness of Grimm's laws.

As we have seen, the so-called 2nd sound shift would have taken place in High German, they claim. Well, High German is the most unsuitable language imaginable for establishing such "laws". From its linguistic monuments — as we have already touched upon — it can be undoubtedly established that no distinction was made at all between BP, DT and G-K. We have already presented some such fluctuations, let us add to these with a couple of more examples: BALG and PALG (boron), DRIGIL and TRIGIL (envoy) or GRIÖZ and CRIÖZ (grain) alternate freely in the linguistic monuments. Well, this state of affairs lasted in the High Germanic until the 19th century — even officially! O. Jespersen, the renowned Danish linguist, in his book "Phonetics" (1897-99) mentions in his book that in South German offices, even in the 19th century, there was only one common column in the name lists for the initials B and P, and D and T.

Nowadays — due to the central standardization of spelling — this is officially no longer the case, but in practice it is still the same. For example, if we say the name "Pichler" to someone, they will immediately ask: should I write it with a hard or soft P (sic)?

For Hungarians, this may seem incredible, because we can immediately tell from the pronunciation whether someone said Bt or Pt. However, with High Germans, it is not just that the articulation is sloppy and blurred. They simply do not have the ear to register the difference between voiced and voiceless pronunciation. No matter how "voiced" we say a Bt, they still ask: is it hard or soft? The situation is exactly the same with D and T, and similar with GK, although not so blatant and not 100%. In school, children are still taught to this day that B is "weiches P" and P is "hartes P"; and so on, with the DT and GK sounds.

So this is not an exception, but a general phenomenon; they cannot distinguish voiced consonants from voiceless ones.

And Grimm's laws are based on the changes of these consonants, BmP>PH>ED+-TrTH>SesG>+K->CH->H. We have also seen that they grew out of the results of German dialect research, German

129
===== PAGE 127 =====

They were developed by scholars (Grimm, Kuhn, etc.) and originally served primarily to explain High German, and secondarily to explain Germanic phonemes. They could only become generally valid "theories" as a result of the semantic phenomenon already mentioned above - and their almost complete lack of criticism.

| These "laws" were established by people who could not distinguish the sounds on which they based their rules. Moreover, they had as their starting point linguistic monuments that their own ancestors — blessed with the same ear for words — had found, for better or worse, to write down.

Knowing these facts, only one consequence can be drawn: among all the peoples of the world, the Germans have the least right to speak about the lawful, regular changes of ! B, D, G and their consonants in the history of language. ! And they are the least called upon to observe such temporal regularities — even if they existed. | 1

-to formulate.

Let us now briefly summarize our criticism.

1. The sound laws are incomplete because they only take into account a part of the sounds, and not other, common and important phonetic phenomena.

2. They have no solid basis, because the so-called "single Indo-Germanic base degree" does not exist in reality, but mostly shows the same | sound shifts as Germanic words. |

3. They are wrong because they see a uniform process in sound changes, but in reality they very often occur unevenly. |

4. They are one-sided because they see sound changes only as a temporal phenomenon, while in reality they are also spatial phenomena, but most importantly and primarily the means of word formation in languages.

5. Tendencies only, not laws, because the number of exceptions is extremely large, and even words with different tonal degrees often occur simultaneously within the same language.

6. Their validity is only apparent, because in most languages there are many words with parallel phonemes and identical or very similar meanings, among which we can choose according to which phoneme corresponds to the laws of sound.

7. They are useless for determining the real circumstances of language affinity, because they reveal many actually related words as coincidences, loanwords, words of unknown origin, or onomatopoeias if they do not fit into the scheme; thus, they exclude a large amount of vocabulary from comparison. |

8. They are unsuitable to serve as a basis for general linguistics, because they are not insights gleaned from the sound-change studies of general language studies, but only from the Germanic and other Indo-European languages.

130
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===== PAGE 128 =====

They reflect the relationship between Germanic languages, but even this is only a very rough approximation.

9. They cannot be used satisfactorily even in the Germanic languages themselves, because here too there are numerous stable word stems that correspond to the phonetic forms of words of both the I. (Indo-Germanic) and II. (Germanic or Gothic) stages,

10. They are particularly harmful because they prevent the development of the most important area of comparative linguistics, semantics.

11. They led linguistic research into a dead end because they inevitably led to the absurd and paradoxical belief that words with the same sound can almost never be related.

12. It was an unreasonably one-sided view to view sound changes only as a temporal process, because there can be several reasons for the metamorphosis of consonants with related articulation:

First and foremost, it has been a tool of word formation since ancient times, consciously used to distinguish nuances of meaning; that is, it is a semantic phenomenon.

Secondly, various, e.g. civilizational influences, tone grinding, wear and tear, in the direction of easier, faster or even slurred pronunciation, such as the advertising or telegraphic style of our time. Thirdly, it is a consequence of certain speech defects: rasping, slurring, etc., which can also extend to entire languages, e.g. in the cases of the R and L languages, or the S and Sz dialects (cf. the so-called Sibolet phenomenon known from the Bible, and in Hungarian terms, the Csángó dialect).

Fourth, auditory insufficiency can also cause voice changes, as we saw earlier in the case of the German speakers.

Fifth, congenital, inherited microanomalies in the anatomical structure of the larynx, nasal and oral cavities (endemic nasal polyps, various mild degrees of wolf-like turbinates, etc.)

Sixth, consonant assimilations to facilitate pronunciation.

Let us conclude the discussion of the laws of sound with a timely epigram by Goethe: "You like to anatomize language, but only its cadaver spirit and life escape from the rough scalpel."

(In Hungarian: "You may dissect the tongue, but only its corpse. Spirit and life quickly slip from under the rough knife."

131
===== PAGE 129 =====

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132
===== PAGE 130 =====

Chapter 4
THE SEMANTIC ROLE OF SOUND CHANGES IN LANGUAGES

Following the generally accepted division of speech sounds into vowels and consonants, we present the changes in these two groups of sounds separately.

I. Vocalization

Vowel changes in inflected languages (Indo-Germanic, Semitic) have a primarily grammatical role. They serve to distinguish tenses, moods, plurals, and internal word formation (verb, noun, adjective, participle). These variations often use the entire vowel scale.

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gender: brechen, bricht, bräche, brach, gebrochen, Bruch, brüchig; (to break, to break, to be broken, to be broken, to break, fragile); bergen, birgt.barg, geboren, Burg, Bürger; (to save, to save, to be saved, to be saved, to be saved, to be a city dweller); trinken, trank, getrunken, Trank, Tränke, Tränkchen; (to drink, to drink, to drink, to drink, to drink, to drink). (Cf. the formation of the active verb, also with Ablaut (stem vowel change): tranken = to drink;

Greek: phero (to take away); phor (thief);
Latin: tego (cover), toga (blanket), tug-urium (shelter, roof);
Arabic: katab (to write), aktub (I write), kutiba (was written), kataba (wrote), katib (writer), ma-ktub (written).

If we take a closer look at the Indo-Germanic language monuments, we can clearly distinguish an older, richer vocalization and a newer layer, much poorer in vowels.

Greek: old; kalypso (hide) newer; klepto (hide, steal) Latin; old: cullen (peak) newer; culmen (peak)
thorybos (confusion) tyrbē (confusion)
sidera (stars) | stolidus (foolish, foolish) yā astra (stars) | stultus (foolish, foolish)
Russian: old: gorod (city) | zoloto (gold) | moloko (milk) | soloma (straw)
newer; grad (city) | zlato (gold) | mleko (milk) sloma (straw)
Ofelder: „marnisco“magad manodfelisa Kiricha |sciluf 2 German, Mensch |Magd Mond |Fels {Kirche = [Schilf | Obst | man) |(servant)|(moon) | (rock) (church)} (reed) (fruit)

133
===== PAGE 131 =====

These vowel changes did indeed occur over time, but their explanation is not found in some occult law of language development, but simply in the different strengths of the vowels in stressed and unstressed syllables: the unstressed vowel faded over time, then disappeared completely. This process is evidenced, for example, by the fact that Germans cannot pronounce unstressed vowels for long periods. In monosyllabic stems, such as „leb-en“ (to live), the E of the infinitive is barely audible in pronunciation. And since in most of the two- or multi-syllable words of the Indo-Germanic languages the first syllable is unstressed, the first vowel is usually dropped. This is how the consonant clusters so characteristic of the Indo-Germanic languages arose, mainly at the beginning of words, but also in the middle or at the endings, according to which syllable was unstressed. As we have seen, Trubetzkoy considered this so important that he included it among the six criteria defining the Indo-Germanic languages (I. in Chapter 2).

These vowel drops often lead to seemingly so different sound forms in related words of certain Indo-Germanic languages that linguistics often assumes metathesis, or sound transfer.

PL: Greek German | yyyyy-yy (confused) | TURB-a (Confusion) TRUB (Confused) Greek os (long) | DLUG-o (long) | DOLG-o (long) West Slavic Russian DOLICH-

Latin West Slavic 'J Russian
CURT-us (abbreviated) KRAT-kij (short) KOROT-ki (short)
Old High German
DURUHH (through, through)
German DURCH (through, through) Upper THROUGH (at, across) across)
German Russian MANAG-i (crowd) MENG-e (mass) | MNOG-o (many) of English West Slavic Russian
German GART (town) CITY (city) GOROD (city) Old High German
HROS (stallion) HORS-e (stallion, 16)

From the examples presented, it can be clearly established that in these differently vocalized word forms, it is not about vowel metathesis, but simply about the loss of the unstressed vowel 826. Cse-134

English

===== PAGE 132 =====

With a little effort, we can find the fully vocalized word form in almost all similar cases, primarily in older linguistic monuments.
The actual reason for postulating metatheses is that the basic words of the hypothetical Indo-Germanic proto-language are imagined with a certain specific vocalization, and any sound form with a vocalization different from this inferred proto-form is seen as an abnormal sound development.
&However, the thesis of the fixed vocalization of the Proto-Indo-Germanic base language is not confirmed by linguistic facts. Not even if we assume that this supposed base language really existed. When comparing the individual Indo-Germanic languages, we find a motley mixture of vowels almost everywhere. It cannot be otherwise, since these languages vary the vowels in verb conjugation and internal word formation on a wide scale. Thus, it is often not possible to determine with certainty even within a language which vowel we consider as the starting point, i.e. which is the "basic vowel". When comparing all Indo-Germanic languages, it is simply impossible to determine the ancient basic sound.

PL: "Feuer" (fire): AGN-i IGN-is "Ich liebe" OGN-i OGN-is (Mind) (Latin) (auslāv) (Lithuanian friend)
(I love): "hocken, sich Lion LOVE LIEB (English) (Russian) (German)

"niederkauern" (to crouch): In the various Slavic languages, the root of this word is found in the following variations: CAP-, CEP-, CIP-, COP-, CUP-. (E.Berneker: Slavisches etymologisches Wörterbuch AM.)

J. Grimm and, following him, Indo-Germanic studies considered the present tense vowel as the basic tone for verbs — and still do today. However, this is just as arbitrary a procedure as if, for example, in the case of the word "lieben", we were to ignore the English and Russian forms and take German as the standard. Moreover, the present tense vowel is often not uniform even within a language, for example, in the so-called "strong" German verbs: "ich breche" (Turkish), "du brichst" (you break). So we can once again establish a complete lack of thorough consideration, since the vocalization of the present tense is not uniform in Grimm's own native language. Despite this, linguistics treats the present tense vowel as a matter of course as the basic tone. It is not advisable to build entire linguistic historical systems on such unstable foundations, as was the case in Indo-Germanic studies. Such structures — although they may be well-thought-out, logical, coherent, and complete systems in themselves — can only lead to a distortion of the real coherence of languages, due to their false starting point.

===== PAGE 133 =====

seit and become a source of completely erroneous language-theoretical ideas, language-historical; language development theses. Therefore, we are not criticizing the various structures of modern linguistics, not that -- let's say -- the individual related languages, with the help of certain rules, are led point by point back: to, for example, the Finno-Ugric basic language". These derivations, language and sound: historical systems would undoubtedly be reasonable, logical, but only if the real existence of the supposed Finno-Ugric basic language could be proven in a way that excludes all doubt. However, as long as this basic language is nothing more than a mere hypothesis, the entire system built on it is also hanging in the air and can in no way be considered a solid linguistic recognition. Under such circumstances, therefore, no one can claim that only his hypothesis is "scientific", all

other assumptions are inherently unscientific. On the contrary, the simultaneous establishment, consideration, and comparison of several working hypotheses can only facilitate a better approach and understanding of the real relationships of languages.

But let us return to vowel changes. In Semitic languages, the grammar is almost entirely based on vowel variations, so much so that the writing systems of these languages hardly indicate vocalization, because in them the description of the consonant skeleton is sufficient to precisely determine the meaning of individual words. These variations occurring in grammar are called symbolic vowel changes in Semiology. In fact, they perform the same function as the "Ablaut" in Indo-Germanic languages.

As a result, it is very difficult to examine the possible semantic implications of vowel changes in Semitic languages, because we cannot determine the vocalization of old Semitic languages and dialects that are no longer spoken based on written records. Thus, in the study of Semitic vocalization, we must limit ourselves to the modern Semitic languages, Hamito-Semitic Coptic, and the Akkadian-Assyrian-Babylonian linguistic records — the latter of which adopted the Sumerian syllabary that also marked vowels.

However, the fact that we must also take into account the semantic role of vowels in Semitic languages can be clearly established from the following few examples:

Hebrew: RAB (shooter), ROB (crowd), RIB (quarre) SAD (breast), SED (demon) KAROB (near), KEREb (body) 'EREB (evening), 'OREB (raven)

But Indo-Germanic languages also contain variously vocalized word forms that represent conscious word formation, specialization or variation of a basic concept.

Greek; KRAT-eros (strong), KART-eros (hard) ALEIPH-o (ointment), ALPH-i (fat)

136

===== PAGE 134 =====

Latin: CLAM-or (fame, fame), CALUM-nia (bad news, notoriety) VALID-us (strong), VALD-e (very, strongly) Gender: WEND-en (turn), WIND-en (twist) SCHROFF (rough), SCHARF (sharp) LIEB-en (love), LOB-en (praise) BITT-en (ask), BETT-eln (beg, implore)

These examples clearly fall within the category of semantic vowel changes.

Since Indo-Germanic and Semitic languages primarily form different grammatical forms by changing vowels, the actual word formation by means of vowel variation naturally plays a subordinate role in them. However, we can recognize from the words presented above that these languages also use the different phonetic forms of vowels to form new words expressing related concepts.

The situation is fundamentally different in inflectional languages. Since they express grammatical relations with inflections, suffixes, and signs, they can freely use the entire vowel scale to distinguish the nuances of semantically related words.

P. Hungarian: HAL, HÁL, HULL; LAK, LIK, LUK, LÉK; ESZ-ik, ISZ-ik.

The inflected Sumerian language also used vowel variations to express synonyms and semantically close meanings.

Sumerian: BAR (to tear), BUR, (to tear apart) GAR (to surround), GUR (to surround) GUR, (to rush), GIR, (to run).

In summary, we can state that vowels can only play a subordinate role in comparative linguistic studies. In inflected languages, they are mainly used in verb inflection and internal word formation, but here they can cover the entire sound scale. In inflected languages, their changes primarily serve to vary and specialize a basic concept, to form semantically related word forms, i.e. in these languages they do not determine the actual basic meaning of words either. This role can ultimately be compared to their function in inflected languages, but with the difference that here they denote somewhat broader semantic connections than those created by internal word formation. The Semitic writing system, which denotes only consonants as the only essential, defining sounds of words, can therefore be said to be undoubtedly logical and legitimate.

"Ablautos" are Indo-Germanic and Semitic words.

It follows from what has been said that the method of comparing consonant stems can be applied — with due caution, of course — to Indo-137

===== PAGE 135 =====

In Germanic languages, it can be applied without further ado to language affinities. But this method can also be used in the case of inflectional languages, but here with even more caution and only with reference to the bare stems, after removing all kinds of affixes, suffixes, and signs,

II. Changes in consonants

In contrast to the practically free vowel variations, there are limits to the changes of consonants. In general, we can say that mostly only consonants articulated in the same places of the sound-forming organs change into each other. In addition to these most common consonant changes, the RL transition, the so-called satem-kentum variation, nasalization and the so-called "lekopäsa" of word-initial consonants also play a significant role in languages. Let us look at the latter first.

RL transition

Relatively many languages lack either the R or the L sound. In ancient Egyptian, Zend, Livonian, Korean, and Japanese, for example, there is no L; Chinese, Eskimo, and several Native American and Oceanic languages, on the other hand, do not have the R sound. However, it is well known that slurring is a fairly common speech defect, and that children learning to speak often cannot pronounce the R sound for a long time and pronounce L or J instead.

So here — especially in the case of L-languages — we are talking about articulation difficulties.

Even in languages where both sounds are found, there is often a transition between them. For example, Parisians say "colidor" instead of the standard French. "corridor" word

Particularly interesting in this regard is the case of the German word "schlürfen" (to slurp), in which it is clearly observed that "sound development" in languages does not occur in the way assumed by modern linguistics, but is the result of conscious separation of meaning. The Indo-Germanic cognate of the word is as follows:

Latin Lithuanian Old Slavonic Middle High German German Swedish SORB-eo[SREB ti | SRUB-ati | SURPF-eln, SURF-eln | SCHLÜRF-en |

If we compare the mass vowels, we immediately notice that the word "schlürfen" is a juxtaposition of the R and L sounds: Latin: RB

SRB Lithuanian; S-

138

===== PAGE 136 =====

"Swedish" S-R-B

SRP in the following: SRF German: S-L-R-F

Kluge explains this L expansion by analogy with the word "schlucken" (to swallow), but we hazard the assumption that the L-sound was introduced into the word "schlürfen" more to distinguish it from the word "schürfen" (to scrape, scratch), precisely because it is a close relative of the R.

Within a language, RL variation almost without exception plays a semantic role: it serves to form either synonyms or nuances of meaning.

E.g. synonyms: Greek KRIB-anos and KLIB-anos (turnace) ARD-o and ALD-o (to sprinkle) KRAD-e and KLAD-e (branch, branching) nuances of meaning; Greek GRAPH-o (write, scratch), GLYPH-o (engrave) Latin SCRIB-o (write), SCULP-o (engrave) German KRAM (lim-lom) and KLAM-otte (waste, scrap) KRAMP-e (clip) and KLAMP-e (pliers, hook, clamp)

The RL transition is also quite common in cognates of different languages. Slavic SEREB-ro (silver), German SILB-er (silver); Greek CHRYS-os (gold) and German GOLD (gold); Afghan VRAZ (flea) and Russian BLOCH-a (flea); Greek LEIR-ion (lily) and Latin LIL-ium (lily); Greek PHRAG-ellon (whip) and Latin FLAG-ellum (whip); Greek CHELID-on (swallow) and Latin HIRUND-o (swallow); Latin PEREGRIN-us (stranger, wanderer) and Italian PELLEGRIN-o (stranger, wanderer).

Satem-kentum variations:

To some extent, this question is related to the already mentioned "Sibboleth" phenomenon known from the Bible (Judges 12:6), as well as the so-called "S-rejection".

"Sibboleth" is nothing more than a slur, a slur. The lack of the S sound can extend to entire peoples and ethnic groups. For example, in Greek and Latin there was no S, and according to the Bible, there was no S either.

In Hungarian, the dialect of the Csángós belongs to this group, who mostly pronounce the S (Hungarian, "sz") sound instead of Š.

But reverse cases also occur; the Alemannic dialects (Vorarlberg, Tyrol) often use Š (sch) instead of the common Pémet S,

139

===== PAGE 137 =====

We can also find in German the projection of the 8-5 transition that connects these sound variations with the satem-kentum variations. The German Š (sch) sound was created — according to the testimony of old linguistic monuments — by merging the SK sound group. In Gothic and 6-High German texts, instead of the modern German, "sch", i.e. S, we regularly find SC (pronounced, "szk"). But this is also confirmed by the Greek and Latin loanwords of the German language, which from the SK (SC) sounds always became (sch) in German. Let us look at the derivatives of schole" (school) in some European Indo-Germanic languages (the Hungarian and Greek, pronunciation is in brackets):

Greek Latin ~~SCOL-o~~ | French English

e | SCOL-a | SCHOOL | SKOL-a | ECOL-e (school) | (school) (school) 8%618((school)

In this example, we can clearly observe the bifurcation of the SK sound group into S and K, but also the lability of the sibilant sounds.

The phenomenon of "S-elövet&s" also points in this direction. We very often find it before K and CH or instead of G in the form SK:

Greek: | SKYT-os (skin) | GRAPH-o (ir) GLYPH-o (carve) CUT-is (skin) SCRIB-o (ir) SCULP-o (carve)

German! HAUT (skin) SCHREIB-en (ir) | SCHLEIF-en (grind, rub)

These sound changes lead us directly to the question of satem-kentum variations, which, as stated above, are actually nothing more than sound transitions between 5, S, and G, K, H.

If we also consider some additional German equivalents of the aforementioned Greek and Latin words, we come across very interesting phenomena:

SCHREIB-en (write, tk, scratchy)

SCHRUBB (scrub, rub)

SCHRAMM (scratching)

On KERB (groove, groove, cut)

GRAB-en (ás)

SKIN SCHLEIF-en (köszörül)

PROTECTION
SCHÜTZ (to protect)
HUT (defense)
| SLAPP-e (Schlag) (ütes)
Knock
(to beat, to strike)
KLEMP-ner (bádogos, tk „ütögető”, „kalapáló”)
KLAP (hit, slap)
HÜT (to protect) 149
===== PAGE 138 =====

That is, the forms satem and kentum can be found simultaneously in words with close semantic connections within a language. The variations showing the 5- projection are also not "regular" intermediate transitions from G or K to S or 5, but phenomena of the kind that can be found in the Indo-Germanic languages. Looking outside the Indo-Germanic languages, we can find in the case of "schlürfen" we have already seen, discover even more instructive connections.

HRT (cut) §-RT (scratch, abrasion); cf. German (Scharte); (scratched) HRB (knife) (scratching); cf. German .] SRF (sharp); cf. German (scharf); (sharp) HRM (to split) §-R-M

Schramme (scratching)
HARAM (left-handed) SARAM-u (cut, trim)
KARÄT-u (raised) SURT-u (knife) HARAP-u (harvest) SIRAP-u (scissors) .
Sumerian:] HAR (to scratch, draw) SAR (to write) GUR, (to cut) SAR, (cut) GUR., (reap) SIR, (cuts off) GIR, (shines) SUR (shines)

Thus, in Semitic languages, and even in Sumerian, we find the phonetic variants "kentum" and "satem" — even within the same languages — for the phonetic distinction of words with close semantic connections or for the formation of synonyms. Even more striking, however, is that the Indo-Germanic, Semitic, and Sumerian words presented undoubtedly show close semantic and phonetic connections with each other. We will discuss this issue in more detail in the next chapter.

Regarding the Sumerian language, we will only note that although modern linguistics considers the transcription of Sumerian words to be very uncertain, and A. Deimel's Sumerian dictionary, from which the above words are derived, is supposedly completely outdated, we cannot agree with this position. From the undeniable Semitic and Indo-Germanic parallels of some Sumerian words we have just learned, it is already clear why these objections are unacceptable to us. In the following, we will have the opportunity to learn about a number of profound Sumerian-Semitic-Indo-Germanic connections that are based not only on the phonetic and semantic similarities of the mass of words, or on far-reaching not only the similarity of the words, but also the multi-order correspondences of certain polysemantic Sumerian root words, as well as the parallel occurrences of synonyms and homonyms.

===== PAGE 139 =====

All these correspondences, between the uncertain Sumerian words . äträsü and their Indo-Germanic-Semitic counterparts, were collected from the supposedly outdated Deimel dictionary.

Returning to the satem-kentum changes, it can be established from the Indo-Germanic examples known so far that here we cannot speak of regular sound changes, much less of satem or kentum "languages", but at best only of tendencies: in some languages there is more S (S), while in others there is more G (K,H). In all Indo-Germanic languages, we can find alternate forms of satem and kentum:

Latin GRAD-us (step)
German SCHRITT (step)
FIVE: ios |HEKYR-os| HEL-ios | HEX | HEPT-a - in: But even within a SOCER |SOL SEX | SEPT-em |SERP-ens (above) (below) |(saint) |(ap6s) (sun) (six) | (seven) / language, satem-kentum variants often occur, partly as cross-linguistic synonyms, and partly again as phonetic distinctions of nuanced meaning,

Synonyms:
D.Charente ESCARAB-ise | ECREB-is
D.Herault KARAB-iso
i.rod.francia | Dep.Basses Pyrenees SKREBis
Significant D.Basses Alpes CHARB (coal) office.france KARB-u : (pronounced:sarb6) (pronounced: karbu)
changes:
SCHNURR-en SCHNARR-en SCHALL (ringing) (squeaks, creaks) (sounds)
KNURR en KNARR-en The HALL
(funny) (squeaks, creaks) (sounds)

===== PAGE 140 =====

(Compare also with the German words mentioned above (page 140): Haut, schreiben, schleifen, etc.)
| Hebrew: HL-HL (scratched) HRB (knife) SLM (drowsiness) SLF (none) SRF (sharp)
(Note the Hebrew-German word similarities that appear by the way!)
KÄK-a KAR-om SÄS KAR-am Hungarian: At CSER-eny ZSIZS-ik Horn

The examples presented clearly prove that the "satem-kentum" sound transition is used semantically by languages, so these are not signs of temporal "sound development".

Nasalization

It is a very common phenomenon. It occurs within a language as well as in related words in different languages.

German
Between languages: Greek latin
BRACH-ion (arm) | BRACCH-ium (arm) | PRANK-e (paw) AGGEL-os (angel) | ANGEL-us (angel| ENGEL (angel)
These examples show that nasalization also plays a role in resolving difficult-to-pronounce consonant clusters. The same is true for languages in which MB, NG, or ND are heard in speech, but BB, GG, and DD are always used in writing; such a language is Swahili, for example.

The inhabitants of the Fiji Islands, however, pronounce the single, undoubted B, G, D as MB-, NG- and ND. Several renowned Sumerologists assume the same in the Sumerian language, in the case of the sounds B, G, D occurring in the middle or at the end of a word. It should also be noted that this sound formation feature is also characteristic of Finno-Ugric languages.

In most cases, nasalized and non-nasalized parallel phonetic words within a language — similarly to the sound changes we have seen so far — also serve to phonetically distinguish words with related meanings formed from the same basic concept and to form word clusters.

143

===== PAGE 141 =====

For example, German:
On BLICK (moment) SCHLEICH-e (cycle) STAK (walks on tiptoe) BLINK (blinks) SCHLANG-e (snake) STANG-e (ore, pose)
On SCHRAG (stand) ACKER (arable land) MATT (mat) on SCHRANK (barrier) ANGER (meadow, lawn) MANT-el (kabát)
| SOCK-en (sock) | SACK-en (silly)ed) SENK-el 4476)
SAG (say) | STAPF (stomping) | SINK (sink) | SING (sing) | STAMPF (stomping)
In Indo-Germanic languages, they also have a grammatical role. On the one hand, in the - verb conjugation:
Greek: MANTH-ano (learns) and e-MATH-on (learned)
Latin: RUMP-o (drum) and RUP-i (thrown); FRANG-o (break) and FRAG-i (broken)
German: DENK-en (think) and DACH-te (thought); BRING-en (bring) and BRACH-te (brought).

On the other hand, in internal word formation:
Greek: LENTH-ano (forget), LETH-e (forgetfulness) Latin: TANG-o (touch), TAC-tus (strike) German: PRANG-en (boast), PRACH-t (pomp).
Nasalization also includes the frequent and important BM, or more precisely, B-,P-,V-,F- M transition.
pl.
Greek: BYRM-ex (ant), Latin; MYRM-ex (ant, worm swarm)
TURB-a (crowd, troop, swarm), TURM-a (cavalry, troop), GLOB-us (bullet), KRAMPF-en (to convulse) GLOM-us (gombolyag)
German: SCHLAF-en (sleep) KLAMPF-en (to connect) SCHLUMM-ern (to sleep) KRÜMM-en (to bend) KLAMM-ern (to connect)
Hebrew: NABH-a (announce) na-SAP (blow) ZRP (flowing)
NAM-a(say) na-SAM (breath) ZRM (flow, flow) HRB (rain) ZBN (time) Hungarian: BÜKK PART DIRT Z-'P (price) 'LP (cover) HRM (cleave) ZMN(id6) Z-'M(anger) 'LM (cover)
POCS-olya MAKK MART MOCS-olya MOCS is

===== PAGE 142 =====

So, in the case of the BM transition, we can mostly determine the occurrence of semantic variants, but to a lesser extent, the formation of synonyms,

Messalhangzölekpás

The attrition of the first vowel of words is considered a very characteristic sound change in the Indo-Germanic languages - especially in the Germanic ones. Similar to the loss of the vowel of 6156 syllables, this attrition can also occur as a result of the stress being shifted back. But not only then. In the case of attritions in the true sense, the vowel is usually first dropped, then the word-initial consonant changes from voiced to voiceless, or becomes increasingly attenuated, becomes breath-like, and finally disappears completely:

latin	ófelínémet	German		
CORV-us	HRAb-an	RAB-e (raven)	(raven)	(raven)

There are also cases where the vowel remains:

German English WURM (worm) | WORM

Swedish

(worm; pronounced: uorm) | ORM-r (worm) WOLF (wolf) | WOLF (wolf; pronounced: uolf) ULF-r (wolf)

At first glance, these examples seem to clearly justify the thesis of "regular sound development" laid down in the sound laws. However, if we apply our complex semantic method here too, and take into account some other Indo-Germanic words that are closely related in meaning and sound form, then a completely different picture emerges.

before our eyes;				
latin	German	English		
LUP-us	WOLF	WOLF (wolf) (wolf) (wolf) VULP-es WELP (smoke) (dog puppy)		

Where did the beautiful "sound development" line from before go? How is it possible that the "Indo-Germanic basic stage" shows the "most worn-out" sound form: LUP-us? The answer to these questions is found in the Greek ALOP-ex and

145

===== PAGE 143 =====

is given by the Latin VULP words. These words make it clear that both full and more or less "worn-out" phonetic words play a semantic role: in the human perspective, they distinguish related animals. This is also proven by the German word WELP, which in turn varies the basic concept with the help of the FP transition. Regarding the CORV word, we have already seen in the chapter dealing with phonetic laws that the parallel Semitic forms would prove exactly the opposite "phonetic development", if such a thing existed at all in the rigid form imagined by linguistics. (See the parallels of the German WURM below.) Knowing the Latin VULP and LUP phonetic variants, we can now better understand the following semantic connection:

English	Swedish	German WORD (word; pronounced: uord) ORD (word) RED-e (speech)		
English LUP-us (wolf; pronounced: uolf)	Swedish ULF-r	latin WOLF	ULF-r	(wolf) (wolf)

Wherever we scratch the polished surface of the seemingly solid scientific edifice of "regular sound developments," a mass of semantic connections buried by one-sided sound laws immediately emerges:

Oind: SRP (snapping), KRM-i (worm)

Zend: RAP (hanging and crawling)

Nose: CRUIM (worm)

Lithuanian: KIRM-ele (worm)

Greek: HERP-o (snake, worm), RHOM-os (snake), KARAB-os (beetle, crab), SKORP-ios (scorpion)?, SKARAB-aios (scarab)

Latin: REP-o (climbing), REP-tilium (sliding),

CRAB-ro (hornet), SERP-o (crawl), SERP-ens (snake)

Gender: RAUP-e (caterpillar), KREB-s (crab), KRABB-e (short-tailed, unarmed crab), KRABB-ein (crawls, crawls), ROBB-en (crawls, crawls)

Francia: ECREV-isse, SKREB-is, ESCARAB-iso, EKREB-is, KARAB-iso (mind „rāk").

Our compilation is far from complete, but it is more than enough to let us note: the "satem" , worn-out" and complete word forms — but the "kentum" and the variants also occur in such a motley mixture in the individual languages that they cannot be classified in their entirety by any rigid vowel-146

===== PAGE 144 =====

cannot be forced into it. Therefore, if we examine this word group using the method of regular sound development, the result can only be that we have to cut their organic unity into several parts, in order to obtain two or three short word sequences in which the Grimm-like sound changes can be detected, and we have to throw aside a considerable part of the words because they do not fit into the scheme.

only be As for the term "tone attrition", in our opinion it would be much more accurate to speak of tone projection in such circumstances, which — as we have seen — plays a primarily semantic role, and can perceived as a tendency over time.

Changes in the consonants "B", "G", "D"

Let us now examine the most important, most frequent, 3161010016 major tone changes, the transitions of the sounds B, D, G into the corresponding, related articulation sounds. It will be instructive if we start from the tone division of Indian grammarians in the analysis of this question:

labials: B, BH, P, PH, M

dentals: D, DH, T, TH, N

velars: G, GH, K, KH, N (Ng)

Therefore, the Indics assigned a nasal to each group. Taking into account that V and U can be formed from BH, F from PH, H and J from GH, and the hard H usually marked CH from KH, DH, TH can go from 2-06, 5-06, the complete sound sequences are as follows:

labials: B, BH, V, P, PHF MU

dentális: D, DH, T, TH, Z, 5, N

veláris: G, GH, K, KH, CH/H) H, N, J (Ng)

Two branches can be observed in the velars. On the one hand, there is a transition to the labio-velar sounds Qu, Gu, and on the other hand, through J-ization, the formation of palatalized velars, which lead to the sounds C, DZ and S, SK. These latter sound changes ultimately explain the kentum-satem transitions as well.

The sound changes that run through the entire cognate sound scale, up to the disappearance of the consonant yyy- 16868, can be clearly observed in the Indo-Germanic cognate of the German word "Wurm" (worm);

Greek	Russian
BYRM-exPREMF [FORM-ica MURAV-jej] WURM WORMJORM-r (ant) (worm) (ant) (ant) (worm) (worm) (worm)	
147	

===== PAGE 145 =====

Looking at these words superficially, we might think of "regular" sound changes here too, but with a more thorough study, we can also find in each language those different sound variations which, similarly to the above-described connections of "krabbeln", also serve to distinguish different types of worms, beetles, and reptiles.

E.g. Greek: BYRM-ex (ant), MYRM-ex (ant, worm swarm) Latin: FORM-ica (ant), VERM-is (worm), PARV-us (crooked, twisted) German: WURM (worm), BREM-se (bug) Russian: MURAV-jej (ant), VERM-ije (insects).

The following words also exhibit the same semantically applied sound changes, some of which we have already learned about when discussing sound laws:

Greek: ARK-ys (web), ARACH-ne (spider), ERG-on (weave, spin)

latin: BULL-io (bubbly, hot, waves), BAL-neo (bathes), PAL-us (swamp, lake), PL-uo (it rains), FL-uo (flows)

Old High German: GURT-i (belt), KRATT-o (basket), CHREZZ-o (cart), HURD (pen, wattle)

German: BLITZ-en (lightning), PLÖTZ-ich (sudden), FLITZ-en (slide); BLATT (leaf), PLATT (flat), FLAD-en (pie); GELL-en (whistle), KEHL-e (larynx), HALL-en (sound); FLIEG-en (fly), FLIEH-en (run), FLACK-ern (flare, flash)

In these examples, we do not even find traces of any regular sound changes occurring over time.

The semantic projection of consonant changes can also be clearly observed in the German words HAU (to hit, to strike), HIEB (to strike, to strike), HUF (to hoof), which are associated with the following words with identical meanings and parallel phonemes:

Slavic: KOP-ti (tap, hit) and KOP-to (hoof), Greek: KOP-to (tap, hit) and HOP-ie (hoof).

However, with these Indo-Germanic words we have not yet exhausted all the correspondences between HAU and HUF. Further research will only bring us to the striking phenomenon that has been observed several times before, namely that we find close phonetic parallels of Indo-Germanic "ancestors" in words with the same meaning in other language families:

148

===== PAGE 146 =====

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Hebrew: AKAP (mark) and "AKAB (hoof) Akkadian: HAP-u (thrust, stab) and IKB-u (heel, hoof) Sumerian: HAB, (knock, strike) and KUB, (hit, beat, knock).

If we want to understand the deeper connections between languages, we cannot be satisfied with comparing individual words from a few languages, taken from their semantic families, but we must compare all the phonetically and semantically identical or largely similar word variations in each language. This can then be followed by the separation of possible homonyms, synonyms or coincidental coincidences, and finally by the development of related word clusters. This is the

The combined, semantic-phonetic method is much more thorough and comprehensive than the isolated study of so-called "regular sound changes". With its help, we can obtain a much clearer picture of the real connections between languages. Let us look at an example.

Indo-Germanic linguistics considers the Greek word "molybdos" (lead) to be isolated, having come into Greek from an unknown, non-Indo-Germanic language. = The Latin "plumbum" (lead) would be a Greek loanword, and the German "Blei" (lead) is supposedly not related to the Greek-Latin words, but is a derivative of the word "blau" (blue). "Wie die bleiernen Kugeln ‚blaue Bohnen‘ heissen, so ist Blei ‚das blaue Metall‘," writes Kluge's etymological dictionary. (In Hungarian: As lead balls are called "blue beans", so is lead "the blue metal")

The Indo-Germanic equivalents of the word "lead" look like this:

Greek Latin Ogermān | Middle High German | German | Slavic MOLYB-dos | PLUMB-um | BLIW-a |BLI.genit.BLIW-es | BLEI |OLOV-o

In other words, these apparently related words are torn apart because the regular sound changes laid down in the sound laws cannot be demonstrated in them. Based on the same considerations, they also consider the connection between the Greek "morphē" (shape, form) and the Latin "forma" (e.g.) to be very doubtful, at most they suspect metathesis — which in this case would mean a complete reversal of the word — because here too the sound differences do not conform to the requirements of Grimm's laws.

b But this is also the case with several other Greek and Latin words with similar phonetic forms.

(form); MYRM-ex (worm-thirst)	Latin; FORM-a (form) MOLYB-dos (lead) Greek: MORPH-e	PLUMB-um (lead) MYRM-idon (fright)	FORM-time (fright)
	VERM-is (worm) BYRM-ex (ant) 149	FORM-ica (ant)	

===== PAGE 147 =====

The Greek M sometimes corresponds to y, sometimes to P, or even to V in Latin. We cannot therefore observe any rigid regularity of sound development, but we can only state that in Latin the initial labial sounds, compared to the Greek, took on more blurred, voiceless sound forms in all the words presented.

That is, linguistic facts only allow us to speak of a tendency towards weaker articulation in the case of labial sounds in Latin.

Here we must make a small digression. The words "molyb- dos", "plumbum" and "morphē", "forma" are of particular interest, as are their more distant connections. All signs point to the fact that they can ultimately be traced back to the Sumerian word "alam" (carved column, figure, form, statue), which also occurs in the form "ba'alam". The correspondence of this with the Hungarian word "bálvány", which also has the meaning of "gate-bálvány", is also considered by the faculty of linguistics to be Elven - of course only as a foreign or vandorsze. The Greco-Latin forms showing the R sound are the Sumerian others, as nearly synonymous LR variations.

According to the evidence of the words "bar" (build, create, form, make, shape, form) and "amar," (shape, form) also belong here and are not

In the case of the words "molybdos", "plumbum", "Blei", "olovo", the connection with "alam", "ba'alam" cannot be directly demonstrated, because 6 Sumerian words cannot be documented with meaning. There are several compelling reasons why we nevertheless draw a parallel between these words. First: The Hungarian words ÓLOM, Vogul WOLEM (lead), Östjåk OLN3 (lead), CheremisZ BULN-3 (lead), which — despite the fact that Benky's historical-etymological dictionary considers to be the results of an independent, independent development from the Slavic word OLOV-o (lead) — cannot, in our opinion, be separated from the Indo-Germanic words "Blei" (lead)- 161. This would be a bit much from the random ones, if only because of the additional connections to be explained below. These parallel-sounding Indo-Germanic-Finno-Ugric names clearly testify that both language families drew their interpretation from a common ancient source. It is based on a Neo-Assyrian root, where Sumerian ANNA = Assyrian ANAKU (lead). The Assyrian word dates back to the 1st millennium BC without further ado, to the 3rd millennium, because on the one hand it is well known that many languages do not distinguish between tin and lead, and on the other hand according to the original Sumerian words from words meaning "lead". Secondly: The "lead" The Sumerian name given by Deimel, ANNA (Yv. NAGGA, NIGGI), is demonstrably incorrect the Ilird dynasty of Ur, dating back to 2100 BC, in addition to copper, the metals AN-NA and SU/G- GAN (antimony?) were used to produce bronze. In a bilingual hymn to the fire god GIBIL, where However, the meaning of "lead" cannot be projected the production of bronze is specifically discussed, it reads as follows:

150

===== PAGE 148 =====

anna" — But lead was never used to make "mix together" Akkadian text: "sa eri u anaki" — Sumerian text: "urudu you mix it up", bronze, but tin was. According to the Sumerian, we don't know. "anna" is not "lead", but "Zinn", i.e. "tin". The original Sumerian meaning of lead is given by these

Thirdly: The use of small human-shaped idols cast from lead was widespread throughout East Asia — even during the Chalcolithic period. Countless such small figures have been found, which, according to the evidence of grave finds, were worn hanging around the neck. (Gertner, Gropengiesser, Wagner: Blei und Silber in ágaischen Raum.) The first metal objects, one might say mass-produced, were therefore made from lead, which could be easily and quickly cast, the small human-shaped idols and amulets. The name of the material is very often transferred to the characteristic object made from it, especially if the object is new. But even today, with several millennia of metalworking traditions, we simply mention gold and silver instead of gold and silver coins, we call a brass band that uses musical instruments made of copper a brass band, we call a knife-sharpening tool made of steel steel, we "iron" clothes, but also horses, and even - precisely our word "statue" in question, which is not some kind of South Slavic word meaning "fence, lath, stand" - as our linguists teach - but belongs to the meaning of "bronze" itself: Sumerian ZABAR = "Bronze" (bronze). This opinion is also confirmed by the fact that the "lead" = "idor" analogy — we are talking about bronzes when it comes to smaller statuettes. Here, but directly the meanings "Spiegel" (mirror) semantic analogy of "characteristic composition" with "material" = can already be detected in Sumerian linguistic monuments, including the word ZABAR, which also has and "Keule" (mace), because these objects were usually made of bronze among the Sumerians. This analogy is also reflected in the international word ARMOUR Sumerian BAR-ZIL, Akkadian PARZILL-u = "Ei-, armored Knowing these linguistic connections, one can only smile at Kluge's efforts, who, in the background of the unlikely agreement between the German sen", i.e. , iron", (cf. the Hungarian word "vasas" = Panzer" with the Latin, is again hidden by the desire to have an "Indo-Germanic basic degree" in the German Z sound according to the laws of sound.

It derives from "pantex", which means "Wanst, Bauch", i.e. belly". He argues that the armor covers the belly, or figuratively the "body" protects. This

They are trying to show it.

Fourthly; The international word AMUL-ett, whose ultimate source is unknown, can also be traced back to the Sumerian phonemes ALAM, BA'ALAM, and is largely identical in meaning to these Sumerian words. Fifthly; The Sumerian word ALUM, which has a parallel phoneme, has a present-151

===== PAGE 149 =====

tése "Oven for firing bricks and melting metal", "melt" is our word points to its connection with "lead", namely that (English: "furnace for brick-making and metal smelting") These are 4 Sumerian words the "lead" may originally have meant "molten", "fused". In this connection we also refer to the striking parallel: which in turn is appears between its names and another Sumerian word, also meaning "Ofen" (stove, oven): 4 ón" Indo-German This question requires further investigation. Based on the above considerations, we can say with almost certainty that the words meaning "lead" in the Indo-Germanic and Hungarian-Finnish languages can be traced back to the Sumerian language.

At the same time — we hope — we have also managed to briefly and schematically present how the complex semantic-phonetic method can be applied in language comparison studies.

Crosstalk, polarization

The transition from one articulation group to the consonants formed in the adjacent position is called a sound crossing. For example, from velar = to dental: GD or KT.

Such sound changes are common in Oceanic languages, but they are also not absent in Europe. In English, for example, CL is often pronounced as TL, and GL as DL: CLEAR is pronounced as TLEAR, and GLORY is pronounced as DLORY instead of GLORY. Transitions from dentals to labials are found in Russian, where the foreign initial TH in words regularly becomes an F sound: THOMAS (TAMAS)-FOMA (Thames), THYMIAN-FIMIAn (thyme). The Russian name for the Greek letter THETA itself is FITA. se Polarization is when labials formed at the very front of the mouth alternate with velar sounds formed at the very back. In comparative linguistic studies, 15 the phenomena of crossing and polarization are taken into account. Without further ado, they combine e.g. The words GHARM (warm) in the Old Indic language, the Greek THERM (warm) and the German WARM (warm), in which the alternation of G, TH, W is considered to be regular sound 8 Indo-Germanic studies are transitions. We do not want to argue that sometimes such extreme sound changes occur. However, in most similar cases it can be proven that they are ancient synonyms and not sound transitions. We very often encounter them in the naming of a basic concept.

152

===== PAGE 150 =====

with words with parallel meanings, the phonemes of which differ only in their initial consonants, while the other sounds are largely the same. These initial sounds belong to two, and not infrequently even three, different articulation circles. We have already seen such an example in the Indo-Germanic worm names, where one word family is built on the consonant skeleton "krm" (kr- b) and the other on the consonant skeleton "brm" (frn, wrm). PL: Latin VERM-is (worm) and CRAB-ro (horse homet). To these we can now add the Latin word TERM-is (wasp) as a representative of the third articulation circle. Similarly, the root words of the nouns "curving, twisting, bustling, turning" can also be traced back to all three articulation circles: Latin PRAV-us (curved, twisted), FERV-eo (bustling, bubbling, boiling), CURV-o (curves, bends, bends), TURB-o (rotates, turns). We can also find synonyms of the same type in Semitic languages:

Akkadian: BARAM-u (turn), BURB-ilati (locust chops) HAR-ú (to dig, turn), HARB-u (reversible plow), HARUB-u (a type of insect) SÉR-u (to bend), SÉR-u (snake), ZÁR-u (to curve), ZIRB-abu (ant), TAR-u (to turn), TUR-zu, TARM-azu (a large butterfly)

All of these Indo-Germanic and Semitic words can ultimately be traced back to the following Sumerian words:

BAR (bows), BARUN (ant)

BURU. (curvature), BUR,-hu (a type of locust)

MAR (a type of worm), MARUN (ant), MER (scorpion)

(cf. Sumerian: BARUN and MARUN; Greek: BYRM-ex and MYRM-e)

xGAR (bow), GUR (rotate), GIR, (scorpion)

HAR (curves, twists), HARUB (a type of insect)

SUR (bends, bends), SURIN (cricket), SURIN, (a type of ant)

DAR (rotate, roll, turn), TAR (bend, spike)
Because of such connections, it is not advisable to mix up variations of words with the same or very close meaning that begin with consonants with different articulations, but otherwise have parallel phonemes, because in most cases this will only cause confusion. It is much more expedient to examine such variations separately, grouped according to the individual articulation circles. Thus, in the case of the aforementioned Indo-Germanic agreement (GHARM-a, THERM-os, WARM; Hungarian: "meleg" (warm)), we are in all probability also dealing with synonyms;

Sumerian BAR (the burning of fire) — (cf. Hungarian PAR-ázs) ý BIL (sky, it will be warm) ~ (v.ð. Hungarian FOL, FOL-ik) MEL (sky, it will be warm), MELAM (light, shine) - (v.ð. Hungarian MEL-eg, LIGHTNING) 153
===== PAGE 151 =====

GAR (light, fire), GIR, (stove), GIRIM (fiery shining) ~ (compare Hungarian GYER-tya)
GIL, (collect) — (compare Hungarian GYUL-ik)
DAL (hot, fiery, rich in embers), DILIM, (stove, furnace) - (compare Hungarian DEL)
Metathesis, the reversal of syllables

When reviewing the semantic role of vocalization and consonant changes, we briefly pointed out that linguistics often assumes voice change even when it is actually just a matter of vowel drop or a semantic consonant transition.
In our opinion, we can hardly speak of vowel shifts, because the DURCH - THROUGH variants are in all cases the result of stress shifts, not sound shifts.

However, metathesis of consonants occurs quite frequently in languages. Apart from purely phonetic reasons, such as slips of the tongue - the German WESPE (wasp) and WEPSE (wasp) - or faulty articulation, we can only consider real metathesis to be cases where the stems are reversed.

E.g. Hebrew Greek upper class German

SKEP-os SCOUW-on | SCHAU-en | §-KP

(look) (look) (look) (look)

Latin: SPEC-o SPEH-a SPH (see)

SPÄH-en

(scan)

(scan)

(scan)

(The "S-drop" does not belong to the stem, but is the result of stem expansion.)

However, the following word pairs are not true metatheses:

Greek Latin (step) (go) German Hebrew SKIRT-ao | GRAD-us SCREAM | JRD (Ugric) (step)

down)

OVERCOME-o | TRAC-tus

STRETCH | WOOD (megy)

(vowel read)

(battle section)

(out)

154

===== PAGE 152 =====

These apparent metatheses - which in this case would mean complete mirror translations of the words - are related to the previously mentioned phenomenon that in languages we often find synonyms corresponding to the three main articulation circles (B, D, G).

In the above words we are dealing with the synonymous meanings of the Sumerian GIR, (foot, step, road, go) and DIR, DIRIG (go, march, road, drive, drive, pass). The appearance of metathesis is given by the fact that in the case of GRAD-us and its cognates we are faced with the expanded form of the base GIR formed with the auxiliary DU, (do, do): GIR, -DU, = , to take a step". Similarly, the ancestor of the TRAC-tus-like variants is the expanded form of DIR (6526 AG (do, do) with the auxiliary, DIR-AG, which is a high-pitched variant corresponding to the Sumerian vowel harmony.

. in fact, it occurs in the Sumerian language itself: DIRIG = True metatheses to make a path, to walk a path". We will deal with this question in more detail in the next chapter, therefore arise only through the inversion of monosyllabic roots.

E.g. Latin: PEC-us (sheep, cattle) and CAP-ella (goat) VACC-a (cow) and CAB-allus (16) FUG-o (drive out) and CAP-io (catch)

German: BOCK and HAB-er (goat)

PACK-en (seize) and KAP-er (confiscate, pirate) SCHIFF (ship) and FISCH (fish)

Hungarian: KAP and FOG MAG and GUM-ó BOG and GOB

Sumerian: BAG (to grab) and GAB (to get, to receive) BAD (to open) and DAB (to close) GUM, (to tear) and MUG (to tear).

As can be seen from these examples, metatheses are primarily semantic in nature: partly synonymous, partly new, sometimes similar, sometimes with exactly the opposite meaning.

are used to form words,

From the point of view of a better understanding of the formation and development of languages, the last mentioned cases are particularly important, when the meaning of a word changes to the opposite. They also testify that man has been consciously and rationally shaping and forming his language since the most ancient times. The cases of various sound changes known so far, which play a predominantly semantic role, prove the same thing: sounds with related articulation were not used by "languages", nor by some kind of "linguistic spirit", but by the people themselves who formed and further developed the languages

~ logically -- the related concepts

155

===== PAGE 153 =====

This is the simple and reasonable explanation of the sound changes, nothing else.

Finally, we should briefly mention a circumstance in the history of writing, which most likely also played a role in the emergence of certain phenomena similar to metathesis. It is known that the Sumerian and Akkadian cuneiform scripts proceeded from left to right, while the West Semitic scripts that developed in the 2nd millennium BC proceeded from right to left, or more precisely, initially in alternating directions from line to line, "like an ox-plough" (bustrophaedon). As a result, when copying and transcribing cuneiform texts, errors in syllable order and misspellings could often occur; all the more so because it is known that we often encounter such syllable changes in Sumerian texts as well. The word LU.-GAL (king), for example, was very often written in the order GAL-LU.. Such syllable-change errors may also be the result of certain forms of S-deletions, notably in word forms such as the German SPUR (trace), but PIRSCH (investigate) or FERS-e (heel), but SPOR (spur). These questions it would be worth examining carefully.

Summary

This brief overview of the most important sound changes clearly proves that in language development, neither the changes of vowels nor of cognate consonants can be interpreted as a temporal process, but they must be evaluated as semantic phenomena that serve grammatical purposes on the one hand, and on the other hand, form words with parallel, cognate meanings, and are applied with conscious rationality. This recognition inevitably follows from the fact that the so-called "cognate" method, which has been used in comparative linguistics until now, has been replaced by the "cognate" method. The etymological method based on "regular" sound changes is not sufficient for a comprehensive examination of the real relationships between languages. This method breaks down semantically closely related word families into their parts, and can only grasp and compare a narrow aspect of each. A new, complex method of linguistic research is therefore needed, which not only pays attention to temporality, but also primarily seeks to what extent the meanings of words with identical or very similar phonetic forms correspond to each other in the individual languages. With the help of such a combined, semantic-phonetic method, it becomes possible to examine word groups that are closely related in their meanings more thoroughly, but also to better distinguish homonyms and homophones that are identical or very similar in phonetic form. However, truly complex etymological studies can only be carried out in the light of our current knowledge of history, culture and language history if we do not remain within the framework established in the 19th century. tongue twister-156

===== PAGE 154 =====

We are not only trying to explore the linguistic aspects of the archaeological finds described in the first chapter, but also within the limits of the West Eurasian language families -- Indo-Germanic, Semitic, and Magyar-Finnic languages -- we are therefore including in our investigations the only satisfactorily known language, Sumerian. We are aware that we are only at the very beginning of research in this direction. We do not believe that we are infallible. However, by applying the combined semantic-phonetic method outlined above, we have already found such surprising and unexpected mass correspondences between the Sumerian language and the languages of the three West Eurasian language families mentioned above that they cannot be accidental. These mass parallels have convinced us that we are on the right track.

In the next chapter, we will provide a preliminary overview of the similarities, often down to the smallest details, found between the Semitic, Indo-Germanic, and Sumerian languages.

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157

===== PAGE 155 =====

Chapter 5

SUMERIAN COLUMNS OF INDOGERMAN AND SEMITIC LANGUAGES

The idea of possible connections between the Indo-Germanic, Semitic and Sumerian languages arose in our minds when, in view of the new results of archaeology on Proto-Asiatic, European and West Asian cultures (I. in Chapter I), the decisive role of the Neolithic, Chalcolithic and Early Bronze Age Proto-Asiatic civilizations in the development of the culture and civilization of the whole of Western Eurasia became clear to us. The Pre-Asiatic influences, emanations and even mass colonization that can be detected archaeologically almost everywhere, in many places,

In light of these facts, we could not accept the linguistic conclusion that the Sumerian language was an insignificant island language, and that the people themselves, according to historians, were completely absorbed into the incoming Semitic waves as early as the Old Babylonian period.

This convenient position is not convincing, not least because the Sumerian people around 2000 BC simply could not have been a negligible number. The Sumerians — even according to the most cautious chronological calculations, the so-called short chronology — lived and ruled in Mesopotamia from about 3600 to 2000 BC. Their number could not have been negligible originally, because a few hundred, or even a few thousand people, would not have been able to carry out the gigantic river regulation and canalization works, the existence of which can be archaeologically documented as early as the middle of the 4th millennium BC. Let us also consider in this connection that the Tigris and the Euphrates are rivers of the same size as the Danube! In our opinion, this is not a trivial matter that can be brushed aside with a superficial wave of the hand, as is done in history and linguistics.

Even if we — very, very cautiously — put the number of Sumerians who immigrated around 3600 at only 20,000, then this population in 2000 BC — assuming an average doubling every 200 years — represented more than 5,000,000 people (see also Chapter 1, pages 45-46). Such a mass could not have been absorbed in a short 2-300 years. We emphasize that our calculation example starts from the lowest possible number of people, because, for example, in Uruk alone, archaeology has already revealed monumental constructions around 3400-3300 BC that could not have been realized by just a few thousand people, in their little free time, in addition to farming and the continuous, recurring maintenance work that was a matter of survival. In addition, the excavation layers also showed that a significant part of the temples, palaces, and houses generally had to be rebuilt every 40-50 years, because the unfired, sun-dried clay bricks crumbled over time.

We can see, previous calculation
example-158
===== PAGE 156 =====

We certainly did not exaggerate in our calculations, but on the contrary, we assumed a much lower number of immigrant Sumerians than they actually were - according to all available data. These considerations do not exactly lead us to conclude that the Sumerian people were a negligible, small number of people.

Linguists who easily dismiss the Sumerian language as an insignificant island language do not take into account the eminently important fact - and one that can be documented thousands of times over - that this language played a dominant role in the whole of East Asia for more than 1500 years, both in the fields of culture, science, religious cults, literature, trade relations, and as the central, imperial language of various organized state formations. In every excavated ancient city of the Near East - from Asia Minor to Persia - in addition to the monuments of the local languages, clay tablets in the Sumerian language are also found in large numbers. We have already seen that we even know of such ones from Transylvania and Tartaryka. Moreover, even after the final cessation of the Sumerian political hegemony, for another millennium and a half, until the middle of the 1st millennium BC, the language of religious cult and science remained Sumerian; exactly the same as Latin in medieval Europe. In connection with this fact, we must definitely recall the very significant linguistic influences that Latin had on the languages of today's European languages. Why would it have been different in the case of the Sumerian language? Especially when we consider that the difference in cultural status between Sumerian culture and the surrounding barbarian areas was much greater than between the Roman Empire and other regions of Europe at that time, and that Latin linguistic influences only affected larger areas for about 500 years, while the Sumerian language had a continuous influence for 1700 years,

Based on these considerations, we can only come to one conclusion: under such circumstances, speaking of Sumerian as an insignificant island language means a very high degree of disregard for historical facts that can be proven by contemporary written and other archaeological and cultural-historical monuments.

From what has been said, we take the principled position that extensive, thorough comparative linguistic studies, including the relationships between individual language families, can only be successfully carried out, in addition to our current knowledge of cultural history, by including the ancient Proto-Asiatic languages, and within these primarily the fairly well-known Sumerian language. We are firmly convinced that such an experiment would not be completely futile even if it did not lead to any positive results. In this case, we could at least gain enough to state firmly that this path is untenable and we must continue paleolinguistic research in a different direction.

The linguistic objections raised so far are by no means the results of objective, detailed, conscientious research.

159
===== PAGE 157 =====

but in all cases, without exception, they stem from a rigid, temporal understanding of sound laws and regular sound changes that is not proven by anything and - so to speak, without any closer examination = they a priori deny any connection between Sumerian and the West Eurasian languages spoken today; they are only willing to acknowledge the Sumerian origin of a few so-called "international loanwords". However, such a sterile denial cannot be considered evidence, especially since experience has taught us not to accept any "statement" until we have verified its real significance in practice.

So, after revealing the unfoundedness of the objections, we will examine in practice whether there are connections between the Sumerian, Indo-Germanic, Semitic, and Hungarian-Finnish languages.

In our research we used the work of A. Deimel entitled "Sumerisches Lexikon". In the previous chapter we have already touched upon the doubts about the uncertainty of the transcription of Sumerian words and the obsolescence of Deimel's work. Nevertheless, we relied on Deimel because, on the one hand, we believe that these controversial questions should also be answered by experience, and on the other hand, we still do not have a better work than Deimel's work. We leave it to the reader to decide, after studying this chapter, how uncertain the Sumerian transcriptions are and how obsolete Deimel's Sumerian lexicon is.

Our research was originally primarily aimed at exploring possible parallels between Sumerian and Hungarian. We only came into closer contact with Indo-Germanic languages secondarily, through the question of the countless loanwords of the Hungarian language from Slavic, German, Latin, Greek, etc.

It has been noticed that these loanwords almost always have Sumerian parallels, while in the Indo-Germanic context they very often occur only in the Indo-Germanic language — or at most in the language group — from which Hungarian is said to have taken them. 616 This observation applies mainly to our Slavic loanwords, but it can also be very often observed in Hungarian words derived from the Bavarian-Austrian regional language.

Under such circumstances, we had to necessarily extend our investigations to the Indo-Germanic languages. First of all, we tried to get an answer to how the Indo-Germanic language family developed, and what kind of support and evidence there is for Grimm's sound laws, as well as the linguistic axiom of regular sound changes derived from them. The validity of these "regular sound changes" was simply not acceptable from the outset, following from fundamental logical considerations.

The thesis of the different laws of sound development in different languages, if this assumption is carried out reasonably, to its final conclusion,

160
===== PAGE 158 =====

If we think about it, it can only lead to one conclusion: that each ancient tribe must have had its own "linguistic" god, or, if you like, demiurge, who - for reasons inscrutable to us - nevertheless, for millennia, according to different rules, they controlled - and even still control - the phonological development of individual languages. We refer such demons to the realm of occultism, because we are convinced that they have no place in scientific linguistics.

In addition to these theoretical considerations, however, in Chapter 3 we were able to learn about several very strong concrete, practical-linguistic objections to Grimm's laws and the linguistic dogma of regular phonetic development.

As we saw in Chapter 2, the 19th century theory of an independent, common Indo-Germanic language family cannot be proven, and today Indo-Germanic studies itself — or rather, a considerable proportion of its authoritative representatives — does not believe it either. Most people evaluate the Indo-Germanic languages merely as a grammatical and linguistic community. They attribute a primary role in their development to Semitic, Finno-Ugric and certain Caucasian languages. According to the most widespread view, the Indo-Germanic languages developed through complicated processes of ethnic and linguistic mixing in the contact zones of these language families. So, in the case of the Indo-Germanic languages, we would actually only be talking about areal language associations. We would like to point out at the outset that our own studies also point in this direction,

In the case of the Semitic languages, it is not necessary to prove that they were subject to very profound Proto-Asiatic — primarily Sumerian — influences from the very beginning of their development. This is a well-known fact. The massive similarities in the vocabulary of the Sumerian and Semitic languages are evident as clearly as day in all Sumerian, Akkadian, or Assyro-Babylonian linguistic monuments. We cannot expect anything else, since the peoples speaking Semitic languages have lived near Mesopotamia since ancient times and have taken over their later culture entirely from the Sumerians, and even very large-scale repeated Sumerian-Semitic intermixing took place in the Mesopotamian region. On the other hand, linguistic research in recent decades has shown that the Semitic languages participated to a very significant extent in the formation of the Indo-Germanic language family. According to the findings of Indo-Germanic studies, the most profound and organic connections can be demonstrated with the Semitic languages of the above-mentioned language families. From all these linguistic, historical and cultural-historical insights, the idea almost involuntarily follows that the Sumerian, Semitic, Indo-Germanic, Finno-Ugric and Caucasian languages must have originally been related to each other in some way. First of all,

161
===== PAGE 159 =====

Therefore, we considered it necessary to examine whether such global net. vi connections can really be detected, and if so, to what extent.

We do not have the opportunity to give a detailed account of the results of our observations in this direction so far within the framework of this study, therefore we will limit ourselves to a brief, sketchy discussion of the most characteristic and striking Sumerian parallels found in Indo-Germanic and Semitic languages, and we will refrain from including Caucasian languages this time. The Hungarian and other Finno-Ugric-Sumerian connections will be presented separately in the following chapters.

First, let's get to know a few dozen simple lexical correspondences per language, first between Indo-Germanic-Semitic, then Indo-Germanic-Sumerian languages, and finally we will present triple parallels from words common to Semitic, Indo-Germanic, and Sumerian languages.

Greek-Hebrew word parallels

Greek	Hebrew 1. BOM (altar)	BAM-a (altar) 2. HEDON-e (joy)	"EDEN (joy) 3. OTHON-e (canvas)
ETHUN (linen) 4. OIN (wine)	JAJIN (wine) 5. AGEIR-o (collect)	AGAR (collect) 6. AGOR-azo (buy)	AGAR (buy, rent)
7. AUL-e (dwelling, yard)	OHEL (dwelling, tent) 8. HARP-e (sickle, sword)	HEREB (sword) 9. ARACH-ne (spider)	ARAG (weaving) 10. KYBER-nao
(to rule, govern)	KEBIR (governor) 11. AGAP-ao (love)	" AGAB (love) 12. SKEN-e (tent, dwelling)	SAKAN (inhabitants) 13. GE, GAIA
	GE, GAIA (valley bottom, district) 14. MISG-o (mixed) soil)	TALES (mix) 15. MAZ-a (barley bread)	(earth, MAS-a, MAZ-a

(unleavened bread) 16. MYZ-o (sopik) MUZ, MIZ (sopik) 17. PYRT-anis (main man) PERAZ-on (leader) 18. PLEK-o (spinning, spinning)
SLATE (spindle, circle) 19. PLAT-ys (flat, plane) PALAS (smooth, level) 20. TYP to (hit) TAP-ap (beat) 21. TYMP-anon (hand drum)
TOP (hand drum) 22. KOLAP-to (beat) HAT (axe, hit) 23. GLYPH-o (cut) GALAB (cuts) 24. CHALYP s (steel) CHALIP
(knife) 25. STRAP to (lightning) SEREP-a (fire, sky) 26. SPHAG ion (bloody sacrifice) ZEB/H/ACH (bloody sacrifice)
27. OR-os (mountain)
162
HOR (mountain)
===== PAGE 160 ===== 28,
29. 30. 31. 33. 34.
35.
37. 38. 39. 40. Sub. 42. 44.
45. 46. 47. 48. 49. 50. 51. 52. 53. 54. 55. 56. 57. 59. 60. 61. 62. 63.
64. 65. 66. 67. 68. 69. 70, 12 CHESS (bags)

KLOB-os (four)
OZ-os (4g)
AITH-o (dead) \
. PYR (fire)
KAI-o (me)
PATASS-o (it)
KEL-eos (flaming) KEL-os (the rush) KEL-is (shame) KEL-on (missiles) KAL-ais (sail) KYLIX (cup) GENN-ao (creates, makes) KAN-on (throw, stick)
. PHALANX (section, squad)
PELAG-os (sea) PLAX (field) PLEG-e (blow, wound) PHYK-os (face paint) THERM-os (heat) SKORP-ios (scorpion) KARP-oo (pick, tear) EREB-os (darkness) SKEPT-ron (staff, scepter) SKERIP-to (lean) SKOP-eo (look) SKAPH-os (basin, ship) KYPELL-on (cup) SKYL-on (booty)
SKOL-ios (curve)
SKOP-elos (rock, cliff) XYPH-os (sword) SKOLOP-os (sharp stake) SMYCH-o (burn) MACHAIRA (knife, sword) MACH-ia (battle, clash) LAMPAD (torch) GAM-eo (marry)
AIET-os (sas)
ORN-is (bird) TAUR-os (bull)
SYRIGG-a (ploughman's flute) SCHOL-e (school, teaching)
CHESS (bag)
CAGE (cage)
THIS (branch, tree)
UD, AUD (firewood) BE"ER-a (fire, burning) KAU-a, KI (burn)
HOOF (hits)
KAL-a' (I, pirft)
KAL (fast)
KAL-ah (scold)
KAL-a' (slings6) KAL-e' (sail, curtain) KALACH-at (cauldron, pot) KAN-a (create, manufacture) KAN-a (reed, measuring rod)
PHELAGG (section, squad)
PELEG (stream, river) PALACH (plough) PALAG (scatter) PHUK (eye paint) DEROM (noon) * AKREB (scorpion) CHARAP (pick, tear) *EREB (evening) SCHEBET (stick, scepter)
SCHARB-it (stick, branch) SAKAP (look) SAP (cup, goblet) SEPEL (cup)
With SCHAL (loot, plunder)
*AKAL (görbüt)
SHAKAP (rock cliff) SEPH-a (sword) (üjh&ber) SCHALAP (knife)
SAMAK (to dry up, to fall asleep) MECHEIRA (weapon) MACH-a (it)
PANTS (fäkiya)
CHAM (apous)
*AIT (bird of prey) ERN-u (eagle) (Assyrian)
SOR, hrämi: TAUR (bull, cattle)
SERIK-a (furułajáték) SACHAL (kioktat) 163 =====

PAGE 161 ===== ý ý ýý ý 1 ýýýý
+ 10, 11, 12, 164 HOD-
os (üt)

. NOS (disease)
HETAIR-os (tars) KYKL-os (circle) ARRAB-on (zálog) PHLOG-eos (shining)
. OPII-is (snake)
BOTYR-ys (grape, grape-picked) ORPH-anos (robbed, deprived)
. PHYL-e (strain)
PHAG-o (eat, chew) PALLAK-is (bed)
. AGR (arable land) . KORAB (raven)
KOR-euo (shout, roar) KILL-o (bubble, roar)
. SHIRT-if (arany)
CHITON (tunic)
. ALEIP-a (fat, oil) . HERP-es (snake) . BOKAR-os (spring)
KARPAS (fabric, canvas)
. BLAX (flabby, flaccid) . PHRAG-ma (wall) . BYSS-os (linen, linen)
KAD (jug)
. DEP (cup) . KORYMB (hairpin) . SIPHAR (sail)
*AD-a (step, you step)
NASAS (patient)
KATAR (company, cooperative) *AGAL (circulates)
*ARAB-on (pledge)
BALAG (shines)
APH-a'a (vipera)
BATZAR (picks grapes, harvests)
" *ERV-a (reveal, nudity, deprive)

PALA' (to split, to separate) PAG (jaw) PILLEG-eS (bed) IKKER (greengrocer) *OREB (raven)
KAR-a (shouts)
GALAL (bubbling, seething) CHARUS (gold) CHITON (tunic) * CHELEB (fat)
" *ROB (worm, insect) BOKER (early time, morning) KARPAS (cloth, linen) BALAH (lax, flaccid) PHEROCH (curtain) BUS (linen, fabric)
SAD (jug)
The TAP (check)
*OREP (mane, neck) SEPHAR-a (braid, plait)
Sumerian-Greek word parallels
Sumerian.
ZI (edge)
. NU (nation, manufactures)

INIM (word)
. GU, (speaks, protests)
GARSU (command, commandment) ALAM (statue, portrait)
. TAL (far, far away) , TIL (end) . GAM (to wrap, to bind, to connect)
GIN (slave, female) DIN (lord) KURUIGI (king)
Greek ZA-6 (live) NE-os (young) ONOM-a (word) GO-a06 (complain, accuse) KERYSS-o (command) ag-ALM-a (statue) TEL-on (far, distant) TEL-os (end) GAM-os (marriage) GYN-é (female) DYN-ustes (ruler) KYRI-os (ruler)

==== PAGE 162 =====

13. KUR (country) CHOR-a (country) 14. KUR (mountain) OR-os (mountain) 15. GU, (land, country) GE (country, land) 16. DUG, (build)
TEK-taino (build 17. MEMBER (find) TYG-chano (hit) 18. BAR (sky, fire) PYR (fire) 19. BAR (shines, lights up) PIAR (lighthouse)
20. UDUN (stove, hearth) AITHN-izo (sky) 21. RA, RAH (pour, flood) (pour, flood) RHE-o (flows, flows) 22. ID (river) IYD-or (water) 23. AMARA KIN-
OMBR (rainy) 24. HE (pouring out) CHE-o (pour) 25. GIN (go) eo (move, move) 26. GIR, (dagger, knife)
KEIR-o (cut off) 27. KIRRUDA (hole, cleft in the ground) CHARAD (cleft, gap) 28. RAH (hit, crush, smash) RHEG-nymi (break, smash) 29. BAL (axe)
PEL-ekys (axe, battle axe) 30. DUB (fills up the ground) TYMB-os (grave mound) 31. DUB, (scatter) TYP-os (beat, beat) 32. DUB,-BA (drum)
TYMPA-non (drum) 33. GAR (enclosure, fence) CHOR-tos (fenced, protected place) 34. GUR (turn) OYR-o (forgotten) 35. TUH (lost)
TYK-os (axe, hammer) 36. LUB (fox) (moth) 39. PAG ALOP-ex (smoke) 37. ANSU (számár) ON-os (szarnár) 38. ZIZ (moly) HIS
(tooth, close) PAG-e (loop, dagger, net, trap) 40. GUB (lead, control) KN of which: GUBUR (assistant)
KYBER-nao (to govern, to govern) Sumerian-Latin word parallels Sumerian TEP-or (heat) 3. Latin 1. LAY (shines, illuminates) LUX (light, brightness) 2. TAB (sky)
BAR (to gather) com-BUR-o (to burn) 4. PAR (light) PAR-eo (illuminate) 5. BAL (flood)
BULL-io (bubbling, boiling, bubbling) 6. AB (father) AV-us (predecessor) 7. IBILA (son) FIL-lus (boy) 8. AR, ARA (worldly, fame) AUR-a (shine of light)
9. GAN (parent) GEN-ins (origin, birth) 10. SUN (old) SEN-ex (old) 11. IGI, IDE (eye, magazine) VID eo (easy) 12.
DUG, (speaking) DIC-o (says) 1. ELELU (howls, howls) 165 ULUL.-o (howls, howls)

==== PAGE 163 =====

ann
166
IMRI (storm) KUKU (darkness)
. AGAR (arable land, field, field)
GIRIM (fruit)
. NAM (fate, essence)
. NAM (no)
. NO (not)
. NAM (announcement)
. HOUSE (stable, cage, close) . WALL (wall)
. MURGU (enclosure, insight) . DA-RI, (duration)
. MEMBER (beat)
. TUG (clothes)
. GUR (hord)
. KID (split)
. GAL, (picks up)
. MAH (mighty, big)
. LI,IA, (oil)
. GAR (make, prepare)
. ZABAR (bronze)
. SECRET (fund)
. DUB (table)
. DAM (husband, man)
. GUR, (fut)
. LEFT (pours out, floods)
. BULUG (in the form of a circle)
. AG (does, does)
. A'A (water)
. BAD (open)
. FOG (sup)
ZAG (shrine, temple)
SANGU (penitent priest)
. LABAR (servant, slave) . LAB (passes by, passes by) . RIG) (rules)
MASKUM (leather)
IMBBER (rain)
CAEC (dark)
AGER (arable land) GERM-en (offspring, fetus, offspring) NUM-en (prophecy, divine essence) NON (no)
NE (nei)
NOM-en (name, fame) CAS-a (hut)
MUR-us (fal)
MARG-o (frame, border) DUR-o (lasts for a while) TAC-tum (hit, struck) TOG-a (clothes)
GER-O (horde)
SCID-i (split)
COLL-is (domb)
MAG-nus (big)
OL-eum (oil)
CRE-o (create, make) CUPR-um (copper)
SER-o (tie, bind)
TAB-ula (table)
DOM-inus (new, young) CURR-o (fut)
FLU-o (flowing)
PLAG-a (district, district) AG-o (do, do)
AQUA (water)
PAT-eo (open)
HAED-us (bucket)
SAC-rum (sanctuary, church) SANC-tus (saint)
LABOR-o (working)
LAB-i (glides, glides) REG-no (rules) MASCA (mask)
Sumerian-Germanic word parallels
Sumerian
. DAG (bright, shining)
. PIRIG (bright, shining) , IL, GIL, (collect) . ý (eye)
German
. DAG-s, German TAG
(Old Germanic DAG = sky, light) BRIGH (bright)
5 ýýý, HEL, German HELL (light) English: EAGE, German AUGÉ (eye)
==== PAGE 164 =====

28. 29,

30, 31, 32. 33. 34,
35.
36, 37, 38. . 121
(fire)
. GIR, (stove, hearth) . KU (eat)
. GUB (accept, buy)
. DU, (does, does)
. KUD (cut)
. LAH (it, ver)
. SIG, (hit, cut, decide) . MAN (brother, companion)
AD, ADDA (atya) . AMA (anya)
. BAR (offspring, fetus, offspring) . BAR (offspring, species)
. BAR (partner)
. KID (split)
. GU, (country)
. DUG, (good, nice)
. MEN (crown, diadem) . METTU (weapon)
. SA, (name)
. SIG (weak)
. SIG (dried) . LAG (put, put, place)
LUH (wash)
LUGA (defiance, willfulness,
stubbornness)
LIDU (sings)
MAS (cleans)
HAL (empties, opens) HAL (bustles, flows)
BAMBOO (sift)
TIR (forest)
GAR (make, produce) BUR., BURU, (fur) BURU, (ear) öfln.: nem.:
nem.: öfln.: eng.: eng.: goth.:
goth: goth: öskand.: goth: nem.:
nem.: goth: goth:
öskand.: goth.: eng.: eng.: goth:
kfln.: goth: öfein.: öfein.: öfein.: nem.: öfein.:
öfein.:

göt: swedish. gen.: german:
HIZZA, german. HITZE (heat) GÄR en (ferment, boil)
KAU-en (shrimp)
KOUF-on, not. KAUF-en (buy, vásárol)
DO, no. TU-n (do, make) CUT (end)
SLAH-an (out, away)
SIG-is, no. SIEG (victory) MANN-a, no. MANN, MEN-sch (man, human)
ATTA (father)
AMMA (grandmother) German AMME (nanny)
BAR-u (child)
BRU (offspring, breed)
BRU-der (brother)
SKAID-an (cut)
GAW-i, German GAU (district, part of the country)
DUG-a (to be skillful, good) MEN (necklace, necklace) MATT-ock (axe, axe, hoe) SAY SIH-t, German SEICH-t . German: SA-gen (says) SIUK-s. German: SIECH (sick, morbid)
(shallow) LAG-jan, German LEG-en (to put, to put, to place)
LUHH-an, German LAUG-en (lúgoz, kílúgoz LUGi,
German LÜGE (lie)
LIOD, no. LIED (song, song) WASCH-en (wash)
HOL, no. HOHL (hollow) QUELL-an, no. QUELL-en (bubbling, seething)
FLUH-t, no. FLIEH-en, FLUCII + (flee, escape) TRI-u (tree)
GREEK (to do)
BOHR-en (for)
OHR (ear)
167
===== PAGE 165 ===== 39, 40.
42. 43. 44.
45. 46.
47. 49.
50. 51. 52. 54. 55. 56. 57. 58. 59.
vosaun
Bun. 10,
168 . ITIMA (god-
room)

DAL, (n.: to be wide, extensive) AVA (water) German: HUL, öfein.: . GAG (stake, stake) öfein.: BULUG (back stake, back stakes) n&m.: GAD (clothes) oteng.: (destroy, ruin) GUZZA (chair,
HALAM SUD (pour out) throne) öfln.: AS (wish, wish, desire) oteng.: . öfln.: PEL (to get dirty) kfln.: SEN (bright, bright) - kfln.: SIL (beat, street) öfln.: . ger.: ~~SIG, SUD, SUR, RUM, SIBIR, SUG, SAH~~ (goat)

NIN (priestess, öfln.: BARAG (lakás) SIBIR (stick, weapon) öfln.:
priestess) 6eng.: HARUB (insect, worm) ném.: SAH, (pig)
GU, GUD (bull, cattle) öfln.: öfln.: TAL (valley)

AHA (water)
HACK-o, no. HACK-en (aprit) PFLOCK (pile stake) and PLANK-e (plank)
KOTZ-e (fabric)
CHWAL-u (violent death) v.v.
German QUAL, QUAL-en (torture, torment) CHUSSI, German KISS-en (seat cushion) (H)EISH-an, German (H)EISCH-e (extend, demand, demand) SKUR (sweat)
SKUT-en, German SCHÜTT-en (pour) PHUOL, German PFUHL (puddle, swamp)
SCHIN (light), German SCHEIN (lightness)
ZIL-e (row, street)
no. ZEIL-e (e.g. Woll-zeile) SPER, no. SPEER (lándzsa, dárda) NUNN-a, no. NONN-e (paternal) BURG, BARACK-e (castle; stall, barracks)
ZIUG, no. ZEUG (fabric, coarse cloth)
ZIGA, no. ZIEGE (goat) SUG-u, Dutch ZEUG („SÖG“) Scand. SUG-g-a (pig) KREB-s, KRABB-e (crab, uncut crab) CHUO, no. KUH (cow) GADUM (room, premises)

Sumerian-Slavic word parallels
Sumerian . TUR (court) Russian: DIRIG (go, walk) Russian: . DAL (remove) Russian: . MES (man, hero) Russian, ZU (tooth)
Russian: MUB, (skull) Russian: . KUR (mountain) Russian: GAR (encircle, surround) Russian: . URUD (copper) Russian: HUL (bad, evil)
Russian:
Slavic DVOR (courtyard) DOROG-a (road) DAL] (distance) MUZ (man) ZU-b (tooth) MOZG (brain) GOR-a (mountain) GOR-od, GRA-d (city) RUD-a (ore) ZOL (bad, evil)
===== PAGE 166 =====
169
mini of Sf wi
WRONA Se --'- 4
12. 13. 14. 15. 16.
17. 18. 19.
20. 21.
Sumerian DIRIG (walk) PIRIG, (bright) BARAG (dwell) BULUG, fhasft) - BULUG (demarcation, boundary stakes) BULUH (hurry) BAL (axe) HALAM (destroy) HARUB (insect, worm)
KUR (mountain) GAR (enclosure, enclosure)
DUN (watch)
DUB (seal)
DAY (ut, ver)
UDUN (stove)
GU., GUD (cattle bull)
GU, (country, fold)
NAM (announcement)
DAG (bright, shiny)
GAD (hemp, linen) PAR (bright, shiny)
Sumerian-Indo-Germanic-Semitic word parallels Indo-Germanic BARAK Semitic 521. DOROG-a (road) Heb. DEREK (way) no. PRACH (pomp, splendor, light) Heb.
(võllam) German BARACK-e (béde, barak) Hebrew PLG ass. PARAK-u (fence, hedge, wall) German, BLOCK-en (capital, block of wood) Hebrew. BLK (to split) German. PFLOCK (stake, stake)
(separation)
German FLIEH-en (flee) Hebrew PLG (flow, flow) German BEIL (axe) ass. PIL-battery (axe) gr. HOLM-os (mortar) heb. HLM (smash, crush) gr. heb. HOR
HERP-es (worm) ass. HARUB-u (a kind of insect) gr. OR-os, or. GOR-a (mountain) encircle) or. GOR-od (city) (mountain) gr. CHOR-tos (protected, fenced place) | heb. CH-Rm (circle, heb.
gr. DYN-astes (ruler) Hebrew DUN (rule) Greek TYP-os (stamp, token, coin) Hebrew TABA' (seal) Greek TYK-os (hammer) Hebrew GA'A (cow moo)
TOK (strikes) Gr. AITN-izo (sky) Aramaic ATUN (hearth) 6ind. GAU (cattle), Heb. GAIA, GU (foundation, ground) German KUH (cow) German GAU (district), Greek GE-os (land) |
Lat. NOM-en, Germ. NAM-e (name) Heb. NAM (say) Old Ind. DAH (shines, illuminates) Old Germ. DAG (day) Hebrew: TZACH (blinding)
German TAG (sun) Greek CHIT-on (linen) ass. KIT u (linen) lat. PAR-eo (to adorn, decorate) Hebrew: PAR (to adorn, decorate)
===== PAGE 167 =====
170
36.
37. 38.
39.
41.
42.
SIBIR (stick, scepter, weapon) GIR, (dagger, knife)
SIL, (to be unharmed) SILIM (peace, salvation) ZAG (sanctuary)
BAD (open)
BAD (remove, away) KUD (cut)
KIRRRUD (hole, crevice) PAD (to cut into pieces)
DUB (board)
TAB (me)
DURAH (rock goat) AGAR (arable land, fallow land)
ófn. gr. lat. lat. lat. lat. ger. eng. gr. lat. lat. lat. gr. gr. KUD (a kind of vessel) spike, guard, close) Greek GUR (a measure of grain) MANA (a unit of weight and money, a measure of time) TAR (peak,
SAB, SAB (cut off) ZUB (curved sword) SAG (sheath, net)
do. do.
do. celtic do.
do.
GAN (nádállvány, kanna) (pregnant, giving birth)
no. Greek, Latin
SPER (lándzsa)
KEIR-o (road)
SAL-vus (unharmed, whole) SILEN-tium (calm) SAC-rum (sanctuary) PAT-eo (open)
WEIT (distance)
CUT
CHARAD-ra (cleft) FID-i (FIND-o) (cleft) TAB-ula (table)
TEP-eo (reading fluency) TRAG-os (back)
AGR-os, Lat. AGER, Ger. ACKER (arable land) KAD-os (jug)
KOR (veka)
MNA ((mina) - fine unit of weight)) lat. MINU-tum (minute)
TYR-sis, lat. TURR-is (tower) DUR-um (castle)
XYPH-os (card)
CHESS, German: SACK (bag) KANN-e (jug)
KANN-a (reed) GEN-us (origin, birth)
assz. heb. heb. heb. assz. heb. heb. heb. heb. heb. assz. assz.
ass.
Hebrew ass. ass.
ass. heb. heb.
assz, héb. assz. heb.
SIBIRR-u (bot, fegyver) CH-RR (vac)
SLv (nyugalom)
š-LM (peace, salvation) ZAK (clean, purified) PIT-u (open)
BA" AD (remove)
K-TT (weight)
CH-RT (cut)
PAD-a' (split)
DAP, ass. TUPP-u (table) TOP-et (stove) TURAH-u (rock-faced goat) UGAR-u (arable land, fallow)
KUDD-u (a type of vessel) KAD (jug)
KURR-u (a measure of grain) MANU (mina and a measure of time) Heb. MANE (mina)
DUR-u (fortification)
TZUR (rock, refuge) SAIPH (sword)
SAKK-u (sack, penitential garment) SAK (sack)
KAN-u (nadszál) KANN-u (kanna) KAN-e (nadszál)
===== PAGE 168 =====
171
43.
44.

45.
46.
47 48. 49. 50.
51. 52.
53.
54. 55.
GUB (lead, control) GUBUR (helper) BARBAR (burning, glowing red), (light, sunrise) BIZ, BIS (white, pure light)
SAB (bowl, basin, pot)
BULUG, (splitting)
DAR (destroy, dismember) KUD (decide, judge, judge) KUR, -KUR, (bend, bend GUR, (bend, bend)
SER, SIR, (life path)
FISH (flows quickly) FISH (goes away, escapes) GUB (rises, becomes too big)
BARUN (ant) HAB., KUB, (hit, beat, chop) Greek Lat.
Greek Lat. Greek Greek
Ger.
Ger. Ger. Lat. Greek Ger.
KYBER-nao (govern) GUBER-nator (governor) PORPHYR-a (purple) PURPUR-a (purple) BYSS-os (linen)
SKAPH-is (bowl) SKAPH-os (tub, trough) SCHAFF, SCHEFF-el (basin, bucket), SCHOPP-en (ice) PFLÜG-en (plough) DRE-schen (cut) de-CID-o (decline)
GYR-o (turn, twist) German KREI-s (circle) SCHER-e (scissors)
HEL-ko (walks, pulls, draws)
Gothic GIB-la (spire, pediment)
German GIPF-el (peak), GIEB-el (pediment)
Greek Greek
BYRM-ex (ant) KOP-to (hit, beat) Russian
KOP-iti (cut), German HAU-
en (cut), HIEB (hit) Heb. Arabic ass. Hebrew

GEBER (governor)
KEBIR (governor) BARAR-u (shines, shines) Se-PARP Ra (dawn) BIS-u (white, pure, bright) BUS (linen)
SABB-u (bowl, basin, pot) SAPH (basin)
PALAH (plough) DAR-a& (cut) KAD-i (judge) GAGGAR-u (disk) KIKAR (disk) S-'-R (split) HAL-áku (go) HAL-ak (go) GAB'-u (mountain peak) GEB-el (mountain) ARIB-u (locust) HAP-u (stab, stab, push) AKAP (trace)

===== PAGE 169 ===== 172

56.
57.
58. 59.
60.
61. 62. 63. 64. 65. 66.
67.
68. 69. 70.
TAR, DAR (split, destroy, dismember)
KUS (leather)
HOME (stable, cage) HOME (close) (KUD, = sits, lives)
BAR, PAR (sky, fire, opens, sprouts)
TAR (to split, to split) UDU (sheep)
BAHAR (potter)
KILLI, (mass, crowd) IGI (eye, see)
BIL (sky, fire, flame)
SUR (bull)
BAR (fear, fear)
BAR (stick, wand)
BAR (examine, look, search) BAR (expand, wide) Greek
Russian German Lat. Greek Lat. Zend German Greek
not.
Eng. Lat. Lat. Swiss Lat. Slav German Greek Lat. Greek German German German German
TRY phos (piece) DRO bi (debris)
TRU mmer (fragment), TRU mm (ruin)
CUT-is (leather)
KEUTIL-os (stable, apartment) CASA (hut)
KATA (house)
HOUSE (house)
PYR (fire)
BRY-o (series)
BRE-nnen (sky), FEUER (fire) MOR-gen (morning)
TEAR
HAED-us (bucket)
FIGUR-o (shape, form) KCHILLE (community, community) OCU-lus (eye)
OKO (eye)
EYE (eye)
PHLO-x (fire, flame)
FUL-gur (lightning)
TAUR-os (bika)
FÜR-chten (fear), FUR-cht (fear) PRÜ-gel (stick, push) PRÜ-fen (examine)
BREI (broad) ass.
heb. ass.
heb. ass. heb. ass.
heb. heb.
ass., heb. heb. heb.

ass. ass. ass. ass. ass. ass.

x TAR áku (setter) TER ep (ruins)
KUS u (leather)
AUAZU (decorative)
HASA (hides, seeks shelter)
BAR aru (shines)
PAR abu (rises, opens) BR k (lightning)
PII Rj (series)
TZOR (stone knife)
GED i (potter) PAHAR u (potter)
KH L (village, community) AJJI n (eye)
BLg (shines)

SUR-to (cook)
PIR-itu (fear)
PUR-u33u (stick, wand)
BAR-O (see, look), PAR-u (seek) PAR-áSu (expand)
===== PAGE 170 =====

'Almost 100% of the words listed can be recognized at first glance as being closely related both semantically and phonetically.' It is also necessary to emphasize that the words presented are only a sample; the total number of similar correspondences that can be detected is many times greater.

However, we cannot be satisfied with such simple dictionary correspondences, because the school of linguistics that works with regular phonetic changes does not recognize isolated lexical parallels, even if they occur in the hundreds. In the eyes of modern linguistics, word correspondences alone have no evidential value in deciding the relationship of two or more languages. The most common objections are the following:

- 1. Random matches
- 2. Onomatopoeic words
- 3. Children's words
- 4. Borrowed words
- 5. International loanwords.

We have already dealt with these objections (Chapters 1, 2 and 3). We have shown that they are not exactly well-founded. Nevertheless, there is no doubt that for all those who believe in the validity of the sound laws, the above considerations are well-founded, compelling counterarguments, since such word correspondences cannot be accommodated in the axiom of regular sound changes. Therefore, we do not want to enter into a debate with the supporters of the sound laws, we accept their conviction in advance, and consequently we do not intend the above vocabulary statements to be proof, but only to illustrate the parallels that extend to all areas of vocabulary.

Instead of fruitless polemics, we will move on to discussing other, much more substantial correspondences.

Our combined semantic-phonetic method requires that we pay special attention to synonyms on the one hand and to homonyms on the other. By synonyms we mean words with the same or largely similar meaning, and we use the concept of homonyms in relation to word stems, so that we sometimes call a pair of words that way even if their stems are only very closely phonetically parallel, but do not match letter by letter (e.g. KH, BP, DT show a difference), i.e. for the sake of simplicity, we also call so-called homophones homonyms.

173
===== PAGE 171 =====

Let's start with such a -- in a broader sense - 6Greek homonym- y
KARN-on | KAREN-on | KRAN-yon | KARAN-os | KERAUN-os (horn) (peak) (skull) | (prince) | (völlamsugár) Of the Indo-Germanic equivalents, we will now only consider the German parallels.

we present:
(Adjective of Odin, meaning "ruler")
For the inclusion of the word HARN (urine, urine), cf. the parallel between "napsugár-vizsugar", but the double meaning of the Hungarian word "hűy": "star" and "mountain" . urine" and the Hungarian "Berg" and "Spitze" also mean.

However, the equivalents of these Greek and German words can also be found in Semitic languages:
This fivefold correspondence, which goes far beyond the boundaries of language families, indicates that we are dealing with very ancient words. Our first methodological question in such cases is: how can the phonetic correspondence of the 526 common Semitic-Indo-Germanic words with five different meanings be explained — semantically speaking?

The semantic connection is on the one hand the "head and the top of something" and on the other hand the "boss" is an obvious, but almost equally plausible, meaning as that to etymologize (Chapter "the head of something" is also a semantic relation. The meaning "völlamsugár" however, for now seems to be a coincidental coincidence, if we do not want to make such a

1. 3). Instead of similar sharp-witted ideas, we consider it more . The "arrow of God" is also as sharp as a horn or a mountain peak, . Schraube" = "sus scrofa"-like word matches expedient to try to decide on the separate meaning of "lightning" or its possible semantic connection with other Indo-Germanic and Semitic words with the same phonetic form, using specific cultural-historical and linguistic data,
174
===== PAGE 172 =====

If we approach the question from this perspective, we immediately come across several ancient, Near Eastern-Sumerian parallels. It is known that the Sumerians — but also the Elamites and later the Babylonian-Assyrian Semites — depicted their gods with horns or horned crowns, with 1-4 pairs of horns on their heads, depending on their rank. The horn was therefore a symbol of divine power.

The explanation for this can undoubtedly be sought in the Neolithic-Century and Early Bronze Age bull cult, which was widespread throughout East Asia, but also in the Mediterranean, as well as in Southern and Central Europe, the linguistic remains of which are still preserved today in the undoubted connections between the German GOTT (God) and the Sumerian GUD (bull, strong, hero), as well as the Proto-Germanic name THOR and the Sumerian words TUR, SUR (bull, szur, shining). The words Old Indic BAGH-a (God), Slavic BOG (God), and German BOCK, Hungarian BAK, BIK-a, BOK, Old Turkic BUG-a (bull), Latin PUNG-o (perfect: pu-PUG-í) = , szur" are late witnesses of the same semantic, or rather religious-historical connections.

On the other hand, we also know that Sumerian mythology imagined the dwelling place of the gods in the mountains, just as the myths of the peoples of Western Eurasia did later. According to Sumerian cosmology, kingship was to be realized in the world of men "he came down from heaven", was a special gift from the gods to humanity, the king was the vicegerent of the gods, who was called to the heavenly order -- more precisely, its mirror image (ý.ý. the findings of archaeology about the otherworldly ideas of the conquering Hungarians, in which this "mirror image cosmology" still appears intact). Reading the text of the "Our Father", no one would suspect that in the expression "as in heaven, so on earth" these ancient Sumerian cosmological ideas live on to this day.

Finally, the meaning of the Sumerian word DINGIR or DIGIR (God) itself is closely related to lightning: GIR=, lightning, sword". A. Deimel defines this word as DI+GIR= , sword of judgment", , lightning of judgment" or DIN+GIR, = The pre-Asiatic-Sumerian "lightning sword", "shining lightning" is interpreted from the word combination. (Sum.-Akkadian Glossary, under the heading DINGIR.)
mythological and cosmogonic images therefore indicate that the source of the phonetic correspondences of the Indo-Germanic-Semitic words in question must be sought in the ancient pre-Asiatic cultures. Indeed, in the Sumerian language we find the explanation of the identical or almost identical phonetic forms: KUR, -, bend, bend" KUR = , mountain", KUR, = "to be strong, powerful" KAR = "gather, shine" and KURUGI = , king" (literally; KURU + IGI = "eye of the country").

So, in the above — in a broader sense — homonyms, we are faced with four Sumerian 526 root words with different meanings but practically identical in phonetic form, which underwent "-N" stem expansion in both Indo-Germanic and Semitic languages and thus remained in close phonetic relationship. The exact semantic connections

175
===== PAGE 173 =====

in Sumerian requires separate in-depth investigations, which we are not able to carry out at this time. Thus, we do not consider our above semantic considerations to be a definitive solution, but merely a guiding experiment.

We mentioned earlier the striking parallels between the German GOTT (God) and the Sumerian GUD (bull), as well as the Proto-Germanic THOR (German STIER /bull/ and the Sumerian SUR (bull, spike), TAR (thorn, mountain, peak, spike). Continuing on this trail that led us to the ancient bull cult, let us look at the Indo-Germanic and Semitic names of a couple of domestic animals.

It is well known that Indo-Germanic linguistics derives the names of domestic animals from verbs meaning "to seize, to close, to restrain". However, the domestication of most modern domestic animals, according to our current knowledge, first occurred in the pre-Asian Neolithic. It therefore seemed obvious to look for words with similar meanings in the Sumerian language and examine whether they can be compared with the corresponding Indo-Germanic verbs and animal names. We consider our results worthy of at least giving a taste of them even within the framework of this brief overview.

German | Greek | Sanskrit | Latin Assyrian | Egyptian | Sumerian BOCK PAC-u | PEC-us | PUH-adu
(goat) (domestic animal) | (hăziállat) | (lamb)
VIEH VAC-a | VACC-a
(domestic animal) (cow) (cow)
PACK-en | PAG-e PAC (lock) (capture) PAC-o | PAH-u (grab) (loop) ! (grab) | (let go)
(Compare these words with the Hungarian words FOG, FÉK-ez, BÉK-ó, BOG-oz, and BAK, BIK-a.) Old Slavik GOVED-o
Egyptian szumer THE TO,, TO
(bull)
(bull, cattle)
Assyrian GAD-álu (ropes)

===== PAGE 174 =====
(Compare with the Hungarian words GID-a, GÖD-ölye, KUTY-a and KÖT.)
German Latin heifer Assyrian Sumerian Hungarian FÁR-se POR-cus BURU BURU CALF (one year old) (pig) (calf, animal- (domestic animal, BAR om
(16) puppy) baby animal) BIR ka FER-ke! (postal6) BUR ge (malac) PFER-d | PARaVER-edus

PFER-ch FRE-no BA'UR-u (clamp, (brake, (hunting, prison) to tame | prey
German Latin Greek | Hebrew (goat) Assyrian | Sumerian HAB-er ICAPP-ella | KAP-ro !'OPH-er IGHUF-r} KAB-ru (goat) | (goat) | (boar) | (roeibuck) | (sheep) CAP-er

CAP-io KAP-alu you , Tooth, 16- | (grab) | (grab) (stable, loop, (tie)) (tie) HAF
KAB-u GUB (captivity) to tighten car) |
(clamp, box)
(Compare with the Hungarian words GÖB-öly, KOP-6 and GÖB-öz, GUB-özik, el-KAP, KEP-e.)
These serial correspondences suggest very close linguistic connections. The possibility of coincidence is eliminated by the fact that words with different phonemes but parallel meanings
The fourfold coincidence of word sequences — and in languages of 3-4 language families at the same time — mathematically excludes it.
177
===== PAGE 175 =====
Such homonyms often extend to the ancient Egyptian language:
Ancient Egyptian Sumerian Akkdd-Assyrian Indo-Germanic Hungarian MAN je-MAN-d MAN MIN no. MAN MONN-no (someone) (two companions) (someone)
(two) (the person and someone)
MAN-i MEN MEN-woman
MEN lat. MON-ile MEN-woman
(necklace) | (diadem, crown) (headband) (collar) (a, adorned) na, jewel)
MEN, (move) (go) (run MyN-abtu lat. MAN-o MAN-MAN MEN-ni
away) earthquake) (optional) . MAN-u MEN, MUN-attu lat. MAN-e MENNY (he napkette (me, nap) (dawn de- (morning)
location)
MAN-a MAN-u, MIN-u | lat. MAN-eo és MAN MEN-ten
(it took a while- (a while- MIN-utum (immediately,
how long) dare) (lasts for a while lat. NUM-ero (7) NUM-ny (number) count)
süly- (press) (count), but: MAN MAN-u MAN-a MAN-u lat. MIN-a PUSH-ni, but cf. (a süly- (a süly- (mina - MIN-d MON-eta (money)
MON-csol mertek) measure) measure)

(The Latin words NUM-ero and the Hungarian NYOM-ni are probably metatheses of MAN.) Let us now examine a few synonyms. 1. "Run, beat, tear, shred":

Greek Assyrian Sumerian | Hungarian KOP-to HAP-u | KUB,HAB, | KOP-og (üt) (onion, (see, leüt) KAP-al (to stab, stab)
Blacksmith
HOP-le IKB-u (hoof) | (hoof, kick) | (heel)
FÁLL-en | 0 PLE-ge PLG PIL-akku BAL BAL-ta (dont, axe) | separate) | HEAD-sze (push, hit) (blow) (split) (hit, ha- | (axe, hit, | PALL-os cut) sit,
to put out (to
extinguish) BAL ash (hasft) arab: BAL-ag | VÁL-aszt BEIL [TLA-gellum | 8 BAL-t
(white) (whip) (white) (white)
178
===== PAGE 176 =====
German Latin (to trample The word is Assyrian Sumerian | Hungarian KLO-pfen | CAL-co KAL-u GAL, KAL-apál
(to trample) down) (Latite, (lelapit)

In KOL y | KAL-ABU GUL KOL-int (smashing, mace) | SCUL-po smash)
(jerk, (chisel) j (axe)
(chisel)
The TIPP BLACK-ulu DUB, (weakly tear of ii (push, stab) | (smash, smash)
settep DÜB-ein kettle ü apart, (lit, tap) throw down; and:
drum)
DRE-schen : TAR-áku IDAR, TARI DAR-ab (cut) leilit cut into (break, (set, | DAR-abol zers-TOR-en you smash, smash,
(destroy) pieces)
zer-TRU-
mmern
(smash)
s-TECH-en TYCH-os (filter) (csatabárd) s-TAK-en TYK-us DAY-
aszt (stabs) (chisel) DOG-en-tock (s-tock, , DAG-asu
DOG-ilk (stick, (COOTE AC7000) beater, (hit, take- BAR-d hair) was)
VERBER-o | PHRA-gma 1 tear, PAR-áru BAR VER (beat, drum, (wall, vá- (slab wall) set (set) | last, set- |
drum PAR-to So, seven defend) PAR-itlya (separates)
words with different phonetic forms, meaning "to hit, to break, to tear, to split", show completely consistent parallels in the Sumerian, Hungarian, Indo-Germanic and Semitic languages. But we also experience the same
phenomenon if, for example, we compare the synonyms of the opposite concept "to tear, to split, to split" in the above languages.
179
===== PAGE 177 =====
2. "To knit, to string, to weave, to spin, to sew":
German HEF-ten (link)
FLE-chten (fon)
FEL-ge (tire)
GÜR-tel (belt) GAR-ten (garden) KOR-b (basket) HUR-de
(grabs)
(chest) KAS-ten (cabinet)
SCHIRR (equipment)
SCHUR-zen (loop, knot)
BINS-e (jaw) BAS 1 (háncs)
BAS teln (barkácsol)
180
Latin CAP-istro (tool?)
PLE-cto (fon)
VOL-vo (rotate)
CRA-tes
(braid)
HOR-tus (garden)
CIS-ta (láda) CASS-is (net, spider web) SER-o
(knot)
BYSS (fine linen fabric)
Greek HYPH-aino (weave)
(to spin) KYR-tia (basket) CHOR-tos (fenced, protected place)
PAG-e PACH (loop) (mesh) KYS-tos
(chest)
old: CR-I (to tie together) ind: SAR-i (clothing)
BYSS (fine linen fabric)
from:
VAS ana (linen fabric)
Assyrian KAP-ásu (to loop)
PIL-akku (orsó)
(fabric)
(kosher)
PAK-atu (hála)
SIR-du SIR-du (sorrel, kantárszar, gyeplő)

The same picture again: this time the parallel, regular correspondences of nine words with different phonemes in the Sumerian, Hungarian, Indo-Germanic and Semitic languages. We can see that the more sides we approach the words of the most important West Eurasian language families with our semantic-phonetic method, the more numerous parallels we find, but the more massive the ancient Proto-Asiatic-Sumerian correspondences become. The unbroken correspondences of the seven and nine different phonetic synonyms of the two basic concepts presented above across four language families make evident the common ancient roots of these language families. However, the two previous tables are by no means complete; they cover only a part of all the demonstrable parallels.

===== PAGE 180 =====

The same meaning can be established from the German Latin
(lightning!) (shining) | görgö heber | asszr | Egypt | viewer | with Hungarian BRE | PAR-os PYR-os | BRk | BAR-äru PÄR-ach) BAR PÄR-äs (ego) (fire)
(opening) (shining, PÖR-köl BRA-ten | shining, | PHAR-os sen light- | PIR-os BRA-ven PURP-ÜA PURR threat sky, red- | PER-zsel (süt) li, shines) (decorate, (illuminate) (preserve)
befez, | tüzöv- | VÖR-os (böz = böve) towner | re- | ros, sGrf- | BAR-na BRA-u purple-
PÄR-ol (brown) világos- | 41 FEUER (tűzreos, sárga)világos- | PIR-ol VÄR-ah chd dik, kinyi- | VIRR-äd (pompa)
| PÄR-lik, sjäd, | PIR-kan s-PRIE-ssen| FRU-o BRY-o PH-R | PÄR-ähu PÄR-a of the stars | (develop) (sprout) | (sprout, open) sun- PÄR-olog (fruit) | FRU-ctus sperte) | VİR-äg PIR-it (morning)
FRÜH-ling (spring)

183

===== PAGE 181 =====

We have already learned about the Indo-Germanic and Semitic parallels of the “black raven” (Chapter 3). Now we can supplement this with the Sumerian-Hungarian correspondences:

Sumerian BUR, or BUR₁HU (raven) and BUR₂ (hole, depth, descent, dark) Hungarian VAR-JU and BOR-Ú, BOR-UL (to fall, to fall over)

From this we can also see that the Indo-Germanic and Semitic words we learned about in Chapter 3 (RAB-e [hol6] jŷ RAPP-e [tekte 16] etc.) are apparently metatheses of Sumerian-Hungarian phonemes. We can observe many similar cases. We could already notice two of them when we presented the Sumerian-Egyptian-Indo-Germanic-Semitic-Hungarian MAN-homonyms: the Latin words NUM-erus (number) and the Hungarian words NYOM-ni were probably also metatheses. That is why we noted at the time — in Chapter 4 — that Semitic and other ancient scripts, which progressed in the opposite direction compared to cuneiform, may be to some extent related to such metatheses. It is known that the Etruscans, but also initially the Greeks and Latins, wrote from right to left.

The fairy tale phrase "they go through seven countries, they go and go" already occurs in the Sumerian epics from the first half of the 3rd millennium BC. In the ancient epic "Enmerkar and the Lord of Aratta", the Uruk envoys go through seven mountains and seven valleys and go again' before reaching the city of Aratta.

***** PAGE 182 *****

183
===== PAGE 183 =====
The regular correspondences of these six homonyms (homeophony) in themselves provide a very strong argument for proving the closer relationships of the language families in question.

Sumerian: GIVES
GIR GUR, HARA 10 (run) (lightning, (a measuring | (evil- (smashing) lightning) vessel) doer) GU -duggal KUR-um (carry, | (insult, (smashing) grain- | slander) | KIR-rod rakṭār)
(earth belly- HARA, (beer- barrel6)

In the examples presented, the linguistic peculiarity that we have already noted when discussing sound changes is unmistakably apparent: "regular sound changes" are not 1160011, but rather semantic phenomena; they serve to make subtle distinctions and vary the basic meaning.

We might think that confections 5707008600 and more extensive than those we have seen above would be hardly conceivable between the words of four language families. However, if we think so, we would be very wrong. The above six G/K,H/L and G/K,H/R sound-form stems also have six hissing initial vowel versions in all four language families, with synonymous or very close meanings. That is, the six

SOR-deo (to be despised) SOR-dicus (shameful)
SAR-culum (hoe, pickaxe, axe, hatchet)
SCHEL-ten (scold) SCHEL-m (copy) SCHLI-mm (bad) SCHLE-cht (bad, lousy)
BEAT (ut)
SCHLA-cke (snake fruit)
SCHER-be (broken pot, tile) SCHRO-ten (break, grind) SILL-átu SUL-co (mark, inscribe) SCAL- SAL-atu (scattered, -1226-yŷ0y ŷy 212)
po (scratch, scrape)
SERR-a (saw SCRl-bo (scratch, abrade, write) SCHÄI-en (bleed)

SCHLI-tzen (pronounced "s-h-i-t-z-e")
SCHER-en (nyír, le- way)
SCHER zel (gyűrke)
SAL eat (szet road)
187
===== PAGE 185 =====

Assyrian.) SAR abu(SARány
shines) false- SER-to SAR-áru (siet) túl hat) (to light up (crime), if (to clean) (eat, SÁR-u lan, up- SAR-eru cross- (from Ugl,

táncol) SAR-pu zuqnak tott) to be)

WILL, WILL-ag S (shines, burns (burns) to be broken) SUR
ZUR (shines) (breaks, 5 penetrates (burns))
(torch)
CHEAT
RUSE
SÁR (gold) |SER-penyő | SER-t SÁR-ga Such With CSER
massive, multi-level and regular correspondences undoubtedly, using scientific terminology, prove the organic connection of the three most important West Eurasian language families and the Sumerian language, their common ancient roots, which go back through Sumer to the pre-Asian Neolithic and Chalcolithic cultures.

We are talking about no less than 4 x 4 x 6 = 96-fold parallelisms here: six-fold correspondence in meaning between words with four phonemes from four language families.
We have already observed in our previous examples that in Indo-Germanic languages the so-called "S-prefix&s" often appears at the beginning of words, as an extension of the base word. When discussing the satem-kentum transitions, we also mentioned that these SK sound changes are closely related to the issue of \$-prefixation. In the same place, we also briefly pointed out with a few examples that Indo-Germanic words containing the S-prefix are mostly synonymous or very similar meaning words in Sumerian beginning with the sounds \$/S,Z) and G/K,H). From the above compilation, we can now definitively state that these S-prefixes are nothing more than phonetic pleonasm, i.e. in the case of proto-linguistic synonyms S and K, a third merged word with an SK initial sound also appears, with the sound form "satem+kentum".

Semantically, however, these variants cannot simply be called pleonasm, because, as the tables above also show, the S-prefix&s often involves a modification of the meaning. In such cases, words with SK initial sounds are related in meaning
188
===== PAGE 186 =====
with the corresponding variants of the individual Indo-Germanic languages beginning with S and K, but they are not complete synonyms of these. In many cases, however, they are simply phonetic distortions: the confusion and incomplete understanding of the Sumerian synonyms with the initial sounds S and K heard alternately. Similar word distortions can still be heard today, e.g. when snobs who want to stand out try to boast of foreign "culture words". This observation leads us to conclude that the Indo-Germanic languages once developed in a manner similar to the phenomenon of pidginization. One of the most important and striking characteristics of pidgin languages is precisely that they adopt the words of the culture language that was involved in their origin in a distorted manner.

The frequent word-initial vowel drops in Indo-Germanic languages also lead us to pidginization. It can be clearly observed in our comparative tables presented so far that the vowels of the first syllables of Indo-Germanic words — compared to Hungarian, Sumerian and Semitic languages — were dropped in the vast majority of cases. As we have already mentioned, this is partly a consequence of the back stress and partly of the parallel pongyola articulation. But this phenomenon also reveals something else. We could say that in Indo-Germanic languages, the first syllables of words, which are the most important and carry the actual basic meaning, were most often distorted.

In Chapter 4, we have already seen some cases where only the final consonant remains from the first syllable: WORT and RED-e; KO-RAB-os, HRAB-an and RAB-e; VULP-es and LUP-us.

Let's now compare a couple of other similar voice loss cases.
German
szumer
(lead, lead)
I am 55 1
KYBER-netos | GUBER-nator Lead
(assistant, deputy) (ýýý mászik) (governor)
(sliding-crawling) (caterpillar) (stupid) HARUB
(leader) leader HAR
Herpes
HERP-it
REP-tilium
REP-o
ROBB-en (coiled) (hanging-(snake) (sliding-climbing)
RAUP-e (a type of insect, worm)
GIR, (run, step)
SUR, STEP ten | (tut) s-
REI-ten (ride) IDIRIG = to be tall, = to be strong \$- ý TRECK-e és RICH-tung
a and ýý inyuyt road section) Uurany)
(strong) (daha, hös) provides ýý knife RE 5

189
===== PAGE 187 =====
Thus, the word-final extensions that originally served as suffixes, suffixes, and signs took over the semantic role of the ancient roots. These distortions, in which the original basic meaning of the word is often only a single sound, once again confirm our suspicion that we are dealing with pidgin languages in the case of Indo-Germanic languages. They testify that the original basic meanings or basic roots of the Sumerian words, which were well or poorly understood and probably already adopted with stem extensions, could not be separated from the suffixes and suffixes.

However, our examples also confirm that in cases of so-called phonetic attrition, instead of a linguistic process taking place over time, it is most often a matter of the conscious formation of semantic variants: words with complete and incomplete phonetic forms can be found within a language at the same time. That is why we do not speak of phonetic attrition, but of phonetic projection (see the relevant part of Chapter 1, I).

According to the findings of linguistics, but also according to the evidence of 4-5000-year-old Sumerian linguistic monuments, the most ancient root words of languages were monosyllabic. [It is therefore important to examine more closely 15 the means by which languages formed their later formed disyllabic and polysyllabic words.
Two-syllable — or more precisely, three consonants (tri-consonance) — base words are primarily characteristic of inflecting languages, Indo-Germanic and Semitic. However, these base words are not original base words, but developed in such a way that, since inflecting languages use the vowel variants in their entirety in their grammar, they mostly had to resort to base expansions to indicate semantic connections and to differentiate related concepts in nuances. Over time, these tri-sonances — or tri-consonances — have become so intertwined that their complexity is now barely recognizable at first glance. But if we examine them more closely, in most cases we can detect an ancient base, a basic concept with a general meaning, which has also become more differentiated in its meaning with the help of different phonetic base expansions. This process of language development can be clearly observed in the basic meaning of 'fast movement' FL root motif in various German derivatives:

FL-L-en = to fall, FUL-L-en = to pour, to fill;
FL-IEG-en = to fly, FL-IEH-en = to flee;
FL-UT-en = to loosen, knead (water),
FL-ITZ-en = suhanni, iramodni, FL-I-ESS-en = folyni,
FL-ATT-ern = to flap (wings).
We can see that German in most cases specializes the basic meaning with suffixes. We get a different picture if we look at the corresponding Latin words;
FL-uo =flows, PL-uo =falls (rain), BULL-io = flows, swells (water),
VOL-o = fly, PAL-us = swamp, lake; BAL-N-eo = bathe.
190
===== PAGE 188 =====

The Latin language therefore + apparently due to its more archaic nature and earlier development -- shows far fewer stem-expanding forms, but rather = at least in this case -- specializes the basic concept with a general meaning by varying the initial consonant.
The most surprising thing about all these Indo-Germanic words, however, is that they can be directly traced back to the polysemantic Sumerian root BAL, which has three main meanings: a) , sich entfernen, ausgießen, überschwemmen" (to remove, pour out, flood); b) "drehen, Turnus, Spindel, flechten" (to turn, turn, spindle, spin); c) "Beil, schlagen, spalten" (to axe, hit, beat, split). The above German and Latin words — as we have seen — exactly match the first basic meaning of BAL. But we also find derivatives of the other two basic meanings, not only in Indo-Germanic, but also in Semitic languages.

German: FL-ECH-ten = to spin; FL-T-en = to fold; FL-ICK-en = to patch; FEL-Ge = hub (which rotates!); FL-EG-el = whip; FL-ETSCH-en = to snarl, to chatter.
Latin: PLE-C-to (to spin); VOL-Vo (to turn); FLI-Go (to strike, beat); FLA-G-ellum (to whip); PELL-o (to push, to stab, to hit).
Greek: PEL-EK-ys (battle axe); PLE-K-so (hit, beat); PEL-AG-os (sea); PLE-Ko (thread).
Akkadian-Assyrian: PIL-AKK-u (axe); PIL-AKK-u (spindle); PAL-u (turn); PAL-gu (channel).
Hebrew: PH-LG (stream); PLH (spindle); BLK (beat, beat); PH-LG (hashit).
Hungarian: FOLY-ik; BALL-ag; WALL; BIL-inc; VÁL-aszt; (el-)VÁL-ik; FEL, FEL-ez (=divides in two, splits); BAL-ta, BAL-aska; FEJ-sze (cf. FEL-ez).
The Germanic tendency to make voiced sounds voiceless, to "harden" them, is also clearly recognizable in the German words we have seen so far. However, we cannot speak of regular sound changes here either, because other German derivatives of all three basic meanings of BAL in many cases also preserved the original B sound: BAL-G-en (to fight); ýýý.

gs BOL-Z-en (to hit with great force); BALL-ern (to hit repeatedly, to pound); BAL-K-en (beam, beam, i.e. "hasftrott"); BALL (ball); BALL-en (roll, roll); BAL-Z-en (the partridge's dance, i.e. "lorog").

In addition to these, there are also numerous German versions with the sound forms P, PF, and W, but we do not have space to present them here.
191

===== PAGE 189 =====

The linguistic process towards plurals had already begun in Sumerian itself, as evidenced by the linguistic monuments that document DIR (goes, marches, travels, drives, passes) and DIRIG (makes a path) (see chapter 4), BAL (axe, hits, splits; turns, turns, spins, spins; moves away, pours out, floods) and BALAG (makes a blow) -variants with identical or nearly identical meanings. For these stem extensions, the Sumerian language used ancient primary basic verbs with the meanings "does, does; has, exists; has, has", using them as auxiliary verbs.

The linguistic role of such stem extensions is best understood through the example of the English language, where they are commonly used to describe actions such as (to be) and "I make a trip"= , But . "do", "be", "I go". this way of "make" auxiliary verbs. E.g.: "I am making . I do not help you" = "I am going to help you". I'm cool, that is speaking is also very a trip", i.e. I am traveling. common in colloquial German: instead of "ich helfe dir" (I help you), ich tue dir helfen" (I make you help); or: instead of "wir spielen" (we play), "wir machen ein Spiel" (we make a game).

A Basic verbs meaning "to do, to be" are found in all three main articulation circles of consonants in the Sumerian language, and in numerous phonetic variations. , To do, to do". Labial sounds (B, P, F, V, M): BA, BA,, BE,, BI, AB,, BA,, -AG-A, BAR, PA,, MA, ME,, Dental sounds (D, T, Z, S): DU,, DE, DI, DUG,, DUG,, DIM, TE, yy, 1. TUD, TIG, U,-TU, SA,, SA,, SIG,, Velar sounds (G, K, H): AG, GI, GE, UGU,, GAR, KL, KU,, AKA, KUD, KIM. Other sound forms: NA,, RU,, LE.. 2. "To be". Labial sounds: BAR, ME. Dental sounds: TI, ZI, SI. Velar sounds: GA, GA,-GA,, GAL,, GAR. The vowel variations are the consequences of Sumerian vowel harmony, while the alternation of consonants with related articulations can be explained by fluctuating Akkadian-Assyrian transcription, differences in linguistic order, or consonant assimilation (1. below). A. Deimel . In his book "Sumerian Grammar" he says about the role of these basic verbs: "Other verb stems are often pleonastically attached to verb stems, which do not change the meaning of the main stem. 192 ===== PAGE 190 =====

These suffixes are: ag = to do; gal = to be; also dug, (rarely dug,) ... According to Delitzsch S.Gl. 146 this dug, for you, = to do, to machen". (In Hungarian: "Verb stems are often joined by other verb stems, which do not change the meaning of the original stem. These suffixes are as follows: ag = to do, to do; gal = to do, to exist; dug, is (rarely dug,) ... According to Delitzsch Sumerian Glossary, p. 146, this dug, you, = to do, to do, instead of, instead of.") Deimel clearly does not recognize either the real role of these stem extensions or their eminent importance in the field of word formation. In his "Sumerian-Akkadian Glossary" In his dictionary, he makes no distinction in meaning, for example, between the words BAL and BULUG; for him, the meaning remains "spalten" (to split) even after the expansion AG (UGU.); BAL = hasit (fat) — he writes. spalten, BULUG, = „spalten (Holz)" = However, if we look at these "pleonasm" in the context of the Hungarian language — also an agglutinative language — we notice something completely different. "Hasit and "has-OG-at" certainly do not mean exactly the same thing, and the "og-" extension cannot simply be called a pleonasm. The same is true of the other "pleonasm" mentioned by Deimel 18.

"AG": parol — párol-OG rág -- "YOU": full - full-AD pattint — patt-OG-at teng — teng-OD-ik moss — moss-OG-at roll — roll-ED-ik ball-AG tour — tour-rág-OD-ik "DUG": ful - ful-DOK-ik KAL all — áll-DOG- "GAL": run - run-GÁL fish — fish-DOK-ik szür-KAL go — men-DEG-EI szür — ál jár - 1

(Compare the variations of the solo and bass lines that also occur in Hungarian with those mentioned above.) Deimel is therefore right in that these extensions do not really change the general basic meaning of the original verb, but only vary and modify the manner of action; they mark subtle distinctions, but with his Indo-Germanic sense of language he could not register these nuanced differences in meaning, because there is no parallel for such in his mother tongue. The German language expresses such differences in meaning either by circumscription, separate auxiliary verbs, or by changing the root vowel, "Ablaut": "trinken" (to drink), "tränken" (to give drink); "dringen" (to press, to hurry), "drängen" (to urge, to make haste), "borgen" (to borrow), "bürgen" (to lend).

193 ===== PAGE 191 =====

In the inflected languages, these Sumerian basic verbs denoting action and existence had a different role. They were used to form the basic words of these languages containing three consonants. What Sumerian and other agglutinating language systems expressed primarily by changing the vocalization of the basic stem, and secondarily by using variants with related articulations of one of the consonants (usually the initial one), they achieved by varying the auxiliary verbs of Sumerian origin used for stem expansion: the creation of new words with a close semantic connection with the basic concept. On the other hand, what Sumerian and agglutinating languages achieved by using the root words meaning "to do, to be" They used basic verbs, while inflected languages used vowel variations to express the time, manner, and different aspects of the action. These two contrasting parallels primarily characterize the difference between the perception — or, if you like, the "spirit of language" — of agglutinating and inflecting languages. When forming a new, related word, the agglutinating language system changes the vowel or perhaps one of its consonants, and uses stem extensions: suffixes, signs, suffixes to express the time and manner of action or for internal word formation. Inflecting languages. think the other way around. If they want to form a new, related word, they expand the basic stem, and for varying the time and manner of action and for internal word formation, they resort to changing the stem vowel, Ablaut. Now let's look at the practical experience on which we based our previous statement, namely that inflected languages used Sumerian root verbs denoting action and existence to form their root words containing three consonants. In the discussion of metatheses (Chapters 1 and 4), we have already briefly touched upon this phenomenon in connection with the Sumerian word DIR (to go, to march, to drive, to pass), DIRIG (to make a path" = DIR+IG. In the following table, we present the Indo-Germanic correspondences of the full semantics of this multi-meaning Sumerian root word, taking only German as a basis for comparison for the time being. We have chosen German because this language is one of the Indo-Germanic languages geographically furthest from Mesopotamia, and historically, its linguistic remains appear only a good 2000 years after Sumerian ceased to exist as a spoken language. As a result of these circumstances, the probability of possible word borrowings is naturally the lowest here. We do not currently have the means to present the parallel correspondences of other Indo-Germanic languages, but we do not consider it absolutely necessary, because those interested can easily find them in any Indo-Germanic etymological dictionary. they can find the relevant words,

The table only lists the derivatives of the Sumerian basic verbs of the three main articulation circles — BA, DU, 68 AG and their phonetic variations.

===== PAGE 192 =====

we dare, while we ignore the less significant R, L, N stem extensions this time,

Sumerian reports ALPTO. DIR = destroy (destroy) DIR+BA = (+PA.) = - - - - - mourn (gyászol) to become darkened, to be saddened (to become dark, sad) to destroy, to destroy (to destroy, destroyed) tränen, trübselig sein (to be tearful, to be tearful-eyed) treiben (to drive, to drive, to drift) go, way (goes, hits) walk around (ide-oda futkos) stark machen (to make strong) überragend sein, hoch sein (to be outstanding, high) viel sein (to be many) a joint disease (a type of joint disease) German reports (zer-) STÖR-en (destroy) TRAU-ern (to mourn) TRÜB-en (to become gloomy, sad) (zer-) DRUM-ern, DRUM-er (ruined) (ruins, fragments) TRIEF-eln, TRAUF-eln, TROFF en (drip) (drip) (drip) TREIB-en, DRIF-t (drive, drive) (current) TRAMP-eln (walks awkwardly) The TRAB (the one) TRIMM (to train) TRUM, TRUMPF (big piece) (trump card) TRUPP-e (team) TRIPPS (duck) (Note: in the past, untreated, chronic cases of gonorrhoea caused knee arthritis. This is where its Greek name, which is still used in medicine today, comes from: "gonorrhoea" = "that"). . knee joint effusion, inflammation

DIRECT FROM, (+YOU)
DIR+IG (+AG)
= gehen (go) = Weg (hit)
= to be saddened, darkened (szomorúnak, sötetnek lenni) = to pull (a ship) (hüz; - hajót) = to be strong, towering, much
TRET-en (to walk) TRASS-e, s-TRASS-e (direction) (street, street) TRIS-t
(sad, mournful) TREID-eln, TROSS-e (tow; - ship) (ship rope) s-TROTZ-en
(to be strong, prominent, numerous) (to swell)
= to collapse = to run around = to push, stab, stab = to help = to go = to hit = to pull, to be tall = to pull, to drag = to be strong, to be outstanding

s-TÜRZ-en (collapses, falls, topples) TROTT-en (trots, trots, trots)
(Foot) TRITT, TRET-en
(kick) (kick)
TROS-ten (comfort) TRECK-en (tract) s-TRECK-e (road section) s-TRECK-en
(extends)
TRAG-en (carry, carry) s-TARK
(strong)
195
===== PAGE 193 =====
= stossen (push, dof, 5749) = PRESS-en (nyom)
trauern, betrübt sein (fill) (to) PRESS (to mourn. to be sad) (sad, anxious) = to be full, to fill (ät-, through- TRICH-tern, TRICH-ter (to be full, to fill)
(funnel) = passieren lassen (v.ö.eng.: THROUGH (let through) (atenged) allow) = to be darkened DUNK-eln (to darken)
(to be darkened) DARK (sweet)

As a counter-example, let us look at some German derivatives of the Sumerian GIR, = "run, courier" (phonetic variant: GIL.), which was also mentioned in Chapter 4 and which has partially synonymous meanings with the words DIR, DIRIG, and also in the light of the stem extensions formed with the BA, DU, and AG basic verbs. At that time, in the description of metatheses (Chapter 4, p. 154), we claimed that the Latin "GRAD-us" (step) and "TRAC-tus" (progress, pull) — such variants — are only apparent metatheses, because in fact they arose through the expansions of the Sumerian GIR, and DIR roots with the corresponding, also Sumerian basic verbs. We will now have the opportunity to justify this claim. _
Base: GIR, = to run, courier (run, runner) KUR-ier (courier); from Latin CURR-o (run) GIR,+BA = Kraft (force) = 6116y) KRAF-t (strength) = laufen (run)
öffnen. HLAUP-an (run) v.ö. a GIL, varidnssal - SCHRITT (step) = hurry KRAU (run) GIR,+DU, = Schritt (step)
to go (to go) SCHREITEN (step, stride) =eilen (hurry) HURT-ig (quickly, rapidly) z stark, Kraft (strong, force) GROSS, (big) KRASS (crude)

= vorüberziehen (passes by vimi.) GRASS-ieren (steps, walks) GLEIT-en (glides); cf. GIL,
= Weg, Pfad (path, path) GERAD-e (straight)
GLEIS (sin, vägány); v.ö. GIL, = foot (láb) MEMBER (day, vegetable day); v. ö. GIL,
GIR,+AG = courier (futár) = to öfín&m. s-CALK (servant) no. SCHALK (cop, clown); v.ö.GIL,
go KRIECH-en (climbing) = stark, Kraft (strong, power) KRAK-eeler (tease, tease) = Run (foot)
KRÜCK-e (crutch; folk:
leg, leg) z gehen, ziehen, Weg, Pfad (goes, marches, KRIEG (war); cf. "Feld-zug" =
path, path) as er befindet sich "camping" = , war," sometimes
auf , Kriegs- pfad" = is at war, literally: "is on a war path"; cf. also Sumerian GIR, = battle, fight.
196
===== PAGE 194 =====
One of the plural forms of GIR already appears in Sumerian language records: GIRIM = "laufen" (run).
Finally - for the sake of completeness - we also present the Sumerian "satem" correspondences of the German words in the tables that show S-preposition; thus providing further evidence for our earlier findings on the actual role of S-preposition (1. of this chapter, pp. 187-188).
German: (Zer-) STOR-en (destroy) Sumerian: ZUR = to crush, to crush
STRASS (street) SAR, = syllabus;
SIL, SULU = road, path, street on STROTZ (swell) SAR = swell, be many on STÜRZ (fall, fall) SUR, = hangs, floats,
SUR = it rains, it falls, it falls STRECK (road section) runs, rushes, SIR, = eletrodolodik STRONG (erosive) SAR, = thick, fat, strong SCHREITEN (step, stride) ON, =
disappears
SAR, = to move away, to remove, to send away SCHALK, n. SCALK (cop, SAR, z grows, expands, multiplies
clown) (envoy, servant)

In view of these serial correspondences, there can be little doubt that the Sumerian language played a very significant role in the development of the triconsonancias of Indo-Germanic root words.

Similar phenomena can be observed in the Semitic languages. These too formed a considerable part of their characteristic three-consonant roots by merging Sumerian roots and already known root verbs. This linguistic process can be clearly observed through the Semitic parallels of the multi-meaning Sumerian roots BAL, BAR (and BIL).
In our next compilation — without any claim to completeness for the time being — we also took into account the correspondences of three Indo-Germanic languages.
[Sumerian | Akkadian Hebrew Greek Latin German 1.BAL+AG] PALG-u | PH-LG | PELAG-os | BULL-io WALL-en | = pour out, | (channel) (stream) (sea) (wave, ; flood, boil, foam) | boil, foam)

[| = bucket SILVER-u | RIDDLE RIDDLE-ys FALX | = üt, see PILAKK-u BEIL (balta) (axe) (battle axe) (hit, beat) (sickle) (axe)
BLK PLEG-and FLIG-o hexatayllag With FLEG (hit, beat) (blow) (hit, beat) (whip, |

[= u yy PH- LG 53 VUL-va s-PAL-ten y 197 Well (splitting) Ka (pubic slit) (cleft)

===== PAGE 195 =====
PH-L-CH = PAL-a PFLÜG-en (barázdál) (spade) (intended)
suppress, s-PHL s-PHALL-o FALL-o FALL-a leigaz, (sink, (fails) (fails) (fails) waste
submerge)
= 0156, PILAKK-u | PELEK | PLEK-o | PLEC-to | FLECH-ten turns BELL-um | BALG-en välik, (spindle) (spindle) (fon) (phone) (fon) = to fight against the enemy [na-
ellen- (enlensé- (háború) e-BER-u BR-CH s-PERCH-o VOL-o FLIEH-en goes (flies, quickly) Common (fight, haste) (run away) gesség) = BALK-utu) =
below). RL transitions can be proven by the partial synonymous meanings of the words BAR and BIL in the Sumerian language: (BAR-, "fire", (BIL-), "fire" etc., l. (flee) flee
But we have already seen that RL variations are almost always semantically related.

Sumerian Akkadian Latin Hebrew German BAR+AG BAR-aru B-L-G FER-I see s-PRÜH-en = shines,
(shines) (shines) (glows, burns) (sparks,
lights sparkles)
up PURRUH-u | BRK z PHRYK-tos FRIG-o (beföz) (will) (fire, burning) (toast)
separates, PAR-ware RK PHRAG-ma FRANG-o tör (break)
PYRG-os, BURG-os BURG (ván circular wall castle) (close) (protected, |-6y PHYRK-os (wall, separation) (break) FRAC-tus (broken) z enclose | PARAK-u | PH-(fort)
BERG-en-fortress) | (defensive bastion, (saving) wall,

= series, PARÁH-u | PH-R-J BRY-o VIRG-a s-PRIE-ssen opens (áld) (series, (series) (series) (shoot, branch) (shoot) opens) = begs,
BRK PREC-or FLEH-en ima (imádkozik) | (könyörög)
arab: B-L-G PHLOG-eos FULG-ur FLACK-ern meggydjít | WALACH-a | (shines) (bright) (lightning) (flashes, flashes) (convinces)
PHLO
x(fire)
198
===== PAGE 196 =====
We can see that BIL is merely a phonetic variant of one of the bases of the polysemantic BAR 16526.
In our table we have presented only the Semitic parallels of the roots BAL and BAR (BIL) formed with the basic verb AG. If we examine the Semitic words expanded with the Sumerian basic verbs of the other two main articulation circles, we will reach the same result. We will postpone the detailed elaboration of further word correspondences until later, because the organic connections we have learned so far, the multiple, regular correspondences going into detail, the massive parallel correspondences of homonyms, homeophones and synonyms are more than enough to make the eminent importance of the question obvious even to the adherents of the laws of sound. We could not undertake more within the framework of this study.

However, we can draw two other, no less important lessons from these massive word matches, which can significantly advance the development of research in this direction. Namely, we can state that the reserved position of linguistics regarding the uncertainty of the transcription of Sumerian words is not justified by experience. Secondly, there is a general opinion that A. Deime's work "Sumerisches Lexikon" is outdated and cannot be used for language comparison purposes, and it also does not hold up in practice. We do not claim that Deime's work does not contain errors, mistakes or typos. We know this very well, because we have experienced inaccuracies ourselves in several cases. However, we claim that these errors are not so significant that they cannot be eliminated with due care, proper checking and investigation (1. e.g. what is said about "pleonastic" stem expansions on page 193).

Therefore, until a new, better work is available for research, we will continue to rely on Deime's lexicon to explore the connections between Sumerian and West Eurasian languages. In this regard, we consider it worth mentioning that F. Kluge's "Etymologisches Wörterbuch der deutschen Sprache" is much more outdated than Deime's Sumerian lexicon, but it was republished in an almost unchanged form even recently — in 1975 — because there is still no better, more comprehensive work. In fact, Benk's latest Hungarian etymological dictionary also refers to it at every turn.

"In our brief overview, we have managed to show that such massive, almost minute, coincidences between the words of Sumerian, Indo-Germanic and Semitic languages and their way of word formation simply rule out the possibility of coincidence by all criteria of evidence. Under such circumstances, we can only speak of organic, very profound linguistic connections. The nature of these linguistic connections, the historical circumstances of their former existence, are most clearly explained by

199
===== PAGE 197 =====

We can explain this, almost self-evidently, with the prehistoric insights that can be established from the archaeological find complexes described in Chapter 1. These harmoniously fitting, reasonably complementary and organically, without any forced explanation, linguistic, cultural-historical and archaeological facts have led us to the well-founded thesis — which, in our opinion, is largely in line with the former historical and cultural-historical situation and is very close to certainty — that during the Neolithic, Copper Age and Early Bronze Age, in East Asia, in parallel with the cultural koine, as its linguistic projection, a linguistic koine also existed, the massive effects of which can still be demonstrated in the languages of the western half of Eurasia, with the help of the Sumerian language. These linguistic influences appear to be inextricably linked to the pre-Asian cultural and ethnic influences discussed in Chapter 1, and constitute their linguistic reflections.

Considering the enormous cultural difference between the population of the Paleolithic and, even more so, the Early Bronze Age civilizations of Proto-Asia and the surrounding existed among the indigenous population of Western Eurasian lands, we can only conclude that these Proto-Asian linguistic influences must have acted and occurred in a manner very similar to the phenomenon of pidginization, just as European colonization in the 17th-19th centuries led to the development of numerous pidgin languages in underdeveloped areas — in just 2-300 years.

The various primitive peoples living on the periphery of the Proto-Asiatic cultural zone, depending on their own level of culture, the more or less developed state of their language, and on the other hand on the intensity and duration of cultural influences, adopted and further pidginized the Proto-Asiatic cultural language, or one of its dialects that was most closely related to them or was geographically closest to them. Thus, in the immediate periphery, certain "lingua-franca"-like mixed languages apparently developed very early on in order to facilitate trade and other relations. We can consider the Elamite, Hurrian, Hattian, Kassu and Gutian languages as such auxiliary languages. However, the surviving remains of these languages are not yet sufficient for definitive language comparisons.

In all those areas where a significant number of agricultural settlers have immigrated, the direct linguistic influences of these colonists also contributed to these effects, which we must attribute paramount importance to, if only because it is known that a larger population, a population density forming a connected, closed population, can only develop when and where the agricultural and animal-breeding economic form becomes established somewhere. The sparse indigenous population of the areas in question, who led a fishing-hunting and gathering lifestyle, therefore quickly,

200
===== PAGE 198 =====

It was soon to become a fatal minority in the face of the rapidly multiplying Proto-Asian agricultural settlers, even if the immigrants did not initially arrive en masse. On the other hand, at about the same time as the southern colonists appeared, the indigenous population began to move further north in many areas. The areas occupied by the larger Proto-Asian settler hordes included primarily the Mediterranean region, the Balkan Peninsula, the southern part of Central Europe with the Carpathian Basin. Southern Ukraine, the northern coast of the Black Sea, the northern foreland of the Caucasus, the southern region of the Caspian Sea, Western Iran and Turkmenistan. Undoubted archaeological traces of smaller-scale pre-Asian immigrations or secondary colonizations originating from the aforementioned areas can be found in the area of Austria, the Bohemian-Moravian basin, the Don-Volga-Oka-Ural region, Eastern Iran, Afghanistan, Baluchistan, the Amu Darya region, the Altai foothills, and even the Minusinsk basin (see Chapter 1).

Ferenc Kemény also evaluates these paleolinguistic questions in roughly the same way in his book "Das Sprechenlernen der Völker". We can safely accept his position in this regard. However, the starting point of his further conclusions - i.e. that he examines Eurasian linguistic connections exclusively on the basis of the Hungarian language - is, in our opinion, excessively one-sided, and his word agreements are very forced. In this we cannot follow Kemény in any case, because we consider his approach to be just as mistaken and narrow-minded as the method of the linguistic school based on regular sound changes, the sterility and untenability of which Kemény criticizes very sharply and with serious arguments, and then rejects it completely.

We do not go so far in our criticism. We do not consider the linguistic research of a century and a half to be in vain, not least because without it we ourselves would not have been able to continue on the path we have begun. In the theory of regular phonetic developments, we only object to the rigid, temporal approach and the arbitrary fragmentation of semantically related word families — which inevitably follows from this approach. Otherwise, however, we readily acknowledge the enormous achievement of modern linguistics in the field of working out the internal connections of language families. We do nothing more than try to broaden the linguistic horizon and, building on the results so far, to explore the connections between the individual West Eurasian language families and their apparently very close ex-linguistic connections with the most ancient, pre-Asian cultures of our Earth. However, to solve this task, for such global stem comparison studies, the dogmatic system of regular sound changes is not sufficient, no matter how well it has proven itself.

201
===== PAGE 199 =====

method for researching the relationships between closely related languages and for distinguishing individual language families from each other. The combined semantic-phonetic method outlined above makes it possible to conduct a comprehensive, comprehensive examination of all semantically related word groups, covering each individual sound variation. Such extensive semantic research has been prevented so far by the categorical imperative of sound laws. Our method - although we are still at the very beginning of the investigations - has already led to surprising results. It has become clear, for example, that homonyms, homeophonies, which are identical in their sound patterns - or only slightly different in nuance but different in their meaning, which linguistics generally considers to be accidental coincidences and explains by the limitations of human sound formation, are very often not homonyms in the true sense, because their meanings are only apparently different, but in reality they are metaphorical semantic variations of a basic concept formed through ancient associations of images, which are either not at all or only very loosely connected in meaning to our modern thinking.

The most important discovery, however, is undoubtedly the unexpected emergence of massive, close parallels between the West Eurasian language families and the Sumerian language, some excerpts of which we have been introduced to above. These serial, multi-level correspondences — both in the field of synonyms, homonyms, and polysemantic Sumerian roots — strengthen our conviction that we are on the right track. We will therefore continue to use 6 combined semantic-phonetic methods in our comparative linguistic studies. According to our thesis, the languages of the Indo-Germanic, Semitic, and other West Eurasian language families are more or less pidginized derivatives of the Neolithic-Stone Age-Early Bronze Age Proto-Asiatic linguistic koine. Since the most significant dialect of this ancient Near Eastern language community was the Sumerian language, which played a dominant role in East Asia for more than 1,500 years, it logically, and even necessarily, follows from our thesis that we attribute a key position and decisive importance to the Sumerian language in the examination of the relationships between West Eurasian languages and language families.

This is the only point of view that differs from the so-called common origin of the West Eurasian languages, which has been gaining ground "Nostratic" theory (Ilyich-Szvitich, Dolgopolsky), which is also in the Soviet Union in recent times, but which largely relegates the Sumerian language to the background. However, we believe that no comprehensive theory of comparative language can exclude Sumerian from linguistic historical studies. Moreover, it is not enough to consider the Sumerian language as just one among the others, because all the currently known prehistoric, linguistic and cultural-202

===== PAGE 200 =====

Historical data clearly indicates that Sumerian was in the 4th, 3rd, but partly occupied a prominent place even in the 2nd millennium, truly dominated among the languages of the then civilized world. By ignoring this language, the historical "Nostratic" theory, in our opinion, excludes the real historical and foundations of language,

From the general linguistic, word-formation, and semantic correspondences discussed in this chapter, from the presented mass lexical correspondences, and from the serially occurring multi-order homonym and synonymous parallels of the latter, we can draw the following summary conclusions:

1. The Indo-Germanic languages do not stand in isolation, but their vocabulary shares the closest connections with all other West Eurasian language families — both ancient and modern — and their linguistic structure shows the closest connections with the Semitic languages.
2. The West Eurasian language families developed through the branches and varying degrees of pidginization of the proto-language that can be called the Neolithic-Early Metal Age pre-Asian language koine.

3. The basic words of this Proto-Asiatic language can still be found in large numbers in all of the West Eurasian language families today.
4. We must explore the proto-linguistic connections with the help of Sumerian linguistic monuments because:
 - a) Sumerian was the most important dialect of the Proto-Asian language, dominant for more than 1500 years, and

b) according to the evidence of linguistic monuments, it has changed little during its one and a half millennium of flourishing, which is undoubted evidence of a fully mature language structure that dates back to an incalculable antiquity.

5. The so-called "Indo-Germanization" of some of the pidgin-like languages that developed in ancient times was a relatively late process: its earliest beginnings can be dated to after the turn of the 3rd/2nd millennium BC. This difference is not so much evident in the vocabulary, but primarily in the characteristic language structure and grammar.

6. Since the emergence of pidgin languages, each such language has developed independently, and the closer parallels between the individual groups are not necessarily the consequences of a common origin or a common proto-language, but most often the results of living side by side, historical and other relations, in the sense of Trubetzkoy's areal languages, linguistic alliances. Therefore, we cannot demonstrate a common proto-language or a unified common people for the individual language families. Attempts in this direction to date have not led to reliable results and can be considered essentially unsuccessful. As we saw in Chapter 2, Indo-Germanic studies itself has already admitted the failure, and even more, the fundamental theoretical error, of such research, while Finno-Ugric linguistics still considers the common common language — indeed,

203
===== PAGE 201 =====
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It is based on the 19th century thesis of several proto-linguistic stages that are believed to be separable in time.
We cannot speak of related and unrelated languages in the western half of Eurasia. The only difference between languages that linguistics considers related and unrelated is that they are more closely or more distantly related.

. Modern linguistics only examines the phonetic changes of words, and even these not in their entirety, but exclusively in the narrow cross-section required by the axiom of "regular sound development" imagined over time, while it not only completely pushes the much more significant semantic connections into the background, but even breaks up semantic units and word families that are obviously related in the interest of regular sound correspondences.

The method of regular sound developments is consequently global,
It cannot be used to examine the relationships between language families. It is only sufficient for comparing languages - and even then only with significant restrictions - whose close relationship is already obvious.

Without recognizing the primary semantic role of sound changes and developing a new, complex semantic-phonetic method of language comparison, linguistics cannot go beyond the boundaries of the classical language families established in the 19th century.
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===== PAGE 202 =====
BOOK II
WITH DOUBLE COURAGE
On the methods and results of Hungarian historical research
===== PAGE 203 =====
Chapter 1: A Glimpse into the History of Science
Within this framework, we cannot of course provide a comprehensive introduction, but can only briefly and sketchily address the main stages in the development of Hungarian historical research.

Hungarian historical research before 1848
Historical research, which can be called modern and methodical from today's perspective, began in Hungary in the 18th century. The extensive collection of material, the search for and systematization of sources began. At the same time, the interest of our historians also extended to the times before the conquest. With the help of our own chronicles and contemporary or nearly contemporary foreign — primarily antique, Greek-Latin — sources, they tried to look into the darkness of ancient times.
It was a work far exceeding the standard His work "Annales veteres Hunnorum, Avarorum et Hungarorum ab anno a Chr. 210 ad annum Chr. 997" is not only a scientific work of its time of György Pray's work published in 1761, and even in 1954 A. Kollautz (Die Awaren. — Die Schichtung einer Nomadenherrschaft) stated that Pray wrote a high-level Avar history in the 18th century, to which research has hardly been able to add anything to this day. Certainly, this cannot be said of many historical works from the 18th century, and it is hardly the case even for those from the 19th century.

Shortly thereafter, István Katona also began publishing his oeuvre, "Historia critica regum Hungariae, 42 tomuli in 44 partibus" (1779-1817), which far preceded, for example, the similar German work by Pertz entitled "Monumenta Germaniae historica" (1826-1844). Due to its wealth of material, this fundamental work by Katona is still indispensable in Hungarian historiography today.

In the direction set by Pray and Katona, in their high-quality historical-critical spirit, their students, the next generation of historians, continued to work. György Fejér, Ferenc Kállay, János Jernyei — to name just a few — all sought the origin and ancestral homeland of the Hungarians in the Caucasus, the Caspian Sea, Transcaucasia and Persia, and brought our ancestors into closer historical and ethnic contact with the peoples who lived in these areas in antiquity, namely the Medes, Parthians, Kushans, Hephthalites, as well as - in full accordance with the general international scientific opinion of the time - the Scythians, Sarmatians, and in the 1st millennium AD the Huns, Avars, Sabirs, Onogurs, and Khazars,

207
===== PAGE 204 =====
In our modern history science, the profession is, so to speak, the doctrine |
It is one of the rules of our history to make good-natured fun of these researchers, or at best to mention their unscientific, confused, and foolish views, which the development of science has supposedly surpassed for more than a hundred years. This opinion, about the complete lack of method and irreparable obsolescence of previous Hungarian historical research, was formed in our history in the second half of the 19th century, and has remained unchanged ever since.
The polyhistor István Horvát is especially often cited as a frightening example, namely his work "Drawings from the oldest stories of the Hungarian nation" (1825), in which - we will allow it - he undoubtedly often went far beyond the school in his word matching. For example, the Révai Lexikon writes about this book: "... not only in Paradise, but in Persia, Greece, Italy, among all ancient peoples, he finds the ancestors of the Hungarians, naturally only by following the similarity of names." Gyula Szekfő calls Horvát's work a "bizarre error", adding: he identified all the peoples of Europe, Asia and Africa that are worth something." (Hóman-Szekfő: Magyar Történet, VII. k. p. 124.)
..... in his dreamy word interpretations, he refers to the Hungarians as Scythians

Let us note carefully where István Horvát looked for parallels to our words: in "paradise" (Mesopotamia), Persia, Greece and Italy. That is, precisely in those areas where massive Sumerian linguistic influences can be detected.

After this consideration, we believe that István Horvát's basic idea was not as fantastic and bizarre as we might think at first glance when reading the above characteristics. His "bizarre error" can only be understood and explained in such a way that he must have noticed something of the close linguistic connections that connect the language families of Western Eurasia. And this was already the case when the excavations of the Proto-Asiatic 81 had not even begun.
István Horvát was not a fantasist. This can be clearly and undoubtedly established from his many other high-quality works that satisfy all the requirements of scholarship — which, however, the zealous quoters of "Rajzolatok" do not usually mention. If, therefore, he nevertheless published "Rajzolatok", it could only have been motivated by his firm conviction of the close connections between the languages he compared and Hungarian. In the linguistic volume we will have the opportunity to show that he was not mistaken in this. He did not have a well-founded, solid method, but before we criticize him, we must also take into account that he did not have at his disposal the linguistic monuments and cultural history data that we now know from archaeological discoveries in Pre-Asia. Nevertheless, he nevertheless saw the deep connections of our language.

208
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===== PAGE 205 =====
with the other West Eurasian languages. And this was no small achievement, even if it went well over the mark.
For comparison, we can mention the case of Budenz, who at the time claimed the Finno-Ugric relationship of about 2000 Hungarian words. Of these 2000 agreements, later criticism found only 450-500 correct. (B. Collinder, quoted by László: történetünk legkorábbi szakaszai, p. 37.) But even if we accept the approximately 1000 words considered Finno-Ugric by Hungarian linguistics as a basis for comparison, a good 50% of Budenz's etymologies in this case still come to mind. Should Budenz be condemned for this, or even, what's more, mocked? On the contrary, everyone emphasizes It could be called a "fantasy of István Horvát". Who would get it?
how correct Budenz's basic idea about the Finno-Ugric relationship of our language was.

But let us also consider that this prehistoric approach - which we could also call "deli" in contrast to the "northern" Finno-Ugric trend - in addition to the early medieval source, there are also the , numerous ancient and Hungarian-Hebrew, Hungarian-Persian, etc. linguistic parallels and similarities demonstrated by the linguists of the time, which were also the basis of the science of the time.

provided a basis that was fully adequate to the level of the Hungarian chronicles. However, apart from these circumstances, the medieval Hungarian chronicles and national traditions also clearly pointed in this direction. Moreover, the Chinese sources that became known in the 18th century revealed the previously unsuspected leading role of the Huns (Hyung-nu) in the history of the Eurasian steppe, from Manchuria to Hungary (J. de Guignes), i.e. they actually confirmed the credibility of our chronicles to a great extent. For researchers thinking in larger historical perspectives, there could be no doubt after this that the Hun traditions of the Hungarian chronicles had to be based on indisputably concrete historical facts and processes, because wherever the ancient Hungarians lived in the Hunnic period, they had to belong to the Hunnic empire.

We can therefore in no way share the view of our contemporary historiography that the Hungarian prehistory research of the first half of the 19th century can interest us only as curiosities of the history of science. We should be much more surprised that, with our current knowledge, this opinion can still exist, and even prevail. The archaeological, cultural history and anthropological discoveries of recent decades increasingly testify that our pre-1848 historians were not romantic dreamers detached from all reality, but based their findings largely on original, thorough and critical source studies. We must not forget that these researchers with a humanities background were familiar with the Greek, Latin, Hebrew and Syriac languages, so they could draw on the original sources, which unfortunately can only be said in very few cases about our later historians.

209
===== PAGE 206 =====

We are convinced that it would not be a useless waste of time if one or two of our researchers devoted themselves to processing the Hungarian historical literature of these early times, with special regard to comparing the views and approaches of our historians of that time with the newer archaeological and cultural history findings. However, it seems that no one takes the trouble to look up what these "Horvat Istváns" actually claimed, but rather, accepting the opinion that has developed over the last hundred years, they continue to diligently quote the destructive criticism of their fathers and grandfathers. However, it would not hurt if they occasionally opened one of these mediocre, "cóplos" or "Biedermeier" folios, because they might not find very thought-provoking things.

For example, I could discover that, according to Bálint Kiss's book "Magyar régiségek" (1839), our historians also classify this work in the category of "Drawings..."
"As early as 1839, István Horvát and his colleagues established that the Avars were not descendants of the East Asian people called Zhuangzhuang — as European historiography had previously believed. Even the historical research taught —, but they migrated west from the territory of the Hephthalite-Hun empire in northeastern Iran after 550. And what does God do: in the 50s of our century the German until recently (Geschichte felfedezte" ezt: HW Haussig (Theo- phylakt's Exkurs über die skythischen Völker), F. Altheim (Geschichte der Hunnen), K. Jettmar és szerzőtársai Mittelasiens) all reject the previously generally accepted identification of Zhuang and Zhuang. And this is — and for us this is the most important aspect of the question — primarily based on Haussig's original source studies.

In the meantime, however, the Zhuangzhuang derivation of the Avars, which had been proclaimed for a hundred years, had become so entrenched in our country that Károly Czeglédi still spoke out against it in 1969. (Migration of nomadic peoples from the East to the West.) Although he no longer follows the old path and allows that the Avars may have had very close historical and ethnic ties with the Hephthalite or White Huns, he does not give up the Zhuang-Zhuang thesis. For this, we also need to know that neither the Hun-Hungarian nor the Avar-Hungarian kinship can be included in the thesis of the exclusive Finno-Ugric origin of the Hungarians, and consequently, of course, neither the historical and ethnic connections between the Huns and the Avars. Especially not since the survival of the masses of the Griffes-Indás, the so-called late Avars, after the conquest was proven (Lipták). Apparently, this is why our historical science has taken up the idea of identifying the Avars with the Zhuang-Zhuangs, who are probably of Tungus-Mongol ethnicity.

which, however, cannot be proven by any written record = and kept quiet about their White-Hun relations, despite the fact that we know of several contemporary source data regarding these, and indeed, as we have seen before,

210
===== PAGE 207 =====
were known as early as 1839. This may have been due to the fact that the name Avar (abar, apar) sounds suspiciously similar to the real name of the Parthians "aparnoy" in Greek-Latin sources, and for this reason, some foreign researchers have recently considered the two peoples to be ethnically identical — at least in part. We will discuss these issues in more detail later.
Now let us just note - in advance - that it seems to have been hasty to make such a boast that the Parthians were indeed related to the Huns and the . At the expense of the "István Horváts" because lately it has been slowly starting to Avars. And this is precisely what our romantic dreamers, among others, claimed.
However, we can also find other, equally interesting and "extremely modern" statements in Kiss's book. For example, he determined the age of Zarathustra, based on careful source analysis, to be around 500 BC. And again "what does God do": this recognition also only appears in international historical literature a good 130 years later. (Altheim-Stiehl: Geschichte Mittelasiens im Altertum, 1970.)

Let us not think that this is an unimportant circumstance for us. It is far from it. Not only because the religion of the ancient Hungarians was undoubtedly very closely linked to the teachings of Zarathustra.* Recent studies clearly indicate that Zarathustra's religion originated in East Iran, in the region of Sogdia or Bactria, where — according to recent research results — in the middle of the 1st millennium BC, i.e. in the time of Zarathustra, we cannot count on an Iranian population. (More on this later.)
However, from the perspective of our current line of thought, this question also has another, very important historical aspect. Namely, the reason is that Indo-Germanic historiography only reached the point in the second half of our century of being able to establish the age of Zarathustra, taking into account the ancient sources of nearly the same age, and carefully analyzing them (1).
Altheim-Stiehl's work) can correctly determine this. Previously, these sources were swept aside, in contrast to the already well-known methods, as was done with all sources that did not fit into the "source criticism" was applied to them theory of Indo-Germanic cultural priority. In the case of Zarathustra, this source criticism was based on so-called "linguistic-historical considerations". Indo-Germanic linguistics at the time established, with the help of Grimm's "laws of sound development", that the sacred books of the Zarathustra religion, the most ancient "Gathas" of the Avesta, correspond to the linguistic state of the Iranian languages around 1000 BC. As a result, * We cannot deal with a detailed analysis of these relationships at this time, but we will only point out that very important tasks still await Hungarian research in this religious history issue,
"Of course" Zarathustra could not live in the

211
===== PAGE 208 =====

After the turn of the 2nd/1st millennium BC. All those ancient sources that spoke of Zarathustra in connection with the first Old Persian rulers of the 6th century AD, contrary to the "linguistically" established "facts", could only be wrong, that is, in the light of this one-sided view.
Well, similarly to the ancient Indian Vedas already introduced in Chapter 2 of Volume 1, these Avestan Gathas -- and with them, of course, the Avesta itself -- have also been greatly rejuvenated in recent decades, after the start of more objective Indo-Germanic research.
These realizations — the thoroughly erroneous 19th-century dating of the Vedas and the Avesta on "linguistic history" and "phonetic development" grounds — played a significant role in the fact that sober, modern Indo-Germanic linguistics rejected the possibility of deducing a common Indo-Germanic proto-language, and even its existence, for theoretical and methodological reasons, but also emphatically questioned the validity of Grimm's phonetic laws.

From these two excerpts alone, we can conclude that the research of Hungarian historians before 1848 cannot be dealt with by simple references to the history of science. On the contrary, many recently discovered circumstances point strongly to the fact that in many respects they far surpassed the scientific standard and methods of the Indo-Germanic historiography of their time, a trend that came to power in the 1930s and 1940s and, in the interests of its one-sided theory of Indo-Germanic cultural priority, in countless cases arbitrarily, mostly unfoundedly and unmethodically rejected the previously known prehistoric connections or confused them to such an extent that a century had to pass before modern research slowly and gradually "rediscovered" these historical connections that were clearly visible 150 years ago.

The school of our Finno-Ugric origin
The rapid expansion of Indo-Germanic studies in European scholarship soon had its effects felt in Hungary as well. After the 1850s, the historical trend and perspective of the pre-1948 Hungarian researchers increasingly faded into the background, parallel to the gradual rise of the views of the German and Austrian school that proclaimed the exclusive Finno-Ugric origin of the Hungarians — Schlözer, and then his followers: Zeuss, Büdinger, Rössler, Dümmler, etc. — This group of researchers with a radically Indo-Germanic perspective categorically denied even the possibility of any other linguistic, ethnic and historical connection between us, and claimed that the prehistory of the Hungarians, the origin of our people and language could only be traced to the areas inhabited by the peoples called Finno-Ugric, i.e. the Upper Volga, Oka, and Kama rivers, the Ural Mountains

212
===== PAGE 209 =====

and Western Siberia, in the Eurasian forest belt, roughly north of latitude 50-55.
This theory was nothing more than an integral part of the global, continental-scale prehistoric view of Indo-Germanic studies at the time. After East Asia, Iran, Central Asia and the Eurasian steppe - from the Carpathian Basin to Mongolia and China - were populated everywhere with Indo-Germans, there was simply no free space left for the Hungarians, who were "determined" to have arrived in Europe at the lowest stage of barbarism, in the southern, more cultured "Indo-Germanic" region. The undoubted Finno-Ugric affinity of our languages was the most important, but also the only, support for this view.

There is no space now to examine in detail every aspect of this incomprehensibly one-sided, stereotyped approach. Nor can we delve into the analysis of its scientific and non-scientific causes and roots. These questions require a separate monograph on the history of science.
Therefore, we only point out that we can only partially agree with the rather widespread views according to which this scientific trend was primarily and most importantly a consequence of the conscious cultural policy of Habsburg absolutism between 1849 and 1867, and that it owed its general spread almost exclusively to this policy. There is no doubt that a certain parallel can be observed between the strengthening and then dominance of the hypothesis of Finno-Ugric origin and the striking expansion of the German and other foreign-born historians and linguists, effectively supported by the Austrian governments of the absolutist period, in Hungarian scientific life, but this phenomenon is only a secondary, less important aspect of the issue.

Much more important, we could say decisive, is the fact that our researchers could not escape the suggestive influence of the seemingly impressive results of Indo-Germanic comparative linguistics and the international historical science based on it. All the less so because the linguistic and historical structure of Indo-Germanic studies at that time still seemed rock-solid, was generally accepted, and in Hungarian corresponded in every way to the then-established standards of methodological scholarship.

We cannot therefore cast stones at our scholars who joined the line set by Indo-Germanic studies and condemn them simply because they did not notice the hairline cracks that were then barely appearing on the surface of the monolithic block of the Indo-Germanic cultural priority approach.

The Finno-Ugric theory undoubtedly came to the Austrian cultural policy of the Bach era (1, Gy. László: "Where it was, where it was not" article in Élet és Irodalom, February 23, 1974). number), which, given the opportunity, sneaks under the noses of the rebellious Hungarians who boast of "Scythian-Hun" ancestors

213

===== PAGE 210 =====

It could be argued, but the further direction of Hungarian linguistics and historical science was not decided by this, but by the generally accepted views of the international scientific world. For the sake of objectivity, however, we must also remember those sharp-eyed Hungarian researchers who, despite all the seemingly overwhelming results of Indo-Germanic studies, recognized its — then still hidden — internal contradictions, and took the courage to — consciously risking their scientific names — repeatedly express their doubts and emphatically cast doubt on more than one generally accepted opinion at that time. Such scientists, who were far ahead of the scientific approach of their time, were Géza Nagy, Károly Fiók, Gyula Rohonyi, Ármin Vámbéry, József Thury, György Almásy, Károly Antal Fischer and many others. The understanding and reasonable conclusions of these researchers have been proven by time in a striking number of cases. In the following, we will have the opportunity to repeatedly point out that their findings very often coincide with the results of the latest research.

However, due to the given situation, their words could not find an echo at the time. The majority of our scholars adhered without reservations to the view of Indo-Germanic studies, which was considered infallible, with all its advantages and disadvantages.

We have already mentioned that we cannot see anything extraordinary or objectionable in this fact itself. In fact, we must evaluate it positively, because at that time the methods of Indo-Germanic studies represented the highest degree of scientificity. The trouble only began when, without checking sources, sober consideration and objective criticism, they enthusiastically picked up and further varied the ideas that Indo-Germanic linguistics and history, which they believed were infallible scientific methods, had raised in connection with the origin and prehistory of the Hungarians - mostly without sufficient knowledge of our language and historical sources. In other words, they practically renounced all independent, original source research and their work consisted mainly in trying to provide Hungarian-related evidence for assumptions that were fashionable in foreign - primarily German - historical research, unfortunately very often at the cost of violently misinterpreting our sources.

This undoubtedly had a role in the cultural snobbery and Western adoration that was increasingly prevalent in Hungarian society at that time, and which has been pervasive in our country ever since, not only in explicitly scientific works, but also in almost all literary products. However, this question again goes beyond the scope of our current topic, so we can only point to one or two of the countless such phenomena.

For example, there is the now commonplace belief that in the first half of the 19th century, the "progressive", "liberal" and "liberal" parties dominated the constitutional-political field for centuries.

===== PAGE 211 =====

berális", , We were lagging behind "democratic" Western Europe, meaning by this the "feudal", "reactionary", "medieval" Hungarian constitution of the order, Well, in In the classic homeland of "liberal" progress, England, in 1828, other Protestant denominations following the non-Anglican faith were granted equal rights, and then some Reformation-era laws declaring them "politically a year the Catholics non-existent" were repealed. However, the situation in Northern Ireland today is the clearest proof that this has remained almost a written grace to this day. But the situation with "democratic" voting rights was no better. Before the 1832 electoral reform, there were 400,000 voters out of a total population of about 16 million, or 2.5%, and after the reform, in 1848, 680,000 out of 20 million inhabitants had the right to vote (3.4%).

However, we must also take into account that until 1832, more than a hundred representative mandates were a privilege of certain members of the nobility, meaning that about a quarter of the representatives were not eligible for election.

In France, during the Bourbon Restoration, until 1830, 50,000 out of 30 million French people could vote (0.166%), and under the "civil king", Louis Philippe, about 190,000 out of 32 million (0.6%).

The data above comes from the 1976 edition of Propyläen Weltgeschichte.

At the same time, the population of Hungary in 1842 was 12,800,000. (Hóman-Szekfő: Hungarian History, volume VII, p. 63) Three years earlier, in 1839, 680,000 of these were nobles, with 136,000 heads of families (ibid., p. 64), which, taking into account the non-family adult nobles, represents 1.1% of the electorate.

We therefore have no way of understanding why the ratio in our "our centuries-old backwardness" — especially not when we consider that the other Western European countries country was much worse than in England and France, and indeed, the concept of suffrage was not even unknown: representatives were appointed by the king or the government, as in Austria, for example, until 1867.

We also don't really understand what Szekfő means by saying that the French Revolution of 1830 (volume 127) Perhaps it means "...drove out the government of Charles X, which restricted the suffrage"? (id. m. VII. that the right to vote was no longer restricted after that? (see above: 0.6%)

So before we continue to roll such clichés like a beetle, it would be instructive to take a little look into the areas behind the resounding backstage voices.

Or let us take the uncritical projection of our 19th-century civilizational backwardness — the multiple causes of which we cannot analyze now — back to the Middle Ages, the constant references of our historical science to the fact that

215

===== PAGE 212 =====

almost all civilizational and cultural achievements have reached us from Western Europe. However, we can read with astonishment in K. Frischler's work that in Hungary "The Adventure of the Crusades" there was already an organized police force in the cities at the end of the 11th century, during the first crusade. Speaking about the crusade of Peter of Amiens (the hermit), in connection with the well-known robbery of the market in Zimony, he writes: "...the Frenchman killed the Hungarian, and when the police (the Hungarians and the Eastern Romans maintained police forces in the cities) wanted to intervene, a wild uprising broke out." (ibid. p. 55) In Hungarian: "...the Frenchman killed the Hungarian and when the police (the Hungarians and the Eastern Romans maintained police forces in the cities) wanted to intervene, a bloody fight broke out." And to get an idea of the cultural picture of Western Europe at that time, let's look at some very characteristic lines from pages 52-53 of Frischler's book.

"The first encounter of Peter's motley crowd with the machinery of the perfect, what can be called modern, Eastern Roman bureaucratic state is almost impossible to imagine in a grotesque enough way:

On the one side, the mass of French and German pilgrims, ignorant, superstitious, impatient, and capable of committing horrible atrocities at any moment; with a civilization as different from that of the Eastern Roman Empire as the way of life of an inner-African negro from that of a Frankfurt family. On the other side, the great Christian empire..., whose organization (it is only in the last hundred years that we have begun to appreciate it correctly) preceded by centuries the much, much more cumbersome feudal system of the West.

... The Westerners felt that they were considered barbarians (and indeed they were — not only in the eyes of the Eastern Romans), and the hatred of the intellectually inferior for the intellectually superior, this eternal human characteristic, poisoned relations between the two groups of people, so different from each other, from the first moment."

One might say that this only applies to the vanguards of robber bands, such as the army of Peter the Hermit. So let's turn the pages further in the book. On page 76 we can read the following about the Byzantine introduction of the knights of Gottfried of Bouillon: "Frankreich gewohnt waren, ihre Notdurft." That is, they were marched into Three soldiers performed a task in the middle of a main street, just as they would do back home. the middle of a main street in Constantinople, as was customary in France. This in itself would not be anything special, since they were ordinary soldiers. However, it is more characteristic that after the Byzantine police naturally locked up the three soldiers who were causing public trouble, the leaders of the Crusaders saw nothing more than a challenge to themselves, the hostile feeling of the Byzantines towards the Crusaders.

216

===== PAGE 213 =====

This matter caused long-term diplomatic complications. The Byzantines simply did not believe that such a thing was not customary there, cf. Then on page 78, Frischler describes the imperial audience of Gottfried of Bouillon and his staff: + "The Frankish knights were led, in accordance with a psychologically well-considered custom, through ever more splendid halls. An Eastern Roman observer noted that during their procession to the Emperor in the Blachernae Palace they put small figures of gold, alabaster, and porcelain in their pockets — souvenirs of a great moment." These were the leaders, the princes and counts,

Remembering the surgical skills of the conquering Hungarians, including the successful craniotomy operations, it is instructive to compare them with the surgeons and aides of the Crusades two hundred years later: "There were men who were unaware, ... who were not even able to treat a wound from the battle.

Thousands died from relatively harmless injuries because they were treated improperly, without the many wound ointments long known in the Orient" (id. m. 97. old.)

In Hungarian: —, They were completely ignorant people, ... who were incapable of treating even a battlefield wound. Thousands died from relatively insignificant wounds because they were treated incorrectly, without the many ointments — long known in the East — that were used for wounds."

We could continue making similar comparisons at length, but we will only come across them if we browse through new foreign works, because Hungarian historiography does not usually deal with such things. On the contrary, it portrays the conquering Hungarians as primitive barbarian hordes who had only recently emerged from the "Commonwealth" conditions, and whose education was much lower than the cultural and civilizational level of all the surrounding peoples at that time.

In addition to the data cited above, we can refer to numerous other foreign historical works¹⁵ when we emphatically state that such a devaluation of the Hungarians who occupied the country — and the parallel praise of the so-called "Western culture" — in no way reflects reality. However, for now, let us be content with quoting Gyula Illyés' identical opinion, which he formulated in a statement given to television in 1980: I recently read a reader's letter 16/6166, sent to a newspaper. He listed in it how science often portrays our past, almost exclusively telling us about the shortcomings, for example, that the Hungarians who came here during the conquest, almost destroyed all cultures.

217

===== PAGE 214 =====

they got it from the peoples here. Many people deduce this from the words alone, because this and that word are of Slavic or German origin. This is a false conclusion, because we know that words are interchangeable. I could say a whole host of things, the concept of which obviously existed in ancient times, but the word that we still use to name them today is only two hundred years old. It is also a false view to the detriment of the disabled, to say only the bad things about history. Since at the same time, the nationalisms around us constantly portray the Hungarians as invaders, this "view" is fatally destructive. The Hungarian intellectual life did not give an objective picture of the Hungarians. The Hungarians who came here were as educated as those who were here." (Alfold". Debrecen, issue 3 May 1980.)

Let's look at a specific example of this approach to our history writing.
In our country - until recent years - it was customary to proclaim that after the Battle of Augsburg in 955, we ran back to the Lajta, some say to the Fischa, from the Germans. Anyone who did not bother to read László Szalay's book "The History of Hungary", published in 1861, could not have learned from our later history books that the Hungarian border on the western edge was still Melk, a good 85 km west of Vienna, twenty years after the Battle of Augsburg, in 975. Yet this information is well-known in Austrian history, and in fact, in Melk, on the site of the former castle, it can still be read today on a memorial plaque. Finally, in 1977, György Györfy took note of the real situation in our country as well. (King Stephen and his work).

Presumably, many Hungarian tourists had already discovered the tablet by then, and it was unlikely that our ancestors would continue to wander to Lajta.
By the way, Szalay was also one of those who did not uncritically accept all foreign opinions about the Hungarians. In his aforementioned work, he repeatedly rejects various ideas conceived in an Indo-Germanic spirit. For example, he writes about the so-called "Great Moravia" as follows: "See Dudík B. Myáhrens allgemeine Geschichte, Brünn, 1860. 102.s ff. 11. The creative imagination of Czech historical investigators has written countless things on several pages of the book." (id. m. vol. I. p. 8. note 2.)
In this regard, we should also know that barely two or three decades later, in his book "The History of the Hungarian Nation to Saint Stephen", he states that Svatopluk's kingdom extended to the Szerém region. (id. m. p. 44)
Indeed, the real "Moravia magna", which Constantine, born in Bilbor, writes about as lying south of the country of the Hungarians, really "extended to the Szerems" -- only not from today's Moravia, but from the Balkan tributary of the Danube, the Morava. Newer research is slowly revealing that Szalay was right to speak of the "creative fantasy" of Czech historical investigators, who gave Emperor Constantine the Great a name.

===== PAGE 215 =====

was simply dismissed as a mistake, and "Great Moravia" was extended to the entire western half of the Kárpát Basin, up to the Tiszántú. However, Emperor Constantine not only placed "Great Moravia" in the south, in the Balkans, but also recorded that after the Hungarian attack the Moravians fled to the Bulgarians and the Croats. This data clearly proves that the emperor was not mistaken in the cardinal directions.

The Archbishopric of St. Methodius of Sirmium also claims the Balkan-Syrmia region. (For more information, see Boba I.: Morávia's History reconsidered, 1971.)
We also learn from Szalay who originally declared our chronicles to be false tales: "Schlözer and the like, who would like to pursue King Béla's anonymous scribe like some beast in the somewhat overgrown "critical forests" of German scholarship..." (ibid., vol. I, p. 11, note 1.) We will show later that Szalay is not exaggerating here either.

Also characteristic are pages 63-64 of volume 1, where Szalay argues with German and Austrian historians who want to deprive the conquering Hungarians of even the elementary knowledge of craftsmanship: That our ancestors knew and practiced all those manual crafts, without which knowledge, in an age when the division of labor was still completely unknown, even the smallest household is unthinkable: that they manufactured their own kind of carts, on which their wives and children followed the migrants; that the war-loving people knew how to forge iron: that will not be doubted by those writers of the German land who, just as they still look at the Hungarians with the eyes of Otto of Freising, so they also speak of them with the wisdom of Otto of Freising."

Here we can almost tangibly feel the basic thesis of Schlözer's extreme view of history, according to which the conquering Hungarians were at the lowest stage of barbarism when they moved into the Carpathian Basin. At the same time, however, Szalay is sober, thoughtful, and takes into account the cultural and economic conditions of the time.
His logical argument clearly shows the perversity and ahistoricity of the one-sided views of the German school.
Despite everything, Pál Hunfalvy and his followers joined this Schlözer trend, whose "scientific" and "objective" statements, and often unconcealed, open hatred of Hungarians, we will have the opportunity to learn about some characteristic quotes in later books.
We do not want to ask at this time what deep psychological or perhaps other reasons there might have been for Hunfalvy specifically adopting this perspective - which holds extreme views even within Indo-Germanic scholarship - to the extent that in many cases - for example in his statements condemning Hungarian chroniclers without reservations - the frothing or mocking tone of Rössler, Bűding, etc. can be clearly recognized. Let it suffice to mention for now that the Hunfalvy school is uncritical,
219

===== PAGE 216 =====

Against this one-sided, entirely negative, dismissive interpretation, an Austrian scholar, RF Kaindl, had to defend and at least partially restore the credibility of the Hungarian chronicles around the turn of the century. He showed, among other things - through thorough and detailed textual analysis - that already in the 11th century there undoubtedly existed at least one - possibly two - Hungarian, "primordial geszta", from which - or from which - the later chronicles were drawn. (Studien zu den ungarischen Geschichtsquellen, VII., VIII.)
These questions did not seem important to the Hunfalvys; they rather devoted most of their time and energy to destroying the credibility of our chronicles.
We can therefore conclude that since the turn of the century, the followers of Hunfalvy's school have not been able to defend their position by saying that it was the generally accepted opinion of science at the time.

However, despite everything, the Hunfalvy school left its indelible mark on our historical science. From the 1870s onwards - with a brief interruption in the 1920s and 1930s - the Hunfalvys' approach remained the sole starting point for the various assumptions and conclusions of Hungarian historical research. With a few exceptions, even today the views of the Hunfalvy school characterize our sound historical literature, as is clearly evident from the Illyés statement quoted above.

But in other areas too, they went so far as to recklessly and greedily adopt the fashionable hypotheses of contemporary Indo-Germanic historiography, that we can trace them almost every decade in our various Indo-Germanic historical literature.
It is enough to refer to the parallel "wanderings" of the Indo-Germanic and Finno-Ugric "ancestral homelands"; the Indo-Germanic ancestral homeland moved to Scandinavia, and the Finno-Ugric simultaneously moved from the Altaï region to the Oka-Kama-Byelaja region, and then — after Indo-Germanic studies abandoned the Scandinavian thesis and placed their ancestral homeland in southern Russia — our Finno-Ugric peoples hastily began their return migration behind the Urals, to the northwestern Siberian region. (Cf. Péter Hajdú's new hypotheses about the "Uralic" ancestral homeland around the mouth of the Ob; although other circumstances apparently played a role in the emergence of this idea.)
Until the end of the 19th century, however, apart from the almost complete absence of criticism and the adherence to an extreme, explicitly anti-Hungarian trend, we cannot see any other extraordinary developments in the development of our historical science. As we have already mentioned, at that time Indo-Germanic studies occupied a dominant position in both linguistic and historical research, and seemed to be the only truly methodical scientific theory. It was therefore obvious, and even necessary, that Hungarian history and

220
===== PAGE 217 =====

The research is in line with the generally accepted international approach.
However, the situation has fundamentally changed since the beginning of the 20th century; with the more extensive development of excavations in Proto-Asia, the discovery of the ancient, non-Indo-Germanic peoples of the Middle East, their high level of culture, and the increasingly clear demonstration of the enormous size of these peoples and cultures, covering almost the entire Western Eurasia.
With the recognition of its cultural and ethnic influences, the most important pillars of the Indo-Germanic cultural priority theory collapsed.
The usual references to the antiquity of Indo-Germanic linguistic monuments and historically known Indo-Germanic cultures suddenly lost their validity. It was no longer possible to argue that before the appearance of peoples speaking Indo-Germanic languages - apart from the Semites - only uncultured, barbaric hordes populated the Earth.
We might expect that international historical and linguistic studies would have drawn the necessary conclusions after this. Unfortunately, this has not happened to this day.
We cannot yet deal with a detailed analysis of the reasons for this surprising phenomenon. This would far exceed the task we have set ourselves this time. Therefore, we can only point out that all these eminently important discoveries in prehistory, culture and linguistic history have also remained unexploited in Hungarian historical science — but also in Finno-Ugric linguistics. Despite the fact that Indo-Germanic studies have already begun to revise their one-sided views of the 19th century in the last three decades — although far from being satisfactory and consistent, but at least in several very important details. Although they are still silent about the linguistic effects of ancient pre-Asian civilizations, a significant proportion of scholars no longer consider the theses of a unified ancient Indo-Germanic people, an ancestral homeland and a common ancestral language acceptable.

It is all the more incomprehensible that in our country both historical and linguistic research continue unchanged in the 19th century. They still try to build not only the development of the Hungarian language, but also our "Ural" ancient history on the hypothetical "(as they are recently written: "Uralic"), or even - dividing this into subgroups - "Finno-Ugric" and "Ugric" ancestral languages. In doing so, they assume something that Indo-Germanic studies itself rejected as unprovable and highly improbable at the end of the 19th century. Let us remember that the division of the ancient Indo-Germanic language into subgroups, understood in terms of time, was not acknowledged by authoritative scholars as early as 1883 (O. Schrader: Linguistic comparison and prehistory.)
So while our history and linguistics were at the level of international scholarship at the time until the end of the 19th century, this can no longer be said about them without reservations in the 20th century. Our century

221
===== PAGE 218 =====

From the beginning, Hungarian research has gradually declined, become provincial and increasingly lags behind international standards.
This process was only briefly halted by the more extensive Turkological research that developed between the two world wars. At that time, it seemed that our historical science could finally break through the historical sources and the results of archaeological and anthropological research. from the magic circle of "exclusively Finno-Ugric origin". In addition to the "Vogul roots" of our linguistics, they began to take into account the contemporaries

In the light of these broader-ranging investigations, the contradictions of the Hunfalvy school's approach increasingly become apparent. For example, it turned out that the level of education of the conquering Hungarians was in no way inferior to the cultural development of other Eastern and Central European peoples of the time. István Zichy, Géza Fejér, Gyula Németh, Lajos Ligeti and others achieved very significant results in this field. The young Gyula László also came forward with his fundamental work. (The Life of the Conquering Hungarians, 1944.)

However, all these attempts could not loosen the oppressive bonds of Finno-Ugric linguistics. This was also facilitated by the fact that the results of Turkological research were soon used by the so-called "Turanism" and extreme right-wing political trends to support their ideas. Given the extreme political passions at the time, it is not surprising that Turkology itself was thus suspected of being extreme right-wing. Unfortunately, no distinction was made between truly scientific research and political phraseology that used it for its own purposes. Thus, after 1945, the baby was thrown out with the bathwater, and our historical science fell back into its old rut. An illustrative example of this is György Györfly's book "Our Chronicles and Hungarian Prehistory", published in 1948, which in every respect and in its entire approach follows in the footsteps of the Hunfalvy school, almost without taking into account the results of previous decades.

Yet the drawbacks of the one-sided approach to Hungarian prehistory, based on purely linguistic hypotheses, were noticed not only in our country in the thirties, but also abroad. Moreover, by scientists who cannot even be suspected of being right-wing. Zakharov and Arendt, Soviet researchers (1934) for example, state that — although they do not want to deal with the criticism of the Finno-Ugric theory in any detail — they "Levedic Studies" (Arch. Hung. XVI. nevertheless consider it necessary to note that the Turkish element and culture undoubtedly dominated among the conquering Hungarians, and that these facts forced the supporters of the Finno-Ugric theory to put forward the most diverse and complicated hypotheses.

In the following, we will try to demonstrate this regrettable one-sidedness of our historical science in an illustrative manner, using specific examples.

===== PAGE 219 =====

to do so: by means of a detailed description of two representative Hungarian historical works of the recent period. Thus, we can observe case by case the contradictions of the exclusive Finno-Ugric origin theory, the inevitable consequences of enforcing this hypothesis at all costs; that Hungarian prehistory research is indeed forced to the most narrow-minded, unreasonable, made-up, and sometimes even bordering on the ridiculous, assumptions, in order to further maintain the fiction of our "Finno-Ugric origin". At the same time, however, we also have the opportunity to point out, in close parallel with the questions raised, those data and other research opportunities promising significant results, which the Finno-Ugric school of our historiography tends to forget about, or — if mentioning them cannot be avoided at all — tries to trivialize them as much as possible.

The two works:

1. Antal Bartha: The Hungarian Society of the 9th and 10th Centuries. (1968 and 1973)

2. István Fodor: On the famous path of Verecke... (1975)

Antal Bartha is an author at the forefront of contemporary Hungarian prehistory research. So much so that he was commissioned to edit the prehistory chapters of the new Hungarian historical synthesis planned for ten volumes. His well-known work is considered one of the significant preliminary studies of this historical synthesis.

We chose István Fodor's book because no less a personality than András Róna-Tas, the president of the "Szeged Prehistorical Working Group", stated that this work is "one of the most outstanding works of the latter period of Hungarian prehistory research." (A. Róna-Tas: A nyelvrokosság, 1978. p. 422)

223

===== PAGE 220 =====

Chapter 2

NOTES ON ANTAL BARTHA'S WORK "HUNGARIAN SOCIETY IN THE 9TH-10TH CENTURIES?"

On nomadization

In the introductory pages of the book, Bartha discusses the circumstances of the nomads' transition to agriculture with a level of detail and precision that deserves the greatest praise.

Here we are missing only one thing, which, however, is very important in the context of general historical perspective and cultural history. Although it is true that Bartha has already delimited his task in time in the title, it would have been absolutely necessary to also point out - at least for the sake of a footnote - that on the one hand, agriculture had been practiced in Southern Russia since the 3rd millennium BC, for example in the areas of the Tripoli or Kuban-Maykop cultures, and only a few centuries later much further north, and on the other hand, the nomads themselves were not only animal breeders from the beginning of time, but nomadism - as a cultural state better suited to the natural conditions of the grassy steppes and realizing their economic exploitation to a higher degree - developed from the agricultural cultures of the Neolithic and then the Early Bronze Age throughout the Eurasian steppe, roughly from the 2nd/1st millennium BC. at the turn of the millennium.

But we also know today that the nomadic tribes that kept large animals never completely abandoned agriculture, but limited it to the production of the most necessary products in their own backyards. The mention of this recognition is very important because we will see later that it is in sharp contrast to Bartha's historical perspective. He will introduce the Hungarians to agriculture in the second half of the 1st millennium AD (on page 89 of his book; — more on this later). Bartha does not write anything about all these things, although we know them mostly from the research of Soviet scientists (I. pl. M. Gjasnow: Südsibirien), so we cannot assume that our author does not know about them.

Certainly, the Finno-Ugric-inspired image of the "wild barbarian" Hungarians of the time of the conquest already appears in the introduction. Bartha must avoid introducing larger historical perspectives, because if he were to mention these, he would have difficulty speaking of "primitive community states" as the direct predecessors of the equestrian nomadic tribal organizations of the second half of the 1st millennium AD, as he repeatedly does in the introductory pages.

However, archaeology has revealed several well-defined agricultural cultures throughout the entire steppe zone, from the Carpathians to the Altai.

224

===== PAGE 221 =====

From the beginning of the 2nd millennium BC — somewhat earlier in the west, somewhat later in the east —. In fact, by the middle of the millennium we find an advanced Bronze Age almost everywhere, not just in one place. (Andronovo culture, Balanovo, beam-frame cultures, etc.) with bronze exports and far-reaching trade connections, also from Eastern Europe to Altai. Now, if we take these facts into account, when can we talk about "primitive community conditions" in this area? Not in the 1st millennium AD at all.

Bartha is therefore clearly still thinking in the 19th century "black and white" cliché: here are the educated Indo-Germans, there are the barbarian nomads, who still have "primitive communal conditions". This is a strange view of history, and it gives us rather mixed feelings, especially when we consider that its representative will compile the prehistoric chapters of the planned great Hungarian historical synthesis.

A from the name "sabartoi asphalo"

Our next remark — or rather our association — is related to note 220 on page 32. There, Bartha transliterates the word "ziebel" in Theophanes' Chronographia without further ado as "iebel", in the sense of the Turkish "jabgu" honorific. Seeing this, we are reminded that our historians always read the Middle Byzantine "bēta" as "v", notably when it comes to the famous "sabartoi" of Constantine born in Bitor, the old name of the Hungarians. In every case, without exception, they transliterate it as "szavárd", and they even mostly prove with examples that the Middle Byzantine "bēta" should only be read as "v". However, "jabgu" = "ziebel" clearly shows that the Byzantines wrote "b" with 15 "bēta", so the pronunciation of this letter varied: a Why all this effort? Why should we read savárd and why not? Later we will see that the old name of the Hungarians "Sabyr" unambiguously leads us to Transcaucasia. The Sabys originally lived there, even before the Hunnic migration, in the 4th century AD (Ammianus Marcellinus etc. — more on this in 1. later historians place them there. "b" is "v" was used to denote sounds. "szabir"?)

Some of their groups only arrived there later, in the second half of the millennium.

not in the Ob-Irtysh region in Western Siberia, where the Hungarians

However, these connections are contradictory to the theory of our original Finno-Ugric origin. Accordingly, attempts have been made to reconcile the word "sabarto" of Constantine with the name "Sevordik" ("black sons") found in Armenian sources, who lived in some villages of Armenia without any apparent reason. This is how it can be explained.

was the

225

===== PAGE 222 =====

to separate it for some Hungarians. "Moving towards Persia", which Constantine mentions in connection with the name "Sabartoi", and on the other hand, he succeeded in deriving the name "Sabartoi" from the name of the Sabirs.

Although nowadays the agreement "sabarto" = "szabir" is more or less accepted, the transcription "szavárd" is still commonly used. This is a strange thing, but it very well illustrates the mass inertial force that is still alive today of long-outdated and refuted literary data.

To the question "hryvnia"

On page 36, regarding the Slavic unit of fineness called "hryvnia", Bartha notes that it is certainly not a Byzantine adoption — although it corresponds exactly to half of the Byzantine "liter" because Byzantine money only began to appear in the —. Eastern Slavic territories after the 9th century, while the grivna system had already become so entrenched and general among the Eastern Slavs in the 8th century that even the Arab dirhams were classified according to the grivna weight system. On the other hand, the grivna is also found among the Western and Southern Slavs, so it is certainly older than the 9th century. The grivna question cannot be considered closed, by the way — he adds in conclusion.

This is all true. The arguments are also sound. However, the historical perspective is again missing. As a Hungarian researcher, perhaps Bartha could have thought of the very obvious

The possibility that the Avars might have transmitted the Byzantine liturgy to the Slavs is also considered. This would be the most likely explanation, both spatially and historically.

It is well known that the Avars minted large quantities of imitations of Byzantine coins, even the solidus (Gy. László: Art during the Migration Period in Hungary).

On the other hand, it is also a well-known fact that they settled the largest part of the Western and Southern Slavs in Central Europe and the Balkans, on their western and southern borders, as steppe peoples against the Franks and Byzantium. (Kollautz: Die Awaren; Mitscha-Märheim: Dunkler Jahrhunderte goldene Spuren; op. cit.: Die Herkunft der Baiern; Zakyntinos: Byzantinische Geschichte; Randa: Handbuch der Weltgeschichte; Zöllner! Geschichte Österreichs: Haussig: Theophylakt's Exkurs über die skythischen Völker; etc.). The same authors unanimously state that the amorphous masses of Slavs were organized by the steppe peoples — first the Huns, then the Avars, and later the Bulgars — and they introduced them to the fundamental elements of warfare, statecraft, and culture. Several also state (e.g. Haussig) that the Slavic languages — especially Russian — are full of words borrowed from the Turkic peoples of the steppe.

The numerous parallel ~ North and South ~ Slavic tribal names that can be detected are evidence of the enormous size of the Avar settlements!

226

===== PAGE 223 =====

Down north, around Krakow, the "white" Croats, in Dalmatia the "red" Croats;

2. the Sorbs in the Oder-Elbe region, the Serbs in the Balkans;

3. in present-day Moravia, the Moravians, the Morvdi of Emperor Constantine the Great, who settled along the Morava River in the Balkans;

4. in the northwestern Carpathians, the Slovaks, whose Hungarian name is Tót, in the Dráva-Száva area the Slavonians - also Tótok in Hungarian (cf. Slavonia with its medieval name, Tótorzság) -, further west, in the Carantania-Krajna-Istria region, the Slovenes (cf. Slovakia 5. the Wends (a tribal group of Sorbs) living in the Oder-Elbe area) and the Wends - "Slovensko" (with the name): along the Drava.

According to these, the Avars carried out the dispersal of the Slavs according to conscious, well-considered military-political considerations. The more populous, stronger tribes, whose possible rebellions could have caused more serious complications, were simply cut in two: one part of them was left in their northern settlements, and the other half of the tribe was sent south, to the Avar grasslands of the Balkans.

In light of these historical and cultural facts, it would be essential to thoroughly review the hundreds of Hungarian words previously thought to be of Slavic origin, as it is highly likely that they are predominantly reverse borrowings.

We can see that raising the possibility of Avar transmission would have been a very delicate matter for Bartha, because it would have brought to light extremely unpleasant further connections that would ruthlessly expose the real scientific value of the "undoubted" findings of Finno-Ugric linguistics.

It should also be noted that the Russian hrynya is not only related to the Byzantine liter, but also to the ancient Babylonian gold standard, which in turn ultimately originates from the Sumerians. According to Lehmann-Haupt (Armenien Einst und Jetzt), it reached the territory of present-day Russia by Urartian-Scythian mediation as early as the 1st millennium BC, together with several other Urartian-Transcaucasian features, such as the niello technique (Tula silver) or the so-called "plank weaving" (Brettchenweberei).

For our part, we can supplement this data by saying that in the western regions of Europe, the first traces of plank weaving can be traced back to the Hallstatt period, which coincides with the Scythian period.

(see Brockhaus Enzyklopädie under the heading "Brettchenweberei"), the Hallstatt culture, however, had very close ties with the Scythian culture (Talbot-Rice: Die Skythen)

"We can therefore fully agree with Bartha that the hrynya issue is far from being closed,

227

===== PAGE 224 =====

To the traces of the Eastern European Slavs in the Regeszetli

On pages 37-38 of Könyvenck, Bartha describes the settlement called Staraya Ladoga, located on the southern shore of Lake Ladoga, essentially as follows: the iron harrow-plough tooth found there ... "Is the earliest known ploughshare in the East Slavic area." (note 273 on page 37) "Most of the jewellery is of the same nature as the Slavic products from the Dnieper region." (p. 38) The buildings show surprising craftsmanship." (p. 38) Then he concludes his description as follows:

"The large community houses discovered in Staraya Ladoga ... are unknown in the East Slavic areas, but similar ones are found in Scandinavia and in former Norman settlements in general. Scandinavian products, even a stick covered with runes (signs) were found... The Nordic trait can also be recognized in the anthropological character of the Korabcl population of the northern Russian areas... we must also take into account a significant Finnish population." (p. 38)

In Hungarian, the settlement was Norman-Finnish. This is not only stated by us, but also by one of the most renowned experts in Viking research, E. Oxenstierna, who also reports "Old Ladoga" as a Norman settlement on his Viking find map. (The Vikings)

However, Bartha does not mention this at all, but instead reiterates that there were He talks about iron plows found in "Slavic territory", "Slavic-like" jewelry, and the like. Yet it is a well-known fact that

no Slavs on the southern shore of Lake Ladoga in the 8th-9th centuries. At that time, Finno-Ugric tribes lived there, which he himself finally mentions under compulsion, 15 but only in a "they were still running" way: "... we must also count on a significant Finnish population." The Norman colonists settled on these, bringing with them the iron found in Germany and Scandinavia, which had been known since Roman times.

After these experiences, we must already have reservations about the Dnieper region jewelry page, because — speaking of these southern, "Slavic character". We are not disappointed in our suspicions. In 40-41.

Dnieper region Slavic areas — Bartha describes a settlement in detail:

One of the small settlements in the territory of the former Severian tribe, near the present-day village of Novotroitskoye, was located east of Kiev, on the banks of the ... Pszol River, near the village and lake called Lebegin. The settlement arose around the turn of the 8th-9th centuries, and, like other settlements in the densely populated region of that time, it was destroyed at the turn of the 9th-10th centuries and was never rebuilt."

He then tells us that the settlement had a blacksmith shop, and several ironworks have even been discovered. The precious metal industry was also significant, with two goldsmith shops found. The silver jewelry was probably made of dirhams; the weight of the jewelry is an almost exact multiple of the weight of the dirhams. We also learn that the inhabitants of the settlement practiced advanced agriculture; not only did they

===== PAGE 225 =====

not only ploughs but also hoes, scythes and sickles were found, "These ploughs made it possible to cultivate the steppe lands with bound soil surrounding the settlement"; - he writes. They produced soft and hard wheat, rye, barley and millet, The hoes prove the existence of garden farming. Among the draft animals, cattle and horses were in the first place, but sheep and pig breeding were also significant. He then notes: "The inhabitants of the settlement were in contact with the peoples of the Saltovo-Mayak culture, i.e. the Khazar Kaganate, this is evidenced by the Saltovo-Mayak type finds. We do not find Scandinavian artifacts, however." Finally, he writes: "At the same time as the destruction of the Slavic settlements in the southern zone, the settlements along the banks of the Don and Donets rivers in the territory of the Khazar Kaganate were also depopulated with tragic suddenness."

This description is again a classic example of the one-sided approach of our historical research. The settlement in question was, with a probability approaching certainty, one of the villages of the Lebedia Hungarians,

Let's look at why in detail, point by point:

1. It lay on the steppe /surrounded by "steppe lands with bound soil", precisely in the area where contemporary Arab sources describe the residences of the Hungarians.

Next to it, there is still a village and lake called Lebegyin.

It was built around 800, around the time of the Hungarians moving to Lebedia.

It was destroyed or depopulated around 900, exactly at the time of the conquest, and was never rebuilt.

. Other settlements in the region, which was densely populated in the 9th century, also became depopulated at the same time as the Hungarians moved west.

. Close connections with the Saltovo-Mayak culture, i.e. a strong Khazar orientation. At the same time, there is a lack of Viking-type finds, i.e. no Norman influence. The former undoubtedly points to Hungarians, while the latter excludes Slavs under Varangian rule.

7. A very developed iron and especially precious metal industry can be demonstrated, with predominantly silver jewelry, just like the Hungarians who conquered the country.

8. The level of development and nature of agriculture are identical in every way to the Khazar and Bulgar-Turkish farming of the same period in the steppe, which, however, did not differ in any way from the farming of the conquering Hungarians,

9. The proportion of domestic animals, cattle -y and to a lesser extent horses r The leading role in animal husbandry corresponds to the type of animal husbandry of the semi-settled, large-animal-keeping Hungarians who conquered the country. u BPWYHY

sának, 10. Gyula László's excavations in the mountains prove that the conquering Hungarians also had permanent settlements,

229

===== PAGE 226 =====

Despite everything, Bartha does not even mention as a possibility that the settlement could have been Hungarian, and we do not even find any reference in his book to the possibility that this assemblage of finds could be somehow connected to the Hungarians - at least indirectly.

Before we analyze this question further, let us also look at note 338 on page 44, because it is closely related to the "Slavic" settlement near Lebegyin, but also to Bartha's characteristic, unified approach to history: "It should be noted that the occurrence of a non-Khazar kagan in the early 9th century cannot be separated from the problem of the identity or difference between "Rus" and Slavic." Bartha's remark refers to the fact that we have data on the title of "kagan" of the Kiev princes from the 9th century.

Khagan of the Kiev princes of How the title "Khagan" is related to the Russo-Slavic problem can undoubtedly be a very important question for Russian historical research. However, we would be much more interested in how the title of the 9th century is related to the issue of the Hungarian principality of Lebedia and Etelköz that immediately preceded the conquest of the homeland. Many people have already written about this, for example, G. Vernadsky, later mentioned by Bartha, in his treatise "Lebedia". Vernadsky compiled those geographical names that still exist in southern Russia today, which, in his opinion, clearly localize the places of residence of the Hungarians at that time. We present these names due to their paramount importance. Lebedin: a locality in the Kiev province, east of Kiev. Lebedin: a locality in Podolia.

Lebedyan: a town in Tambov Governorate, on the Upper Don.

Lybed: river near Kiev.

Lybed: river near Chernigov.

Lybed: river near Ryazan.

Ugra: river, a tributary of the Upper Oka.

Ugrin: river near Kharkov; tributary of the Donets.

Ugorskoye: area in the immediate vicinity of Kiev.

10. Kut-Suezkov: a village in the Kharkiv region.

11. Krasny-Kut: a locality in the Kharkiv province.

From these names, Vernadsky concludes that there were two major Hungarian centers:

- 1. the Don-Donets basin;
- 2. the region of the middle and lower reaches of the Dnieper.

So, a large area – he writes - extended from Podolia to the Upper Don, the Donets region and to the Oka. He especially emphasizes that there was more than likely a Hungarian settlement in the immediate vicinity of Kiev.

It is also of interest to us that the word "voevoda", meaning "sherceg", can already be traced back to the Khazars.

230

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===== PAGE 227 =====

His most significant finding, however, is that from the 840s or 850s, Álmós ruled in Kiev, and around 856 Askold and Dyr appeared in Kiev, and although they ruled there, they did not rule independently. All signs point, writes Vernadsky, to their being vassals. When, around 882, Oleg killed Askold and yyyyy, their bodies were also named "Ugorskoye" according to the Kiev Chronicle.

They took him to Olma's palace, on the hill now called Ugorskoye. And "Olma" is none other than Álmós. His palace bears witness to this.

After this, we can have little doubt as to why the prince of Kiev was called "Khagan" in the 9th century.

However, from Vernadsky's work — as an addition — we can also see that the evidence of geographical and toponyms localizes the 9th-century settlements of the Hungarians to exactly the same areas, which — as Bartha writes — he calls "the Slavic settlements of the southern Dnieper region" and "the settlements "they were depopulated with tragic suddenness around the turn of the 9th and 10th centuries", but which he, with careful wording, along the banks of the Don and Donets rivers in the territory of the Khazar Kaganate". Yet, in 1957, Vernadsky could not have known about the excavations whose conclusions were clearly confirmed: the settlements in the areas he had determined as the Lebedian settlements of the Hungarians were depopulated with tragic suddenness, simultaneously with the conquest of the homeland.

But we cannot ignore the last i-dot: Vernadsky's place name "Lebedin" in the Kiev governorate is exactly the same "Lebegyin" next to which the previously described settlement — according to Bartha, Slavic — was excavated.

In this connection, we should also mention the statement of Nándor Fettich, from his work "The Metalwork of the Conquering Magyars". Fettich states that a great many Hungarian equestrian graves were found in the territory of the former Norman-Russian state of Kiev. But many of the tarsoly plates and other characteristically Hungarian-type finds from the time of the conquest were also found in the territory of the Kiev state. This indicates that Hungarians also lived in the territory of the Kiev state. From the development of metalwork, it can be read that, parallel to the appearance of the Hungarians in the 9th century, gold and silverwork of southeastern origin, Persian-Saracenic in orientation, spread all the way to Kiev. Since in the 9th century no other equestrian nomadic people had such magnificent, rich equipment made mainly of silver as the Hungarians, only the Hungarians could have brought this metalwork with them, and that from the direction of the Caucasus. The tarsoly plates of the time of the conquest only occur in large numbers in Hungary and in the middle Dnieper region: The place of origin of this metalwork is therefore Levedia Eg, the territory of the Kievan state. The Hungarians, by all indications, lived in peaceful relations with the Normans, and even mixed with them. This is confirmed by

231

===== PAGE 228 =====

that many Varangians came with them to the Kárpát Basin, this is clearly proven by the strong North-Germanic incrustation of the skulls in the cemetery of Hencida during the conquest. All these circumstances indicate – writes Fettich + that the Kievan state was under Hungarian rule in the 9th century, and even later, the Hungarians ruled in the area of Saltov.

The same conclusion was reached by the Soviet archaeologists Zakharov and Árendt in their aforementioned treatise. (Studia Levedica, 1934.) The Saltovo culture discovered at that time is definitely called the legacy of the Lebedian Hungarians. They state that the swords from Saltovo and the North Caucasus, Koban region are closest to the Hungarian weapons of the conquest period. The Saltovo culture appeared at the end of the 8th century and disappeared at the end of the 9th century - they write. It is by no means a Khazar culture, because it is absent from the very center of the Khazar Empire. It surrounds the Khazar center of the Lower Volga in a huge semicircle: from the middle Volga, through the upper reaches of the Don, Donets and the Kuban region to the eastern half of the Caucasus, to the Kuma River. The finds of horse-riding nomads in the Volga-Kama region are also associated with a group of Hungarians, who, according to them, subdued the Finno-Ugric people there and established tax-collecting settlements among them. Another group of Hungarians lived in the Kuban-Caucasus region. Very important in this respect is Rubruig's statement — they write — that they were also masters of the situation in the lower and middle reaches of the Dnieper.

who found Hungarians in the city of Magyari on the Kuma River even in the 13th century. Finally, it is established that the Hungarians were in the 9th century

It is very instructive to compare Zakharov-Árendt's reasonable, clear, and above all, completely consistent with the historical situation at that time, findings with the uncertain, nebulous ideas of today's Hungarian historical research.

"Unfortunately, there was no place for the Hungarian tribes on the archaeological maps yet," writes István Erdélyi, for example, in his article "Archaeological Relics of Ancient Hungary in Eastern Europe." (Hungarian Archaeology. Study.) In light of the above, we would rather say that "there was no place anymore."

The Saltov-Mayacki culture was in the meantime divided into Bulgar-Turkic, Alan, Khazar and archaeology; in: Magy. Östort.

unknown steppe" variants. (Fodor: Our Bulgarian-Turkish loanwords and the

tanumum.) Of these, the Alan and the Esik, which were depopulated with tragic the "unknown steppe" group in its entirety, and the Bulgarian-Turkish section largely covers precisely those areas

suddenness at the end of the 9th century, while the Khazar version is found in the area of the city of "Magyari" on the Kuma River, where, according to Rubruk, Hungarians still lived in the 13th century, who, together with the Alans, formed into free bands, killed the Mongols every night. (Bendefy: The Caucasian Homeland of the Hungarians.)

We know very well that it is not possible to determine with certainty from archaeological finds alone what ethnic group they are. But not here. about this

232

===== PAGE 229 =====

It is not about the fact that the Hungarians undoubtedly lived in Southern Russia in the 9th century, so their memories must also be there. The Saltovo-Mayack culture is the only archaeological culture in Southern Russia for which it can be shown that life ceased on about half of its territory by the end of the 9th century, i.e. exactly at the same time as the conquest.

Behind all these confusions — even if unspoken — lies the doctrine of our Finno-Ugric origin. Our archaeologists and historians know very well that we must seek the legacy of the Hungarians among the Szaltov-Majac culture. This is undoubtedly clear from the lines of every relevant article and study. But the linguistic-historical hypotheses of Finno-Ugric studies stand before us like a ban, which our linguists state as solid, irrefutable facts. In this case, our alleged Bulgarian-Turkic, Ossetian (Alan) and East Slavic loanwords, which are considered to be from before the occupation. These peoples — according to the prescriptions of their linguistics — must be given a place without reason, right next to the Hungarians, so that the adoption of these loanwords can be explained. Thus, the areas of Saltovo-Mayacki culture were divided up among Alans, Bulgar-Turks, and Slavs, until finally there was no room left for the Hungarians. Now we can see why Bartha had to call the steppe settlement near Lebegyin, which proved to have developed agriculture and animal husbandry, Slavic: this perfectly explains the words "scythe", "hoe", and even its alleged Slavic origin.

"bit" and other horse breeding related words

As for the "undoubted" linguistic historical statements dealing with sound laws and regular sound changes, we will now only recall the linguistic historical determination of the age of the Rigveda and the Avesta, also made using the same "undoubted" methods, which have certainly been blatantly disproved by the facts in the meantime.

We discuss these linguistic issues in detail in our Volume III, but in order to better illuminate the connections, we must address them again in this volume.

On the connections between Sabir, Khazar, and Hungarian folk names

On pages 82-84 of his book, Bartha discusses the historical relations between the Hungarians and the Khazars. He describes the various historical views on this issue. He also briefly refers to the fact that a fraction of the Hungarian tribes migrated towards Persia, but he does not even mention the older name of the Hungarians, "Sabyr". Similarly, in relation to the Khazars, he is completely silent about the fact that according to Masudi, the original, real name of the Khazars was "Sabyr", the Haskaltak.

"Khazar" is just a Persian nickname, which the Khazars themselves never used.

233

===== PAGE 230 =====

For Bartha, the verifiable agreement of these three folk names with contemporary historical sources is obviously an unimportant side circumstance that is not even worth referring to in the notes. Why? Again, based on "linguistic-historical considerations": the Hungarians originate from the forest belt, and the name Sabir indisputably points south, to the areas behind the Caucasus, as we have already mentioned. In such cases, according to the distorted logic of the Finno-Ugric theory, contemporary or nearly contemporary sources can be neglected, that is to say, they can be ignored.

Yet the Sabir question is in all probability one of the most important key questions of our prehistory. This is proven, for example, by the results of Gyula Németh's research, which he outlined in his work "The Formation of the Conquering Magyar Nation". He states that Maszudi was not mistaken, because the Persian meaning of the word "Khazar" — "lost, wandering" — really corresponds to the meaning "lost, wandering". He emphasizes that after AD 558 the ethnonym "Sabir" disappears from the sources, and around 580, within the Turkish, in the southern Sabir regions. It inevitably follows from the fact that we must reckon with at least 300, but probably more likely 400 years of very close Sabir-Hungarian contact, and even coexistence, and that we must start from this in further research on the Khazar process of the Hungarians. There is no doubt that the Hungarians were part of the former Sabir tribal alliance — this is clearly proven by the name of Emperor Constantine as "Sabartoi" — and later also within the framework of the Khazar Empire.

In his book "Attila and the Huns", Gyula Németh provides another, very important source of information regarding the ancient name of the Hungarians, "Szabir": Victoris' chronicle calls the Sabirs "Ugni" (from 515 AD). This "Ugni" — occurring uniquely — can only be identical to the ethnic name otherwise written as "Ugr".

It is clear from what has been said so far that Sabir-Hungarian relations, or rather the issues of at least partial ethnic identity, cannot be ignored in our prehistoric research.

About our Bulgarian-Turkish loanwords

Bartha begins his discussion of the economy of the 9th-century Hungarian tribes based on our Turkish, Ossetian (Alan) and other Iranian loanwords, and in the following he builds the entire system of our 9th-century economic life on these words. However, it is beyond doubt," he writes, "that the Turkish and Iranian borrowings were mostly made between the second half of the 6th century and the 9th century.

"It happened between the end of the 19th century." (p. 84) In support of this statement, in note no. 27, he refers to the paper by Z. Gombocz: "Die bulgarisch-türkischen Lehnwörter in der ungarischen Sprache" (1912), noting, incidentally, that the sign-234

===== PAGE 231 =====
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written in all capital letters, like this: "Die Bulgarisch-Türkischen..." But this is not important, it could also be a printing error. What is important, however, is the appearance of the linguistic prohibition as a categorical imperative, which we just referred to: Well, there is a lot of doubt about this. But it is beyond doubt..."

The renowned German Turkologist, J. Benzing, in his article "Die angeblichen bolgar-türkischen Lehnwörter im Ungarischen", flatly denies the Bulgarian-Turkish origin of Hungarian words considered to be loanwords,

Benzing states that the Muslim-Turkish scholar Mahmud Kashgari, in his linguistic work (Divan lugat at-Turk) written in 1073, did not yet know about the sound transitions -ʳ Résez + (l) in the Turkic languages. Brief summary of the article:

Since, based on the above data, we have every right to state that the Z > R and A > Y(l) sound changes in Bulgarian did not occur before the 11th century, the Hungarians "The loanwords that were previously considered Bulgarian could not have been taken over from the Bulgarians, because at that time they no longer had any contact with the Bulgarians." These are indeed very strong arguments. From a language that only began to use the sounds R and Y(T) in question towards the end of the 11th century instead of the earlier Z and A, it is hardly possible to borrow words with the sound form R and Y(D) between the 6th and 9th centuries. We do not know that Hungarian linguistics has refuted Benzing's above proof. All that can be stated without a doubt is that even in recent years, only Gombocz's 1912 article is referred to on this issue. (E.g. Fodor, in his already mentioned article in Magy. öst. talumm.). Therefore, they are probably not familiar with Benzing's article. In any case, it is very difficult to imagine how Benzing's argument and source evidence -- which is, moreover, a contemporary linguistic work -- could be refuted in a meaningful way. However, we can state one thing with a clear conscience after this, namely that the adoption of our so-called Bulgarian-Turkish loanwords between the 6th and 9th centuries is not an undoubted fact. Consequently, any conclusion based on this unproven linguistic assumption, whether regarding our economic relations in the 9th century or anything else, can only be a chain of hypotheses moving in a vacuum, nothing more.

We should also note that our linguistics itself is quite uncertain about the question of the words we consider to be Turkish. They cannot satisfactorily explain the strange fact that one group of these words shows Western, and some Eastern-Turkic characteristics, but the majority of them cannot be fitted into the phonetic system of either the Eastern or Western Turkic languages (Ligeti: The Ancient History of the Hungarians).

235
===== PAGE 232 =====

These data, compared with Benzing's above statement, make it very conceivable, even within the framework of academic linguistics, that our so-called Turkish loanwords are loanwords not in Hungarian, but perhaps in the Turkish languages. Incidentally, this was pointed out at the turn of the century by some of our excellent researchers, for example Károly Fiók. (1. later.)

The Iranian origin of the steppe peoples
Bartha's following statement is also related to similar linguistic issues: "The possible Iranian origin of our word 'cow' fits into the environment in which various peoples of Iranian origin played a very significant role." (pp. 84-85)

First of all, on the sidelines of the Med question — as if leading to absurdity — it seems fitting to mention M. Mayrhofer's article "Die Rekonstruktion des Medischen" (1968), in which the author attempts to "reconstruct" this practically completely unknown language from various Iranian dialects — as the title suggests. Regarding the question of the Iranian-ness of the Scythians, we should mention some of the statements in Géza Nagy's article "A Few Words About the Scythians". Nagy states that the surviving Scythian words can either not be derived from Aryan languages at all, or can only be derived through linguistic acrobatics. Only a quarter of the personal names can be explained from Iranian languages, and even of this quarter only a part is undoubtedly Iranian. Classical sources never call the Scythians relatives of the Persians. Moreover, the Persians themselves write about the later Turks that they are people of the Saka, i.e. Scythian race.

We cannot go into further detail about Nagy's arguments now, but in this paper, and later in his larger treatise "On the Scythians" (this was his academic chair), he presents a series of evidence against the Iranian-ness of the Scythians. In the first volume of his monograph "Byzantinoturcica", Gyula Maravcsik emphasizes that the statements of Herodotus, Hippocrates, and later Pliny about the Scythians point to Turanian peoples, not Iranians. According to Pliny, the name of one of the Sarmatian tribes was "tyrcæ". — It is characteristic how the Indo-Germanic historiography "evaluated" this data. In no case Turks" (Osteuropäische und ostasjatische Streifzüge). With this "statement", the then Indo-Germanist Marguar simply stated that, according to his methods, the question was settled.

F. Altheim writes in his treatise "Das Alte Iran" that the most ancient texts of the Avesta, the "gathā", are not understood even today because they are written in a "very simple, syntactic" language. He wrote it together with R. Stiehl

236
===== PAGE 233 =====

And from his book "Geschichte Mittelasien im Altertum" it is also clear that the Avestan Gīrāni language is almost inextricably intertwined with the ancient Turks.

We can rightly ask the question: what scientific evidentiary value do the various "Iranian" people and personal names, mostly derived from Avestan names, have, for example, among the Scythians, Sarmatians, Sakas or Parthians, which are practically the only evidence of the Iranian-ness of these peoples? But let's take a closer look at some of these "Iranian" etymologies from Altheim-Stiehl's mentioned book. "Massageta" = "People of the Tower", "zum Turm Gehörige", i.e. "people of the tower", "belonging to the tower" (id. m. p. 129). The background of the etymology: the Tolstoy expedition in Khorezm discovered fortifications dating back to the 1st millennium BC, with towers at the corners. (Tolstow: Auf den Spuren der altchoresmischen Kultur) It is therefore obvious — at least seen through Indo-Germanic lenses — that in the 1st millennium BC, also people living around the Aral Sea Massagetae could only be "tower men". — method" we could even explain the name of the Hungarians "hungri": "hungriffes". (Cf. the late Avar, early Hungarian "griffes" bronze medals). It is incomprehensible that our researchers for this They haven't thought of an "obvious" solution yet. -Or is something like that only allowed in Indo-Germanic studies? "Aspasiak"-aspa-syā-kaz "those who rejoice at the horses", so if anyone objects to this "who rejoice in the steeds" (id. m. p. 453. note no. 10). etymology, they can provide something else. Since Byzantine Stephanos writes this folk name as "apasiakas", the second possibility is "+apa-syaka" = "those who rejoice at the water", in which interpretation Altheim generously allows a free choice between the waters of the Oxus River (Amu Darya) or 2/2 6 (1. id. m. p. 453. no. 10., note).

For these hair-raising absurdities, we also need to know that the ethnonym "Aspasiak" first appears in the Aral Sea area around 230 BC. Then, about a hundred years later, also from the Aral Sea region, the peoples called "Asi" and "Pasiani" or "Pasiak" (Strabon) move south, whose names the "Aspasiak" gives in 826 - 2 years earlier -- are obviously combined: "as" + "Pasian", or "as" + "Pasiak" = "Aspasiak". Presumably, they could have formed a tribal alliance at that time. However, this clear historical data did not prevent the Altheims from etymologizing the name "Aspasiak" by breaking it down into the syllables "aspa", "sya" and "ka", even if the result was absurd. The main thing is that they managed to find an "Iranian" etymology. -- Lät-237

===== PAGE 234 =====

We can also conclude from this that the methods of word derivation in Indo-Germanic studies have not changed much since Zeuss, Pictet and Müllenhoff. Let us recall how O. Schrader thought about such etymologies as early as 1883 (Vol. I, Chapter 2). In the same book, the authors also provide the Iranian etymologies of the month names of the Sakas who migrated to Eastern Iran. These month names have survived to us in the records of the Khorezmian-Arab author Biruni. It will be enough to list these word explanations only in Hungarian translation, because they speak for themselves:

1. "King Kavād's Snow"
2. "The month of the "Rah River" (meaning the Volga — the southern part of Afghanistan! --, saying that its Mordvin name is probably "from the Iranians, Rawa, which in turn was taken over by the Mordvins, etc., etc.) "A month rich in

"The snow of Irā, the god of King Kayan"	"Month of united horses"
"A month with bigger horses"	
Turar city snow"	
"Snow of Aria Province"	
. A month equal to a horse"	
10. "High Priest Snow"	
11. "Prince Karasanay's snow"	
12. "Saurva démon hava"	

We believe that further commentary is unnecessary. Therefore, we will only point out the palpable "cavalry neurosis", which is a common phenomenon in Indo-Germanic historiography. In all probability, this is a late overcompensation of the atavistic fear of horsemen of the foot Indo-Germanic people. They tried with all their might to "horse-train" almost all ancient Indo-Germanic — or supposed — peoples, and even the taming of horses, harnessing them to carts and riding horses were considered Indo-Germanic inventions until recently. They flaunted these supposed Indo-Germanic cultural achievements at every possible and impossible opportunity. The Indo-Germanic "chariot knights" (Streitwagenritter) formed a solid foundation for various ancient Indo-Germanic migration theories for decades, and there were even historians who referred to the "chariot outlook on life" or "chariot worldview" (e.g. E. Wahle) in this connection as "Streitwagengesinnung", which means

"Die Indogermanisierung Mitteleuropas" 1954.). While F. HanCar proved that neither the domestication of the horse, nor the use of chariots in combat, nor horse riding can be demonstrated among the ancient Indo-Germanic peoples (Handar, Das Pferd... etc. 1956.)

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238

===== PAGE 235 =====

The mythological manifestations of the aforementioned atavistic fear can also be found in the centaur tales spread from Greece to India. This has been repeatedly pointed out by Géza Nagy in his works "A Few Words About the Scythians" and "About the Scythians". But this is also confirmed by the fact that - as we could also notice from Altheim-Stiehl's etymologies - they never speak simply of horses (Pferd), but always of stallions (Ross).

In K. Jettmar's book "Die frühen Steppenvölker" we can read that the only evidence of the Iranian-ness of the Scythians is a few "Iranian-sounding" (sic!) names. The Sarmatians are considered (!) Iranian because, according to Herodotus, their language was similar to the Scythian language.

It is characteristic, however, that at the same time the Scythian common words reported by Herodotus are considered to be errors because they cannot be reconciled with words of the corresponding meaning in any Iranian dialect.

M. Ebert writes the same thing in his work "Südrussland im Altertum".

We also learn from F. Altheim (Weltgeschichte Asiens im griechischen Zeitalter) that the Parthians were similarly included in the great family of Iranian horsemen: although we do not know their language, since Apollodoros writes that the Parthian language was between Scythian and Median, they could only have been Iranian. From this we can also see why the Medes also had to be "Iranianized".

In his treatise "Theophylact's Exkurs über die Scythian Völker", HW Haussig states that the population of the Kushan Empire, which is generally considered to be Iranian, was very mixed and contained many Altaic elements.

It is worth noting that the Kushan Empire was formed by the Saka and other horse-riding peoples who migrated from the northern steppes to Bactria, the territory of present-day Afghanistan. We will return to this question later.

Well, this is how those "Iranian peoples of the steppe" look in reality, whose existence Hungarian historiography considers as an indisputable, undoubtedly proven fact, and builds the cultural-historical concept of the development of the Finno-Ugric and then the so-called Ugric-Hungarian peoples on the words allegedly taken from them.

We cannot therefore accept the chains of our historical science and linguistics; neither with regard to the prehistoric hypothesis based on the a priori assumption of different "Iranian origin" steppe peoples and linguistic relations of the Hungarians nor of the other Finno-Ugric-speaking peoples. And not until the Iranian origin and Iranian languages of these steppe peoples -- from the Scythians to the Alani -- have been proven at least to the highest degree of probability. It is already obvious from the data presented so far that we cannot speak of evidence at all in this question, but are only confronted with the groundless hypotheses of 19th-century Indo-Germanic studies. And as the basis is, so is the development.

===== PAGE 236 =====

building. More precisely: the walls can withstand only as much as the weakest brick of the foundation. However, going further, numerous data directly indicate that this

in most peoples - both before and after Christ - much we should see peoples of an Altaic nature rather than Iranian ones.

As early as 1904, when the "Steppe Indo-Iranians" still ruled Indo-Germanic history without competition, O. Franke established, based on data from Chinese sources, that the original homeland of the Turkic peoples was in Central Asia, more specifically in Turkmenistan, and extended to the Caspian Sea. He also pointed out that the alleged Inner Asian Indo-Germans were all Klapproth's discoveries, and that these have been uncritically propagated in the literature ever since, without any evidence. (See also the relevant part of Chapter 2 of the following volume.) The names and titles of the Sakas who invaded Northern India in the 1st century BC are predominantly Turkic, and the Hindu sources themselves call them "Turuska". Likewise, the Bactrian descendants of the "Yüecs" people, the Kushans, also bore Turkish dignitary names - he writes. (Franke: Contributions from Chinese Sources to Knowledge of the Turkic Peoples and Skyths of Central Asia.)

We note now that almost all new data that has come to light in this field since 1904 confirms Franke's views.

Liu Mau-Tsai In his book "Die chinesischen Nachrichten zur Geschichte der Ost-Türken" (1958), he states that according to Chinese sources, the Turks were a separate branch of the Huns. Their original ethnic name was "a-si-na".

In connection with this data, it is necessary to point out that this ethnic name has a remarkably close phonetic parallel with the name of the aforementioned "ashi" ("Asian") people who migrated to Bactria around 130 BC. However, European historiography considers this people to be Iranian in the first place and, as we will see later, this name in particular serves as the basis for assuming that the "Az" and Alans were Iranian. It is very likely that this people called "ashi", "Asian" is identical with the "a-si-na", the Turkic people who used to live in Turkmenistan. All the more so because Liu Mau-Tsai also points out that the ancient Turkic tradition placed the Öshaz "beyond the western sea", by which sea is meant the Káspi or rather the Aral Sea.

This probability is also largely supported by the fact that we have written evidence of the presence of Turkic peoples in Turkmenistan from the time of Alexander the Great: the brother of the then king of the "Scythians", i.e. Sakas, living north of the Jaxartesztol (Syr Darya), bore the name "Carthasis" = "Kara-Tas" (A. Gutschmid: Geschichte Irans und seiner Nachbarländer von Alex: ander bis zum Untergang der Arsaciden.)

W. Eberhard, in his treatise "The Culture of the Old Central and West Asian Peoples According to Chinese Sources" (1941), also writes that the Turkish (Altaic) homeland "reaches the limits of certainty, the Wallachian-249

===== PAGE 237 =====

nüsegel" was in the territory of Turkmenistan. At the same time, he also emphatically points out that an ancient culture has been continuously detected in Turkmenistan since the Neolithic Age, which was closely connected with the pre-Asian civilizations of the same period. The names Anau, Merv, and Khorezm indicate the development; but many facts of the early history of Pre-Asia can only be explained if we take into account Altaic influences since the most ancient times. Regarding the question of the alleged presence of the Indo-Germans in Central Asia and the steppes, he states that this is one of the most complicated prehistoric problems. The only thing certain about the various ancient Turkmen and East Iranian (Bactrian, Sogdian, etc.) peoples is that they did not differ from the Hunnic and later Turkish peoples in their lifestyle, customs, and largely in their anthropological character. But anyway, the Hunnic influences from the 1st century BC onwards were not very different from the Hunnic and later Turkic peoples. From 200 to 150 AD, they dominated the entire steppe, from Mongolia to the Parthian Empire.

We cannot leave the name of G. Hüsing without mentioning either, who in his article "Die Urbe- völkerung Irans" directly accuses historiography with an Indo-Germanic perspective of falsification.

They go so far, he writes, as to speak of a pure Aryan population in Iran during the Old Persian period, even though contemporary sources prove that the population was very mixed. Before the Aryan immigration, Iran had a dense and cultured non-Indo-Germanic population.

Hüsing's views -- in other respects as well (1st following volume, chapter 2) -- have since been fully confirmed by archaeology and historiography. And from the point of view of our present question, we can draw the following lesson from this: in the times before Christ, in the Scythian-Sarmatian period, let alone in the steppe, but even in Iran itself, we cannot speak of a pure Iranian population.

Hüsing's insight can only be truly appreciated if we compare his opinion with the results of modern research. In Altheim's "Weltgeschichte Asiens im griechischen Zeitalter" we read that the urban culture of eastern Iran (Bactria, Sogdia) of the 1st millennium BC was not Indo-Germanic. Bactria and most of the city names cannot be derived from Indo-Iranian languages. The pre-Indo-Germanic indigenous population made up the lion's share of the population of the region. Likewise, the population of Luristan in western Iran was not Iranian in the Achaemenid period. The Luristan bronzes have nothing to do with the Iranians, because they undoubtedly date from the times before their appearance, and disappear simultaneously with the appearance of the Persians. The equestrian graves of the B-necropolis of Tepe Sialk, which are generally believed to be associated with the appearance of the equestrian nomadic Iranians, are also the remains of this pre-Indo-Germanic population.

It is necessary to point out here that these are the find assemblages defined by Altheim as pre-Indo-Germanic, but Bactria itself, in the earlier Indo-Germanic historiography, was already included as "Iranian" finds and a tribal area, and to a large extent served as the basis for the modern-day AI

===== PAGE 238 =====

gas to the Iranian civilization. Some orthodox Indo-Germanic historians and cultural historians still believe this today. For example, R. Ghirshman (Iran I.) considers the Luristan bronzes to be Iranian products, and discusses for long pages the "Iranian" world of ideas he has read from them. But in the end he must admit that according to the vast majority of researchers they are the legacy of the Kassites. (1. also see Chapter 2 of Volume I.)

When discussing these questions, we should not forget György Almásy, who already in 1902 -- i.e. simultaneously with the works of Franke and Hüsing -- clearly recognized the close ethnic and historical connections of the Hunnic-Turkic peoples of the steppe, but also the improbability of dating the Aryan migrations too high. In his paper

"Centralasien, die Urheimat der Türkvvölker", he states that the migration of the Indic peoples cannot be dated earlier than 1000 BC. Around this time the Aryans appeared in India, and somewhat later the Iranians in Armenia and Persia. It cannot be a coincidence, he writes, that the attacks of the "Hyung-nu" people -- that is, the Huns -- on China began almost exactly at the same time. These two events are undoubtedly connected.

We also recall the relevant pages of Németh Gy.'s book "Attila and his Huns": -- Although according to Németh, the comparison of the folk names "kui", "jyüü, . . . hun", or "hun-jü", "hien-jü", , hien-jün" appearing in Chinese sources in the 11th-9th centuries BC with the later (Han period) names "hun" or "hiung-nu" is -- as he writes -- "linguistically unacceptable", However, we believe that such sound development hypotheses should only be assigned a very subordinate role compared to the evidence of historical sources.

The Aryan migration pushed some of these ethnic groups, which could be called ancient Turks or ancient Altai, eastward, Almásy continues. The homeland of the Altai (Turk) people is most likely to be found in Central Asia. This is also confirmed by the fact that the Iranians fought against the Altai-Turkic peoples throughout their history (the opposite of Iran and Turan). The Scythian or Saka peoples living east of the Volga can therefore be considered Altai-Turkic ethnic groups as early as the 1st millennium BC -- at least for the most part. Moreover, based on linguistic correspondences, the Sumerians also formed a branch of the ancient people called Altai today.

We can only say the same thing about Almásy's objective conclusions, which take into account contemporary sources and are based on them, as we have already stated above in relation to Hüsing: his results achieved through thorough analyses, which were unanimously condemned by the entire international scientific community at the turn of the century, were effectively supported by later research in a remarkably large number of cases.

In connection with the Scythian question, we must separately address the fundamental insights of Gyula Mészáros (History of the Peoples of Eastern Europe II. 242
===== PAGE 239 =====

+ Chatti and Scythians), because these also shared the common fate of other research results that were not exclusively Finno-Ugric in nature: they were put on the back burner by the trend of our Finno-Ugric origins, and they are still systematically ignored today.
Through thorough source studies, name comparisons and in-depth analyses of artistic motifs, Mészáros revealed - already in the late 1930s - proto-Asian-Scythian connections of such eminent significance, which have since been largely confirmed by archaeological research.
He showed that starting from 1300 BC, migrations from south to north began from Asia Minor and Northern Mesopotamia through the Caucasus to the Kuban-Terek-Koban region. Skeletal remains from this time at Koban, near the Kur-Araxes, and in Cappadocia prove the close coexistence and mixing of the same long-headed and short-headed peoples. These migrations reached their peak in the 7th-6th centuries BC. Huge masses of pre-Asians flooded the Kuban-Don region and the northern coast of the Black Sea. Their culture is organically integrated, its main element is the general use of iron. Contemporary sources clearly prove that these peoples were the Scythians. The Scythians, therefore, in the 13th century BC They lived south of the Caucasus before the 7th century, Mészáros states.

The surviving Scythian place names are often found in identical phonetic forms in Asia Minor and Transcaucasia, sometimes dating back to the cuneiform Hittite times.
We also find illustrative documents for determining the starting point of the Scythian migration in the Asia Minor parallels of Scythian personal names. Identical personal names with identical word meanings appear in both areas, which in Asia Minor mostly originate from the characteristic, pre-Hittite proto-Hittite layer.
Among others, the names of the first known Scythian kings, "Partatua" and "Madyas", are undoubtedly ancient Asia Minor names. (V. ö. König: Die älteste Geschichte der Meder und Perser; — described in the next volume, chapter 2)

He then emphatically points out that the four Scythian social strata mentioned in ancient sources correspond exactly to the division of Babylonian, Assyrian and Hattian societies.
He states that the Hittite place names, river names, mountain names and province names are mostly derived from the language of the indigenous Hattian population. Likewise, most personal names, even the names of the Hittite kings. In the language of the Hittite court ceremony, among the names of dignitaries, in the texts of the religious cult, there are also a lot of Hattian words, and even most of the names of gods are of Hattian origin. All this clearly proves that the decisive ethnic factor of the Hittite empire was the indigenous Hattian people.
around 1200, the so-called "Late Hittite", i.e. After the invasion of the "Sea Peoples" in Cappadocia, northern Syria, for another 500 years, the catastrophe
243
===== PAGE 240 =====

The Hattian state existed. The onomasticon (names) of these countries is also predominantly in the Hattian language. The Assyrian king Sargon II (correctly Sarru-Kenu) captured their last stronghold, the city of Carchemish, in 717 BC. Shortly thereafter, a new people appeared in the area around Lake Urmia, the "Asguza" or "Iskuzai", i.e. the Scythians. European historiography interprets this Scythian invasion as part of a migration of peoples originating from Inner Asia. However, there is no evidence for this. On the contrary, the national name and personal names of the Scythians preserve undoubted remnants of the Hattian language in their elements and structure.
However, according to Greek sources, the Scythians migrated to Southern Russia from Asia Minor and Transcaucasia. At first, they lived around the Araxes (Herodotus, Diodoros Siculus). Pliny places their oldest homeland in Syria. According to the Greeks, the Scythians invented the tricks of working iron, and even the blacksmith's bellows, copper casting, the anchor and the potter's wheel (Euphorus, 4th century BC). Since then, we know that iron was indeed first used by the Hittites. The ancient tradition thus — quite correctly — transferred the deeds of the ancient cultural people of Asia Minor to their ethnic descendants, the Scythians. For this reason, Justin, for example, writes that the Scythians are one of the oldest peoples in the world, along with the Egyptians, and they ruled over all of Asia for 1,500 years. Their rule was ended by the Assyrians.

Mészáros emphatically emphasizes that this tradition is almost completely identical to the ancient pre-Asian history that has been learned in recent decades, so it cannot be a coincidence, much less a fairy tale.

For example, it can be proven that the early Koban culture originated in Asia Minor, in the Hittite cultural circle. Scythian symbolism is also identical in detail to that of earlier Asia Minor. The main motif of Scythian art, the deer, which was originally a solar symbol, is found in Cappadocia and Syria in the 2nd millennium BC, and in Mesopotamia in the 3rd millennium BC. The eagle with outstretched wings is also very common among the Scythians. This is also a solar symbol. It occurs already in Susa pottery in the 4th millennium, in the first half of the 3rd millennium among the Sumerians (Imdugud relief — with deer!), and in Cappadocia in the middle and end of the 2nd millennium. It first appears in the Koban culture around 1000 BC north of the Caucasus. The swastika has been common in Elé-Azsid since the pottery age of Samarra and Susa (G+. millennium). North of the Caucasus, this motif also first appears in the early Iron Age, and then becomes generally widespread among the Scythians. Likewise, the ibex, the lion-headed and bird-headed griffin, and the sword cult are demonstrably a pre-Asian heritage among the Scythians.

Since Zeuss and Müllenhoff, European science has considered the Scythians to be an Iranian race and their language to be an Iranian dialect. However, when Ebert's "der Vorgeschichte" was published in 1928, it Reallexicon could only show six word forms whose Iranian origin seemed proven. Of these six words, however, only three are undoubtedly Iranian -- he notes

244
===== PAGE 241 =====

and Mészáros's - 6 two of these are also two parts of the same personal name: "Ariapeithes". With the largest and most valuable part of the Scythian language remains, the bilingual and bilingual memories, however, they could do nothing. Miraculously, it never occurs to anyone that the Iranian theory could perhaps be revised.
According to Mészáros, the relics of the ancient Hattian language show the most parallels with the Caucasian "Pakhy" language, but are also close to Circassian and Abkhazian. The remains of the Scythians can probably be found among the Circassians and Abkhazians.
Finally, he draws attention to the Scythian nature of the Ananino culture, where iron appeared around 600 BC, accompanied by typical Scythian daggers, arrowheads, the spread-winged eagle and the bird-headed griffin. At the same time, knowledge of horse breeding and earthen fort construction also spread in the area in question. The creators and bearers of the Ananino culture were therefore the Scythian tribes who migrated from the Caucasus-Kuban region to the Ural region, he states. They cultivated the "pre-Finno-Ugric people", whose Mongoloid-Uralic anthropological character is very aptly described by Herodotus: far to the north, at the foot of the mountains (Ural), short, bald, large-faced people live in a society of Scythians, from whom they adopted their clothing.
Their name is "argippai". He states that the alleged horse breeding of the ancient Ugrians is merely an unproven linguistic assumption. On the other hand, it is a fact that the primitive Obi-Ugrians did not breed horses even in the Middle Ages, and only occasionally took one from their southern neighbors as a sacrificial animal. — as our linguists claim — but they are demonstrably derived from Our words "horse" and "saddle" are not of Ugric origin either. the Caucasian languages.
Mészáros's broad-sighted historical and cultural-historical approach clearly proves that for unbiased, objective research, the primary role of ancient pre-Asian civilizations in the cultural development of Eastern Europe, and consequently, of course, in the peoples now called Finno-Ugric, was clearly recognizable as early as the 1930s. Mészáros was undoubtedly one of the most prominent individuals in Hungarian historical science in the last 30-40 years.
He opened fundamentally new paths for our pre-historical research and linguistics, promising very significant results.

The question of the Scythian homeland has recently been increasingly raised in international prehistory research. In this regard, we recall T; Talbot-Rice, E.
Porada, R. Ghirsmán and other renowned researchers. (Volume 1, Chapter 2.) But this is also indicated by Lehmann-Haupt's observations, which we learned about in connection with the "hryvnia" issue,

However, the trend of our exclusive Finno-Ugric origin -- along with several similar opportunities -- left this opportunity unused,
245
===== PAGE 242 =====
and Mészáros' undoubtedly very thoughtful research results were, as usual, simply covered by a veil of oblivion, without any meaningful scientific refutation.

The need for revision of previous Indogermanocentric research
From the data just presented, as well as from the relevant literature, it can be clearly established that a thorough revision and re-examination of foreign - especially German - scientific works before 1945 would be very necessary, both in the historical and linguistic fields. Here, we are not only thinking specifically of the scientific production of the Hitler period, but we must go back quite a long way, to the middle of the 19th century, for this revision. For - we hope - everyone agrees with us that the ideological underpinning of the so-called "Third Reich", the theory of the Indo-Germanic "Über-| mensch", was not an invention of Hitler's, but is deeply rooted in the Indo-Germanic perspective of 19th century history and linguistics.

It is strange that this exaggerated criticism of linguistic works, which cannot exactly be called scientific, has apparently not yet begun in Hungary, despite the fact that researchers in Germany itself - in both parts - have already done a great deal in this field.
Hungarian historical studies — and even more so linguistics — are still based on the 19th century historical approach and language-historical ideas adopted from Indo-Germanic studies at the time, and work with the methods of 19th century Indo-Germanic studies, but strangely, they hardly take into account the newer methods, insights and results of their former role model, Indo-Germanic linguistic and historical research. On the contrary, today's Indo-Germanic studies have joined an obscure, grotesque, and now completely isolated and ahistorical trend, a school that is irrevocably doomed to death. On occasion, we will also point out the worrying prevalence of this approach in our historical research. The reason for this is obviously to be found in the fact that the one-sided thesis of our exclusive Finno-Ugric origin grew out of the 19th century Indo-Germanocentric view of history, and was an organic part of it; it is therefore obvious that it can only be maintained in its current form by adhering to the priority ideas of Indo-Germanic culture and by continuing to apply 19th century methods. In fact, at that moment in which the rigid language family theory, the outdated theses of a common ancestral language and people, are being rejected, similar to modern Indo-Germanic studies,

246
===== PAGE 243 =====

If they give up, the whole thing collapses, especially the historical and cultural-historical hypothesis of the language built on "Vogul roots", and the research has to start from scratch, so to speak.
We admit that this requires a great deal of critical thinking and, above all, mental strength, but at the same time, we firmly believe that drawing these conclusions will become an indispensable necessity sooner or later.
In any case, it would be better sooner than later.

The reassessment of the Indo-Germanic literature is the first and most important prerequisite for the development of this new approach. This in itself would require a huge investment of work, because since the middle of the 19th century we would have to follow point by point the origin and evidence of each hypothesis — if there are any at all, their axiomatic insertion into our historical and linguistic development worldview. We —, their muscularity and finally would have to develop a critique of this process in the smallest detail, in all issues that may be in any way related to Hungarian prehistory. Namely — in our opinion — with the help of the ever-expanding data set of archaeology.

It is no coincidence that the striking phenomenon that in our country today we find almost exclusively among archaeologists those who increasingly question the previously believed solid results of prehistoric research — based primarily on linguistic concepts, so-called “linguistic-historical facts.” It often happens in such cases that archaeologists come into conflict not only with prehistorians, but also directly with linguists. But this is also natural, especially if we include in our considerations the undoubtedly ascertainable past excesses of Indo-Germanic studies, the countless historical and linguistic fallacies that serve the ideology of pan-Germanic chauvinism, mainly accepted on the basis of the principle of authority. At one time, the results and findings of archaeology only served to provide support for the pre-formulated propositions of the Indo-Germanic cultural priority theory — with the help of an appropriate one-sided interpretation. In the meantime, however, archaeology has become quite independent, its methods have become sophisticated, and the findings have multiplied to an almost unimaginable extent, so that today the situation is reversed: the archaeologist is much more likely to be able to approach the reality of ancient times, the living conditions of prehistoric peoples, their former migrations, and their relationships, than the historian or the linguist.

Despite this, linguistics still considers itself to be the decisive factor in revealing the historical relations of these ancient, unwritten ages. Yet this conviction of linguistics is based only on the unproven 19th-century dogma of Grimm-like sound laws and the “regular sound development” derived from them. We admit that these sound laws corresponded to the

247
===== PAGE 244 =====
by examining the connections between languages that are more closely related to each other — although they have never been used for broader, comprehensive language comparisons that go beyond the boundaries of individual “classical” language families. However, nowadays, with the knowledge of 4-5000-year-old pre-Asian linguistic monuments, which have shown that the West Eurasian languages spoken today are connected to these ancient languages by thousands and thousands of indissoluble threads, and that the sound forms of words have developed over the millennia in a predominantly “regular” manner, we must realize that with the laws of sound and “regular sound changes” we can only grasp and explore a rather narrow cross-section of the real connections between languages and their development.

These considerations led us to the conviction that research into Hungarian language development and prehistory must emerge from the mud of the 19th century perspective. Since our linguistics and historical science are ultimately a function of Indo-Germanic studies and have been organically connected to it within the framework of the linguistic and historical worldview of international scholarship since the mid-19th century, we see no other methodological option than a completely objective critique of precisely this Indo-Germanic study, going back to its beginnings. Only after this has been accomplished can we hope to gain a clearer picture of the many obscure questions of Hungarian prehistory and linguistic history by embarking on new paths. One of the most important objectives of our present volume is to demonstrate the indispensable necessity of this critique in a way that eliminates all doubt.

A house built on sand is very unstable. And yet, all signs indicate that neither Indo-Germanic studies nor our exclusively Finno-Ugric linguistics and history have solid foundations.

A few words about ice fishing
Returning to Bartha's book, his next statement on page 88 reveals that there can be no great collaboration between our prehistorians and linguists. The technique of fishing under the winter ice can be traced back to Slavic influences. between; ,
The unsuspecting reader has known so far that our word “hal” is the most frequently cited, so to speak, “paradise” Finno-Ugric correspondence in Hungarian linguistics. Well, it is very difficult to imagine that the predecessors of the Voguls and Ostyaks would not have known about ice fishing — long before the 9th century, with 7-8 month winters. But the most important linguistic sign of ice fishing, —, because without this knowledge they would have had difficulty surviving our word “lék” is not Slavic either, but according to Benky's latest etymological dictionary
248
===== PAGE 245 =====

2
of disputed origin: either an ancient legacy from the Finno-Ugric period or of Turkic origin; however, a Finno-Ugric origin is more likely.
Bartha is thus once again trying to paint a false, negative picture of the 9th century Hungarians when he wants to make people believe that ice fishing was only discovered sometime around the 9th century. Ice fishing was already known to Mesolithic cultures. Bartha's opinion is therefore an absurdity in terms of cultural history.
/ Regarding the question of references to literature When discussing iron processing — in note 56 on page 89 — we learn from Bartha that, according to calculations in an article by Kornél Bakay published in 1965, the minimum iron requirement for 20,000 warriors is over 20,000 kg.
There would be nothing to criticize about this if Viktor Padányi had not already made these calculations in 1963. (Dentumagaria.) However, Bartha does not mention yy. The point here is not whether we agree with Padányi's book in detail or not, but there is no doubt that we can find many very thought-provoking, novel insights in it. This can also be gauged from the fact that our historians have used some of Padányi's statements in their works more than once, without, of course, naming their sources. Only Gyula László honestly states that what Padányi writes about the circumstances of the conquest can be accepted by everyone. (On the Conquerors).

We consider this collective silence to be more serious than ordinary cases of plagiarism, namely because we must know that Padányi wrote his book in Australia, cut off from almost all scientific aids, in his free hours after his breadwinner's job, and not with all the conveniences of a professionally equipped scientific institute, pursuing historical research as his main occupation, and even drawing a salary for it, which advantages his exploiters — in addition to carefully “forgetting” the reference — all enjoy. We all know the proverb: it's easy to burp when you're full.

As an example, we can mention the name of Samu Padányi of Szádeczky- “Additions to the Problematics of the Szavárd Hungarians” (1977), where the author's very rich bibliography Kardoss, although we search in vain for it. Yet Padányi in his “Dentumagariá” pointed out emphatically the same fortress named “Matzaron” mentioned by Theophylaktos — together with its Sabir connections, which Szádeczky-Kardoss also discusses: he stated that the name of the fortress preserves the memory of the Sabir-Hungarians of Transcaucasia. Szádeczky-Kardoss also comes to the same conclusion, noticeably under the influence of Padányi. But he does not mention his name,
249
===== PAGE 246 =====
Even if one does not agree with Padányi's view of history, scientific openness would require that one at least name the source of the ideas he raised and found correct. This is what Gyula László did, even though he also rejects most of “Dentumagaria”.
On the agriculture of the Finno-Ugric and Hungarians
Bartha ends the subsection discussing the Hungarian economy in the 9th century with a final conclusion: “Consequently, the Hungarians learned about agriculture in the area south of the forest steppe” (p. 89).

After reading the aforementioned five and a half page sub-chapter again and very carefully, we had to conclude that what was said there “only follows” that the Hungarians practiced agriculture “in the area south of the forest steppe” — during their stay there, in the 8th-9th century AD. However, where and when they learned about agriculture” is a completely different question. Bartha's statement above is as simple a misrepresentation as if someone were to claim that American farmers learned about agriculture in Oklahoma in the 19th century.

We only refer in passing to the already mentioned archaeological finding, which was formulated by one of our renowned prehistorians as follows: “On both sides of the Urals, along the Ob, Irtysh, Ufa, Volga, Oka and other rivers, from the beginning of the 2nd millennium BC to the middle of the millennium, a population of immigrants arrived from the south in successive waves... Under the influence of the southern immigrants, the Finno-Ugric groups, in addition to their predatory farming, took over animal husbandry and agriculture, and developed a high-quality bronze industry.” (Bartha A.: Society and economy in Hungarian prehistory) and

According to this renowned prehistorian, the Hungarians, or rather their Finno-Ugric predecessors — which is always the case in this regard — began farming in the first half of the 2nd millennium BC. What they met the theoretical and methodological considerations might have prompted Bartha to reintroduce the Hungarians of the 8th-9th centuries BC to farming, which had been known for 2,500 years?

Certainly, our exclusive Finno-Ugric origin often has very winding paths. The main goal is to explain the 19th century Stone Age cultural state of “our closest linguistic relatives”, the Voguls and Ostyaks, by lowering them to a similar cultural level to the direct predecessors of the conquering Hungarians in the 8th-9th centuries. However, this = as we can see — is not possible even at the cost of the most amazing contradictions. xDo we remember after this that the Finno-Ugric people of the Ananjin period also knew agriculture? It is unnecessary, because Bartha's above statement is anyway

250
===== PAGE 247 =====
It also refutes itself, but at the same time it also bears witness to a degree of historical and cultural-historical confusion that is unparalleled.
On the one hand, he talks about the agriculture of the “Finno-Ugrians” of the 2nd millennium BC, and even about their parallel bronze industry, and on the other hand, he wants to dispute the fact that the 8th century AD immediately preceded knowledge of agriculture from the Hungarians of the 19th century. What else is this if not a confused reflection of the 19th century Indo-Germanic “wild barbarian” Hungarians in Bartha's mind, who, however, miraculously does not notice that with this statement he is also killing the agriculture of the Finno-Ugric people of the 6th and 2nd millennium BC. We could even call this a boomerang of historical perspective. This is where those prehistorians end up — inevitably and necessarily — who, despite the newer historical and archaeological discoveries, still try to piece together prehistory from the theses of 19th century linguistics that they believe to be solid.

On linguistic paleontology
On pages 92 and 93, Bartha further develops the same “linguistic prehistory” when he discusses our words “úr”, “by”, “in”, “tömény”, mainly in the interpretation of György Györfly, and uses these words to outline the picture of 9th-century Hungarian society.

However, according to competent linguists — for example, Géza Bárczy — one should not draw “Mr.” “b6”, “In” are of uncertain origin. We do not consider it the most fortunate idea to socio-historical conclusions from words of uncertain origin, and moreover, tied to a given era. We believe that this has little to do with systematic scholarship.

Bartha could have been on much firmer ground if, for example, he had drawn conclusions about the social relations of the 9th-century Hungarians living in that region from the archaeological finds of the settlement near Lebegyin described above. He could have done this with more reason even if he considered the settlement to be Slavic. However, this certainly has serious methodological obstacles: it is not possible to compare the social relations of primitive Hungarians with those of advanced Slavic settlements. He rather uses words of uncertain origin. This has no methodological obstacles, despite the fact that in recent decades many renowned Indo-Germanic linguists and paleontologists have clearly shown that this so-called “linguistic paleontology”, which Bartha employs, was fundamentally wrong in terms of theoretical and methodological considerations. 1, next volume, chapter 2.)

251
===== PAGE 248 =====
To the Kurszán theory
Likewise, no methodological considerations prevent Bartha from simply stating György Györfy's Kurszán hypothesis as fact on page 93: “Kuszán -- Kurszán... according to recent research is identical with the chief prince of the Hungarians.”

However, Györfy Kurszán's theory is still quite far from being conclusively proven. It is enough to recall that the 16th-century Renaissance Bavarian chronicler named Aventinus, on whom Györfy Kurszán primarily bases his hypothesis — explaining at length that we should believe Aventinus as a distant, objective chronicler much more than the biased Hungarian chronicles that falsify in favor of the Árpád house — is so “objective” and credible that in his chronicle he also claims, for example, that Nuremberg was founded by Heracles. (D. Dümmerth: Álmos fejedelem mítosza és verálása.) But Károly Czeglédy also established that Györfy's theory is in conflict not only with Hungarian but also with Byzantine sources. (Das sakrale Königium bei den Steppenvölkern.)

Speaking of Györfy's Kurszán theory, we must also point out the peculiar logic with which Györfy tries to use the white horse legend to prove the lies of our chronicles, saying: Kündlü's son Kusid (Kusán) was simply degraded to the position of Árpád's envoy.
We ask: was Kusan (or Kuszal) killed in the Bavarian camp during armistice negotiations anything other than Árpád's envoy? Since the Neanderthals, envoys have usually visited the enemy camp, not sacred high kings.

By the way, the truly contemporary sources once write “rex eorum Chusso” (Annales Sangallenses majores), and another time “Chussal dux eorum” (Annales Einsiedlenses) in connection with the mentioned event. However, since other Western sources also repeatedly call the leaders of the Hungarians fighting abroad “rex” (king) instead of “dux” (leader), it cannot be concluded from this expression alone that they were princes.

In short, as long as Györfy's hypothesis has not been proven beyond all doubt, Bartha has the undiminished individual right to accept this assumption. However, we can no longer agree with Györfy's statement of his improbable hypothesis as fact in a scientific work that is intended to be fundamental.
We would like to add a reference from an authoritative author to this principled statement: “Evidence is a concept of great weight, it is more of a guest in history than a white raven in nature... Irrationalism, the pursuit of mirages, is a symptom that is difficult to treat. Gyula László cites illustrative examples of this pathological symptom, despite his objection that the “two-252

===== PAGE 249 =====
“The assumption of a “total conquest”, there were those who called those who argued against it “enemies of Hungarians.” (Bartha A.: What is against reason is ugly.)
: We believe that this expert opinion also applies to Antal Bartha. Therefore, before he speaks of “pathological symptoms” in relation to others who try to present hypotheses as facts, it would be necessary for him to clear the trash from his own doorstep. We consider this especially important for him to take to heart when compiling the prehistoric chapters of the planned Hungarian historical synthesis.

We emphasize: this is a fundamental principle requirement, the strict application of which can be demanded from specialists to a much greater extent than from anyone else.
The . The personal name “Álmos” and the etymological methods of our linguistics
In note 133 on page 100 of his book, Bartha states that since, according to Ibn Fadlan, the name of the son of the Volga Bulgarian king in 921/22 is a personal name of Bulgarian-Turkic origin in Hungarian. . He was probably sleepy, it is “a personal name of Bulgarian-Turkic origin.” (The original Arabic text reads “lms”).
This astonishing conclusion proves more eloquently than any criticism from outside the incredible degree of uncriticalness and one-sidedness of the exclusively Finno-Ugric perspective.
It shows that historical and linguistic sources and data are twisted, distorted, and adapted to the thesis of Finno-Ugric origin. In particular, we can learn from them
a . “methods” by which Finno-Ugric studies declared thousands of Hungarian words a priori to be loanwords.
Let us note the overwhelming logic: the name Álmos appeared among the Volga Bulgarians in 921/22, “presumably ~~the Hungarian invasion~~ ~~the Hungarian invasion~~” can be dated almost a hundred years earlier?”, a name — which, moreover, still Hungarian language, and this meaning is organically connected to the name Álmos through the ancient Turul myth — for Bartha, it can only be a “personal name of Bulgar-Turkic origin”.

If Bartha considers this logic methodical and scientific, we would be very curious to know what he considers to be “chasing a mirage”? . “irrationalism” or .

Incidentally, we find the same skewed logic in many cases in Benkő's latest historical-etymological dictionary (hereinafter TESz), when, for example, it indicates the origin of certain Hungarian words in words that cannot be given, but are merely inferred. Or in our word “szablya”, where the origin is already “uncertain” (a wandering word), but

253
===== PAGE 250 =====
The Serbo-Croatian transmission is very likely, despite the fact that the oldest South Slavic data is 24 years later (1417) than the first Hungarian occurrence of the word (1393). But even this “compromise” is only due to the fact that Gyula László repeatedly pointed out the archaeological and cultural-historical impossibility of the Slavic derivation of “szablya”. In fact, in older Hungarian etymological dictionaries, “szablya” is still listed as a Slavic loanword.

For the sake of completeness, we should also mention that according to the TESz, the etymology of “szablya” is unclear. A layman unversed in the hair-splitting of phonetic linguistics is astonished by this statement, because he involuntarily thinks of our basic verb “szab”, of which “szablya” is obviously only a further developed derivative. However, this obvious, reasonable and almost blatantly obvious explanation encounters several difficulties, “says the TESz, because it could have an infinitive; the explicit meaning of szab “cuts” can only be dated from a relatively late period; etc.

However, if the layman is not convinced by this gibberish argument — for example, not least because these same scholarly linguists do not hesitate to derive many of our words from “starry” Slavic words, that is, words that cannot be dated, but are merely inferred — and points out, for example, the latest edition of Kluge's German etymological dictionary (1975), then
a . Under the heading “Sable” you can read the following: “... W. Kurrelmeyer demonstrates this since 1428... The following evidence comes from the Polish border... This points to an initial borrowing from Polish “szabla”. During the Turkish Wars of the 16th century, its source word, “magy. szablya” (from “szabni” to cut), also had a direct influence.
From there the word also spread into other Slavic and into some of the Romance languages.” (Kluge: Etymological Dictionary of the German Language, 21st ed. 1975. 617-618.)

In Hungarian: “W. Kurrelmeyer shows that since 1428 ... The data that immediately follows come from the Polish border ... This points to the primary adoption of the Polish sabre. During the Turkish wars of the 16th century, the source of the Polish word, the Hungarian szablya (from the verb “szabni- to cut”), also had a direct influence. From there, the word penetrated other Slavic languages and some Romance languages as well.”

That is, the representative etymological dictionary of the German language sees no “difficulty” in deriving “szablá” from the Hungarian language due to its identical phonetic form and meaning. We must say: neither. (Cf. also . from the verb “sab” and its Sumerian “sab” = “schneiden, abschneiden, zerschneiden”),
254
===== PAGE 251 =====

The question of Hungarian rule in Kiev and the objectivity of our historical research
On page 102, Bartha briefly mentions the Hungarian rule in Kiev assumed by Vernadsky. He writes that “there is a lack of reliable data on this, although given the balance of power that developed at the time, this assumption cannot be refuted.”

It is surprising how well he can simultaneously apply the principles of method and objective science to this question. We almost don't recognize the Bartha we used to know.
Like a phoenix rising from its ashes, it radiates the highest demands of impartial objectivity. Our joy over this glorious rebirth is only slightly disturbed by the fact that this Solomonic wisdom appears just when - and only then - when there is an opportunity to take a position on a very important issue from a Hungarian point of view, which would raise the political significance of the Hungarians at the time of the conquest in 9th century Eastern Europe to the same level that our people undoubtedly and indisputably occupied in Central Europe a few years later, immediately after the conquest - as can now be proven by contemporary sources. This force could not have grown out of nowhere overnight. Historical logic would therefore require that we reckon with the Hungarians as significant political factors even immediately before the conquest.

However, at this very moment, Bartha's scientific super-conscience arose, and this does not even allow him to accept an assumption about which, in the case of the name “Álmos”, or, let's say, he himself writes that “the assumption cannot be refuted”. We would example, in the case of the previously discussed issue of Györfy's Kurszán theory, which stands on much, much more shaky foundations?
be curious, where was this hypercritical spirit, for

After this, we can understand the silence about the undoubted Hungarian connections of the settlement near Lebegyin: there too Bartha's scientific conscience prevailed. Since no business card of Álmos was found in any of the excavated houses in Lebegyin, nor even a monogrammed horseshoe nail, there is no doubt that “certain data are missing”. On this basis, we can drive the entire historical science to absurdity or even throw it into oblivion, because from these ancient times such A . “certain data” are missing almost everywhere,

“Pechnoy-run”

The first sentence of Chapter III begins: "In 896, two extremely serious blows; as a consequence of the defeat suffered by the Byzantine-Bulgarian and Pecheneg armies..." (p. 119)

255

===== PAGE 252 =====

That is, according to Bartha, the Byzantine-Bulgarian army would have defeated Levente's army together. This is such a surprising and new insight that we would have been very happy if Bartha had provided it. But all joking aside: we do not want to assume that Bartha does not know the real historical circumstances; his sentence above rather reveals that he has difficulties with precise wording.

Considering the above statement in essence, we do not understand why he places the events of the Bulgarian war, the Pecheneg attack and the conquest in 896 without any reference? According to the opinion of most of our historians, all this happened in 895. Bartha does not provide any evidence to support this view. This is a rather unusual procedure, especially when someone claims things that are contrary to popular belief. Perhaps "Roma locuta, causa finita"? We cannot know. The lines following the previous quote also reveal that, according to Bartha, the Bulgarian and Pecheneg defeats triggered the conquest. After these defeats, the people of the settlement lands set out, he writes.

In other words, he still holds the view that has long been dominant in our historiography - also a legacy of the 19th century - which Padányi refuted with a very thorough analysis in 1963 (Dentumagyarja). As we have seen earlier, Gyula László accepted this refutation without reservations. But he had already taken the same position much earlier in his work "We Hungarians" (1941). He stated that the conquest of the country Iron Gate. was based on a well-thought-out strategic plan. It was an all-encompassing operation, with Mihály Ferdinándy simultaneously advancing from the direction of Verecke and the

With this we only want to emphasize again that, given the state of the question, the procedure chosen by Bartha is impermissible: simply in the form of a statement, all kinds of Without any reference, he takes a stand for a hypothesis that dates back to the 19th century, is ahistorical and, apparently, outdated in every respect. The phoenix The "methodical" rise of the philosopher therefore came to an end very quickly and — miraculously — again on a topic where scientific arguments and historical A careful, conscientious comparison and consideration of the pros and cons of these circumstances would undoubtedly have resulted in a more favorable assessment of the historical and political significance of the conquering Hungarians, The pre-conquest population of the Carpathian Basin "The settlement of the Hungarian ethnic group in this then very sparsely populated area" ~ writes Bartha, on page 122 of his book, about the Carpathian Basin. Again, there is no literary data for this statement. Yet, besides us, many others would certainly be curious about those scientific findings.

===== PAGE 253 =====

to the extensive research that formed Bartha's opinion. However, Bartha considers it better to keep these sources secret, because the known data in this direction, i.e. almost without exception, prove the exact opposite of what Bartha claims. Such silences are also an integral part of the methods of proving our exclusive Finno-Ugric origin. They believe that it is better not to talk about anything that does not point in a Finno-Ugric direction. In this case, namely about the Avar-Hungarian ethnic relations, the very difficult to refute evidence of Gyula László's "double conquest" thesis.

In short, both contemporary written sources, anthropological studies, and the results of archaeology clearly state that the Avars did not disappear after the campaigns of Charlemagne, but rather arrived in large and populous groups during the Hungarian conquest and merged with the conquerors. We have 35-40,000 authentic graves from the so-called Late Avar period alone. (László Gy.: According to the studies of Pál Lipták, the anthropological formula of the early double conquest"; the relevant literature can also be found there.) Árpád-era Hungarians shows essentially the same features as the skulls of the Late Avar cemeteries. He also comes to the conclusion that a very large number of the Late Avar population mixed with the conquering Hungarians. (Lipták: Paleoanthropology of the Ethnogenesis of the Hungarians.) Certainly, these facts are not very compatible with the 19th century dogma of our exclusive Finno-Ugric origin. Therefore, Bartha does not give any data, but, according to an old, well-established custom, merely "states", thinking that his own scientific name is sufficient for the bona fide reader - who is not in a position to check all the data separately. evidence.

But anyway, one should not give in to the opinions of such biased, consciously biased domestic chroniclers in favor of the Hungarians, as he learned well from his father Bartha. Much more credible is therefore the objective, impartial foreign chronicler who once wrote about the "deserts of the Avars" (Eginhard) or, of course, the statement of the Kiev Chronicle that "they perished like the Avars, not even a memory of them remained".

We could also cite other "objective foreign chroniclers" - contemporaries and contemporary ones who say that after the collapse of the Avar empire, large masses of the people remained in the Carpathian Basin (e.g. Altheim: Geschichte der Hunnen), but after the archaeological and anthropological facts presented above, this is not essential, and we mention it only for the sake of completeness.

Once again, we can state that Bartha's above statement is not a historical fact — or probability — deduced through scientific methodology, but simply Bartha's private opinion, or more precisely, an unfounded assumption of the exclusively Finno-Ugric school of prehistory — in a packaging that gives the appearance of a scientific statement.

257

===== PAGE 254 =====

On the "Sassanid-Iranian" artistic style From pages 125 to 130, Bartha discusses Sassanid and late Sassanid art and its Eastern European and Western Asian influences. Since this style occupied a central place in the art and form of the conquering Hungarians, and its effects even played an important role in our folk art in the recent past, we would like to deal with it in more detail, outside of Bartha's perspective. Bartha, starting only from the given facts of Sassanid and late Sassanid art, follows the traces of the small steppe and forest influences of this style, within temporally limited boundaries, roughly in the projection of the 6th-9th centuries AD. He completed this task in a manner deserving of all praise. He gives a very detailed, and also clearly understandable summary from beginning to end, of the occurrences of objects in the Sassanid and late Sassanid styles from the second half of the 1st millennium AD. We can see from this that he can work methodically — if he wants to.

He discusses at length, over two pages, the origin of the characteristic motif ensemble, and even raises the question of the legitimacy of the technical term "Sassanid". However, in these considerations with a broader perspective, which partly lead to the field of general cultural history, we can no longer experience the same exhaustive, detailed methodology that we appreciated earlier.

When discussing the origins of the Sassanid style, we can only encounter trite, clichéd expressions from the repertoire of Indo-Germanic historiography of 50-60 years ago, such as "Bactrian-Greek" or "Hellenistic-Iranian" influences. As for the roots of the Sassanid style, we certainly learn nothing from Bartha about the results of his research, the Parthian origin of the motifs of Sassanid—, but in recent decades art, and indirectly about the massive Scythian, and through this, Urartian-Hurrian and Luristan traditions, which are now considered commonplace in the relevant scientific literature.

But this is natural for a prehistorian with a Finno-Ugric perspective. The Parthian, even Scythian and Urartian-Hurrian roots of Sassanid art could raise very unpleasant questions regarding the ultimate origin of Hungarian art during the conquest period, so it is more advisable to ignore these connections from the outset and rather speak in general terms of Bactrian-Greek or "Hellenistic-Iranian" antecedents. However, these references are only very, very narrow sections of the question, and we can even say that they serve directly to obscure the problem,

Modern research has clearly established that the so-called "Sassanian" style is entirely a continuation of one of the stylistic trends of Parthian art (Ghirsman: Iran II, — Parther und Sasaniden; Porada: Alt-Iran; Altheim: Das Alte Iran; Lukonin: Persien II; etc.).

258

===== PAGE 255 =====

On the other hand, we have indicated that the Parthians brought new life to the stiff, sterile Old Persian court art. They renewed and developed the ancient Scythian-Urartian-Luristan artistic traditions. The strong Hellenistic influences of the early Parthian period later completely faded, and a powerful, rich, and in every respect independent Parthian art emerged, with numerous stylistic variations.

At the same time, however, we have also seen that recent studies have revealed the close and far-reaching steppe connections of the Parthian Empire. These connections extended all the way to the East Asian Hun (Hyung-nu) empire that flourished in the 2nd-1st centuries BC. Ghirsman, for example, identified several finds from Noin-Ula as Parthian in origin. But Altheim also emphatically points out the surprising parallels between the Parthian animal style and the bronzes from the Ordos region. (Altheim-Stiehl: Geschichte Mittelasiens im Altertum.) These newly discovered Parthian-Hun and other steppe connections allow us to place the previously generally accepted views on a new and at the same time more solid basis in several very important issues. For example, we do not necessarily have to seek the origin of the glass or precious stone-encrusted cup from the Hunnic cremation grave found in Szeged-Nagyszéksős in the motifs of the so-called "Chosrau cup" from more than a hundred years later, as has been the case so far. The terminus ante quem of the burning of the Nagyszék Sösi cup cannot be placed later than 453 (the death of Attila), but this of course does not say anything about the date of its production; it could have been made several decades, or even a century or two earlier: such expensive vessels were considered family treasures, passed down through generations. The terminus post quem of the making of the Chosrau cup, however, is AD 531: the accession of Chosrau I. Despite this, the Nagyszékső cup was declared a Persian adoption, with the excuse that Sassanid art was much more advanced than the Hunnic. Now, after the common Parthian antecedents of Sassanid and Hunnic art have been revealed, there is no obstacle to evaluating the two cups as at least an independent, parallel development. We wrote, "At least an independent, parallel development" because there are good reasons to believe that the Chosrau cup could be a direct Hunnic adoption. We are thinking primarily of the Iranian hegemony of the Hephthalite or White Huns during the half century immediately preceding the accession of Chosrau I to the throne (1. later).

On the party question The Parthian question is increasingly emerging as a key issue of primary importance in modern Middle Eastern and Central Asian historical research. It will be very instructive to examine the real historical and cultural-historical role of the Parthians and the Parthian

259

===== PAGE 256 =====

We will take a closer look at these arts, which until recently were treated rather superficially by Indo-Germanic historiography, based on the lessons of recent research.

First of all, we must emphasize the very suggestive and very important circumstance that the truly Iranian Persians never considered the Parthians to be among them. The Shah-Name is also conspicuously silent about them (Czeglédy: Migration of Nomadic Peoples from the East to the West). Altheim also states (Reich gegen Mitternacht) that the Persians always considered Parthian rule as a foreign rule. In his essay "Das Alte Iran", Pcdig writes that the Parthians were the complete opposite of the Persians in their customs and character traits. He also mentions that the Parthians' role in creating and transmitting culture is almost immeasurable. A. v. Gutschmid reports that in the first century AD, the vast region stretching from the Sea of Azov to the Indus was ruled by members of the Parthian Arsacid house. We know of five Parthian subkingdoms. With Arsacid branches on the throne, under the — at least nominal — authority of the great king: Parthia, Media, Armenia, Scythia (north of the Caucasus) and Bactria with the Indus region. (Geschichte Irans und seiner Nachbarländer von Alexander bis zum Untergang der Arsaciden.) It is also noteworthy that the Kushan Empire developed from the Parthian sub-kingdom of the Bactria-Indus region in the 2nd-3rd centuries AD. It is also very important that, according to the testimony of the Armenian writer Faustus Byzantinus, the throne of the former Parthian vassal kingdom of Scythia, i.e. the Pontus-Caucasus-Caspian region, was still occupied by an Arsacid king at the beginning of the 4th century: Sanesan, the king of the "Maskites and Huns", who was a relative of the Armenian king - also an Arsacid. Géza Nagy pointed out this information as early as 1897 (The Prehistory of the Hungarians), adding that the leading elements of the Eastern European steppe peoples emerged from the Parthian ruling class before the Huns became powerful. From these data, it can be undoubtedly established that even a hundred years after the fall of the Parthian Empire, its former peripheral territories were ruled by descendants of the Arsacid House. But the most important piece of information in this regard is that several early-medieval Armenian sources directly trace the Arsacids to the Asian Hun dynasty. And this must have had a historical basis, because the Arsacids sat on the Armenian throne for more than 300 years, so the Armenian writers must have known the origins and traditions of their own dynasty well. We would also like to draw your attention here to the fact that the two main branches of the Huns established their empires precisely in the territories of the two vassal kingdoms of the former Parthian Empire: the Hephthalite Huns invaded the former Arsacid kingdom in the Bactria-Indus region, while the European Huns appeared in the region of the Parthian successor state of Scythia, Pontus-Caucasus, and here they strengthened under the Arsacid dynasty of Sanesan.

24
===== PAGE 257 =====
Altheim's statement in Volume II of his work "Geschichte der Hunnen" also points to close Parthian-Hunnic relations: The Parthian king Artabanus, together with his army, greets the insurgents. day, just as according to Chinese sources the king of the East Asian Huns did, + notes Altheim. In the same work, volume IV, Altheim raises a possibility of paramount importance from the point of view of cultural history. He says that the writing used in the Parthian Empire has so far resisted all attempts at decipherment. Words were written here and there in different ways, and no system has been found for their interpretation. From this and other signs, we must also think - writes Altheim - that the Parthians used the Sumerian-Akkadian ideograms to write down their root words and provided them with the appropriate endings written in the letters of the Aramaic alphabet. In our opinion, this question would require thorough, detailed investigations.

In his book "Weltgeschichte Asiens im griechischen Zeitalter", Altheim also addresses the question of the Parthian language. He states that the so-called "Northwestern Arsacid dialect", which is generally considered to be the language of the Parthians, was not the original Parthian language, but a regional Persian language. Therefore, we do not know the language of the Parthians, Altheim states. In the same work, he describes the customs and fighting methods of the Parthians, which are exactly the same as the later Altaic peoples. They led the battle with drums, as did the Turks later. In war they had leaders and in peace they had judges: "duces in bello, in pace rectores" -- Pliny writes about them, following Apollodorus. It is noteworthy here that our Hands have a completely identical statement about the Huns. Did he get it from Pliny? - it is difficult to assume such a thing, a good hundred years before the beginnings of humanism. We also learn from Altheim that the Sakas belonging to the Parthian Empire, who were defeated by the Parthian king Mithridates I around 140 BC and settled in the eastern Iranian province, later named Sistan for this reason, occupied a large part of the Indus Valley around 60 BC, and were able to gain independence from the Parthians there for a while. -- Incidentally, note that these were the Sakas who were called "Turuska" by the Indics, and their dignitaries were mostly Turks (l. above, p. 240). -- However, they soon came under Parthian rule again, with the Arsacid branch on the throne, and after the middle of the 1st century AD this ruling house increasingly became independent of the Parthian great king. It was during this Arsacid dynasty, around 150 AD, that Sanskrit first appeared in secular texts, at the initiative of the Parthian kings; these Sakas-Indic vassal kings wrote the first profane Sanskrit texts. Sanskrit was never a living language, Altheim states, but an artificial language formed with rules.

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We can therefore see that the much-mentioned Sanskrit, to which Indo-Germanic studies at the time attributed a worldwide linguistic development and historical 261
===== PAGE 258 =====
It developed as a normative professional language, built on a vision -- like Esperanto = -- in an environment where, in addition to the Indic people, the Parthian ruling class and the masses of the people also played a very significant role throughout the centuries. It should also be mentioned that Altheim deals with Parthian art and its more distant connections in this work. He explains that the Parthians had an independent animal style, the main motif of which was the deer - just like the Scythians. However, this Parthian animal style is surprisingly similar to the Ordos bronzes and even to the later Avar style, Altheim writes.

The same is stated by Ghirshman, in the already cited work (Iran 11. - Parther und Sasaniden). This latter circumstance should be considered very important when considering the hitherto unresolved question of the history of motifs: what roots nourished the strong Scythian stylistic parallels that re-emerged in Eastern Europe with the Huns, Avars, and then the Magyars. (l. Gyula László, Nándor Fethich and others' findings on this.)

The above excerpts, taken from the very rich recent literature dealing with the Parthian question, clearly indicate that from the middle of the 3rd century BC to the beginning of the 4th century AD, that is, roughly until the Huns gained power in Eastern Europe, we must attribute a decisive role to the Parthian Empire and its successors in the historical events and cultural development of East and Central Asia, as well as Eastern Europe. Research that can be called objective, no longer based on the exclusive Indo-Germanic cultural priority, began only 3-4 decades ago, but has already led to surprising results that have shown the 19th century view of history to be fundamentally flawed. We are convinced that Hungarian historical research must sooner or later take note of these insights if it does not want to completely break away from international scholarship.

As for our original, narrower question, based on the connections we have known so far, we can say that the "Sassanid", as if, for example, the entire European "Sassanid-Iranian" style trend is approximately our research on Renaissance, could rightly be called The significance of the Lomovatovo culture for prehistory. We would call it the Medici style. After this digression, let us open Bartha's book again. On page 137, he describes the so-called Lomovatov culture of the Byelaja, Chusovaja, and Kama regions, its two chronological groups, the Harinal 68 version from the end of the 4th century to the second half of the 5th century, and the Nyevolino version from the 6th to the 8th century. 262
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===== PAGE 259 =====
"The research links the development of the Lomovatov culture with the appearance of a new ethnic group migrating from south to north, and sees Ugriic tribes as its ethnic background. It is difficult to take a position on this specific ethnic issue based on the available data... At the same time as their appearance, the majority of Iranian silver products appeared, namely in the area between Kungur and Cherding (administratively Pjermi Region), which coincides with the spread of the Lomovatov culture. The Lomovatov tribes maintained almost regular contacts with the southern areas, and during their rule the famous Iranian silver flowed unhindered to the north," he writes.

This archaeological culture seems to be of paramount importance from the point of view of our prehistory. We fully agree with Bartha on the issue of the "Ugriic" ethnicity assumed by Soviet archaeologists, because this would mean that the "Ugrians" - that is, the Voguls and Ossetians, called "Ugrians" in today's scientific terminology - appeared in the Kama-Bjelaja region only at the end of the 4th century AD. However, this is hardly conceivable according to all other data at our disposal. The ancestral ethnic group of the Voguls and Ossetians, who were arbitrarily christened "Ugrians" by Finno-Ugriic studies in the 19th century, certainly lived in these areas in the millennia BC.

However, we can consider it very likely that the real "Ugriic" - i.e. "Ogur", "Ugur", "Onogur" or "Hunugur" - ethnic group of the contemporaneous early medieval sources, or more precisely, some of its groups, did indeed migrate to the territory of present-day Bashkiria and Perm during this period and organized the fur trade there by forming colonial settlements.

Let us consider Jordanes' statement that the "Hun-Ugurs" -- whom he mentions, along with the Sabirs, as if as another name for them -- transport the expensive northern furs to the southern, Byzantine and Persian markets. This could also explain why the famous "Iranian silver" flowed northwards under the rule of these tribes: the furs were paid for -- at least in part -- with this silver. Moreover, the Sabir traditions of the Voguls and Ostyaks could even be shed light if we consider that Jordanes in The Historical Role of the Huns As we have seen, he considers the names "sabir" and "hunugur" to be essentially identical.

On the other hand, in the case of the Lomovatov ethnic group, if we broaden our horizons a little, freeing ourselves from the navel-gazing of the Finno-Ugriic perspective, and not only fix our gaze rigidly on the Kama-Bjelaja region, but also observe the historical events of the steppe at that time, it immediately becomes apparent that the first wave of this ethnic group coming from the south, the Harina group, was precisely

263
===== PAGE 260 =====
appears in the vicinity of Káma-Bjelaja when the European Huns | in Southern Russia and the Carpathian Basin, and the eastern, so-called Hephthalite or Fcher Huns, establish their vast empires in Southern Turkmenistan, Sogdia and Bactria. The continental scale of these historically unquestionably demonstrable The events that were triggered by the historical activity of the Huns, according to all common sense, should also be organically connected with the migration of the Harinas to the north. Nowadays, the deeply ingrained Eurocentric historical perspective that examined the Hun question only in its Germanic-Roman aspect can no longer be maintained. Foreign researchers have long noticed that the Huns began their actions at almost the same time, almost in a planned manner, on the one hand

Eastern Iran and Southern Turkmenistan, and on the other hand against Europe. Ammianus Marcellinus dates the appearance of the "Chionites", i.e. the White Huns, in Sogdia and Bactria to 357 AD. Similarly, according to a Chinese record from around 440, the Xiongnu people occupied Sogdia three generations earlier, i.e. around 350-360. The Huns' advance into Europe began in the early 370s, but this date only refers to the subjugation of the Goths: the Alans had already been conquered earlier.

Altheim states ((Geschichte der Hunnen, Vol. I) that the center of the Huns in the 4th-6th centuries was Western Turkmenistan — Eastern Iran. They received their supplies from there, even long after their first European invasion: the Hun-like Bulgars, Avars, Khazars all came from the territory of the White-Hun empire. In particular, the Avars were the descendants of the Hephthalite empire.

"var" is It was formed by tribes called "huns", who were later joined by significant Onogur and Bulgar groups. After 550, the Avars migrated to Europe along the northern borders of Persia, bypassing Lake Caspian from the south, through the Caucasus, he writes.

Also from the territory of the Hephthalite Empire, the three peoples broke westward in 585 along "Turkish" people, about whom Patriarch Michael of Syria reports. They also set out from Northeastern Iran and the southern shores of the Caspian Sea and then arrived in Southern Russia via the Caucasus. This is also proven by the source data that reports on the Khazars who remained there in the area of Merv in 651. These three peoples were led by three brothers. One of them — Bulgar — advanced on the northern shore of the Black Sea to the Alduna, while the other two brothers settled further east, in the area between the Caspian and the Don. One of these peoples, the Hungarians, could have come to Europe. This would "Kazari" was. Altheim firmly states that according to the yyy, the third brother, not mentioned, is only related to the satisfactorily resolve -- among many other open questions, e.g. Emperor Constantine's much-debated statement that the Hungarians "lived with the Khazars for three years" is also true. In Greek, the paleographic difference between 3 and 303 is so small — "treis" (3) and "t'treis" (303).

264
===== PAGE 261 =====
~, that the copyists could very easily have written, If we place the Hungarians' move to Etelköz in 888 -- which is very likely, they lived with the -, then with 303 6 4 we get the year 585, The Hungarians Khazars from 585-16[888 -- concludes Altheim's line of thought.

From Altheim's logical conclusions we can clearly gauge how much further we would have come in unraveling the countless unresolved questions of our prehistory if the dogma of our exclusive Finno-Ugric origin had not narrowed the horizon of our researchers to the region of 50-55 degrees north latitude. But we can also notice that it was not out of a desire to split hairs or to search for a knot of khakan that we mentioned the circumstance, which at that time and then perhaps seemed insignificant, that Bartha did not even mention the old name of the Hungarians "Sabir", and was profoundly silent about the actual name of the Khazars "Sabir".
The Onogur-Sabir-Avar migration route
Altheim is not alone in his views, however.

As early as 1953, in his treatise entitled "Theophylact's Exkurs über die skythischen Völker", Haussig proved with a vast scientific apparatus, largely through original source research, that the Onogurs, Sabirs, and then the Avars migrated to Europe along the southern shore of Lake Caspian. He established that the former Transcaucasian settlements of the Sabirs and Avars are recorded in the Syrian list of peoples of the rhetorician Zacharias, as well as in Persian, Armenian, Greek, Latin (e.g. Ammianus Marcellinus) and later Arabic sources. Some Persian sources even know about the migration of the Avars through the Caucasus.

We remind you that in this regard, Elemér Moór lists . In his article "The main problems of Hungarian prehistory", in which he discusses the Sabir and Avar migrations in great detail, geographical-climatological arguments that the area between the Ural Mountains and Lake Caspian is unsuitable for the migration of nomadic peoples. The only possible route would be the southern shore of Lake Caspian and the Caucasus + he writes =, but in the case of the Sabirs and Avars he rejects this possibility, because in his opinion this was out of the question at that time because of the Persians.

Since then, however, research has chipped away at some of the Sassanid glory. Today we know that since the Hephthalite-Huns conquered western Turkmenistan and eastern Iran, practically nothing has happened in Iran against their will (L. Altheim: Das Alte Iran, etc.). We must take this literally, because it has been revealed, for example, that the Sassanid great kings paid taxes to the Hephthalites regularly for decades. But we also have data that the Hephthalite king repeatedly decided which claimant to the Persian throne would be placed on the throne. Recent research has shown that the Sassanid

265
===== PAGE 262 =====
Only under some prominent rulers did Persia extend to the northern and eastern regions of present-day Iran, but for the most part it relied only on southwestern Persia (Fars, Susiana), Media and Mesopotamia as a permanent tribal territory. The region of Khorasan and Merv, for example, was already a disputed territory, sometimes under Persian, sometimes under Hephthalite rule.
From all these data it follows that the various views of our historiography, all of which start from the a priori assumption of the migration route of the Onogurs, Sabirs, and then the Avars from northern, Western Siberia via the Ural River and the Volga, are all false and should be rejected. The sole purpose of these assumptions is anyway to move the "Finno-Ugric" ancient Hungarians from the Ural Mountains to the Sea of Azov in the 5th century AD, where they were first mentioned in historical sources.

However, we cannot leave without mentioning that this hypothesis of the Onogur-Sabir migration originating from the Urals or Western Siberia had no objective or historical basis even at the time of its invention. On the contrary, the already well-known Armenian sources unambiguously describe that the Ogurs and Bulgars lived in the immediate vicinity of the Armenians, in the Caucasus and its northern foreland, long before the great Hunnic migration. For example, the 5th-century Armenian historian Mózes Chorenai, in his description of the events of the Parthian period, repeatedly mentions the Ogurs and Bulgars, who also settled partly in Armenia. But the Byzantine Agathias, writing in the 6th century, also reports that in his time there was an old fortress in Transcaucasia called "Onoguris", because the Onoguris were defeated there in the past.
According to Ammianus Marcellinus, the Sabirs lived in Dagestan as early as the middle of the 4th century.
So we can choose any name we want: "ogur", "onogur", "sabir" or "bulgar"; — each of these ethnonyms, with contemporaneous source data, evidently appears in the Caucasus region before the Hunnic times, and not in Western Siberia or the Urals. In other words, these Armenian, Roman and Byzantine data, even separately, very strongly support the evidence obtained by Haussig and Altheim from other sources about the southern route of the Onogur-Sabir-Avar migration.
But it is also very characteristic that Chorenai Moses was considered by Indo-Germanic historiography - and, following him, of course, by Hungarian historical research - for decades as a late compiler of the 8th or 9th century, who only lived back to the 5th century and wrote as if he had lived then. His old Parthian-era data on the Ogurs and Bulgars above could not be included among the peoples of the pre-Hunnic Caucasus region - according to them, pure "Aran", Alan and other - peoples. Chorenai therefore became a fraud, for the same reason as countless other old chroniclers who wrote such things.

266
===== PAGE 263 =====
They dared to write things that contradicted the preconceived views of 19th-century Indo-Germanic historians.
Modern, objective criticism, however, has fully rehabilitated Moses Chorenai. It has been established that his original work dealt with Armenian history only up to 440, and that what is found in the surviving manuscript about later events is undoubtedly a subsequent interpolation (Brockhaus Encyklopädie, 1971; under the heading "Movses Chorenatzi").
These insights have recently made their impact felt in Hungarian historiography. Not directly, though — they are ostentatiously silent about Haussig's article here — but "by the way, in passing". The but hypotheses according to which the Hungarians lived in the area of Bashkiria until the end of the 8th century or the beginning of the 9th century, and from there migrated — not long after the conquest 6166 — to Lebedid have increased remarkably. It is also characteristic that for this purpose they have already begun their habilitation — the very school from which their most "storyteller" Anonymous partial re-bitter detractors have so far been expelled — and they diligently refer to the fact that, according to Anonymous, the route of the conquerors led through Suzdal.

Other aspects of HW Haussig's research | Hungarian historical theory
However, let us continue to examine Haussig's mentioned work, because we can find many other very interesting things in it, which cannot be ignored in our prehistoric research.

About the Avars, Onogurs, Sabirs
they lived in Turkmenistan, "true" Asian Avars, contrary to the previously widespread opinion, cannot be identified with the Zhuang-Zhuang people, but rather with the Western Turkmen. First of all, and in the Chinese they were called "a-pa" and in the Persians "abar". Around 310 AD, a branch of the Hun or "Gun" people occupied Western Turkmenistan and absorbed the people called Var" (. Abar) who had been living there. The names of the two leading tribes of the European Avars, on the other hand, were Var" and "Chunni", which obviously shows this Turkmen admixture. The Oghuz, Onogur and Sabir tribes called them "Avar" or "Apar" both in Asia and in Europe, and the Persian sources directly write the Avars as Hephthalites. As a result, Theophylactos' claim that the European Avars only adopted the Huns (v. Guns) and mixed heavily with them, and this is the ethnonym "abar", "apar" "Avar" laughs, just it is true that there were not pure "Apars" (or "Abars"), but the 267

===== PAGE 264 =====
more than likely the same as the "Aparnoi" people of the ancient Greeks, i.e. the Parthians. — So far Haussig.
If we examine these connections from the perspective of Hungarian prehistory, we must again realize that "István Horváth and his colleagues" clearly recognized many circumstances through their in-depth study of ancient sources that modern Hungarian historiography has not yet noticed. Considering that the Parthians, according to the unanimous information of all contemporary sources, emerged from the Scythians - more precisely from the Scythian "Daha" tribes - and comparing this with Haussig's above results, without any effort or "chasing a mirage", an unbroken, and if of course not complete, but undoubtedly very significant and remarkable partial historical and ethnic continuity leading to the Hungarians through the "storyteller" Hungarian chronicles and the historical picture of our pre-1848 historians: from the Scythians to Parthians, Huns, Avars, Sabirs, and others, involuntarily unfolds before our eyes. We emphasize that this historical perspective, represented by numerous Hungarian researchers 150 years ago, is still being used today. Indo-Germanic historians, who have been working conscientiously and with objective tools since 1945, are beginning to prove this in increasing proportions.

Therefore, we should not be surprised that in recent decades it has been primarily Hungarian researchers living abroad who have initiated the criticism of the one-sided historical approach based on the thesis of an exclusive Finno-Ugric origin. This is not about "emigration disease", with which slogan — most often instead of scientific refutations — the representatives of the Finno-Ugric trend have tried to push this criticism aside, but simply about the fact that Hungarian historians abroad, but also interested "autodiact dilitantes", are increasingly recognizing the flaws of Indo-Germanic historical research before 1945. Almost every day, they can witness how false beliefs that have been entrenched for a hundred years are being systematically overthrown with the appearance of a new scientific work.

What can a Hungarian expatriate think if, for example, he reads in the V. volume of the "Reallexikon der Assyriologie" that "in the 2nd millennium BC no "Indo-Iranians" can be detected in Mesopotamia, the previously assumed Aryan ruling class of the Mitanni Hurrians" is therefore a myth, and then he comes across the volume "Hungarian Prehistoric Studies", and there Harmatta not only states the Mitanni Aryan "Herrenschicht" (ruling class) unchanged, but also counts Indo-Iranians at the end of the 3rd millennium BC" in Mesopotamia? There is no need to elaborate on such examples further, they can be found with Garmada.

Returning to Haussig's thesis, he further emphasizes that in the middle of the 2nd century AD, Ptolemy already calls the Ural River by its Turkish name, "Djaiz" (Jaik) and mentions the "ýýý", or "Cun" people north of the Caucasus. At this time, the presence of Altaic peoples in Eastern Europe can already be proven. Moreover, even much further back than this also in times

268

===== PAGE 265 =====

looks back: the name of the Scythian "Targitaos" reported by Herodotus is strikingly similar to the later Altaic "targit" > "targyt"?" — "targyn" "+" "tarkan" + "tarchan" dignity name.

in Attila's empire (reported by Jordanes). "Onogur" or "Hun-Ogur" tribes lived west of the Sea of Azov, as well as in Dacia, Moesia and Thrace, and after the collapse of the Hunnic empire, Majd describes how they moved further east again

This information from Jordanes was already used by Géza Nagy to make us, as chroniclers, believe that this issue "the second arrival of the Huns into Pannonia" explains the expression (Our ancestors who conquered the homeland). We, "second", but should be thoroughly examined, not only from this perspective. increasingly emerging from recent research "third income"

Haussig's research results, which he presents in his detailed explanation of the circumstances of the Onogur-Sabir-Avar migration around 460, outlined above, are also very important. From the direction of the Sabirs' advance from Dagestan through the Caucasus, it naturally follows that the "Saragurs" and "Urogs" (v. , Urugs"), who lived north of the Onogurs at that time, in the Kuban region, together with the "Akacirs", gradually moved further and further north.

In this connection, we would like to emphasize the appearance of the Nevolino group of the Lomovatonv culture, which we have already discussed, in the Kama-Bjelaja region at the turn of the 5th-6th centuries, barely two or three decades after the Sabir-Onogur-Saragur-Urog-Akacir migrations. It is obvious that the appearance of the Nevolino people cannot be separated from these Caucasian-steppe people movements, but can only be viewed in close connection with them. That is, the Nevolino ethnic group — similarly to the Hatinal discussed above — in all probability appeared in the Kama region as the real, historical "Ugors": the Saragurs, Urogs, Onogurs or the groups of Hunurs who migrated north.

However, Bartha carefully conceals all these connections. For him, these historical data are negligible. What could the theory of our Finno-Ugric origin have to do with these Ugrians who migrated north from the Caucasus and the Sea of Azov region, when under the ethnonym "Ugrians" — since the clever mystification of this name by the Hunfalvys — our researchers understand only the ancestors of the Voguls and Ossetians, among whom they also include the ancestors of the Hungarians. We will return to this "Ugrian" question later.

However, let us continue to follow Haussig's arguments. According to the testimony of Ammianus Marcellinus and other sources, the Sabirs lived in Dagestan even before the Hunnic migration, he writes.

From this data he concludes that the Sabirs could not have been Altaians. – We must also point out here that according to Altheim they are certainly not Turks, but not Iranians either (Geschichte der Hunnen).

269

===== PAGE 266 =====

This question 06316 requires detailed investigation and analysis, because it is more than likely connected with the questions of the origin of the Hungarian language. We recall in this connection 15 that Arab authors describe the language of the Khazars — who, according to Masudi, were actually Sabirs — as a completely separate language, which does not resemble either the other "Turkic" or Iranian languages. We will deal with these linguistic questions in detail later.

A „sakauraka“, „asi“, „pasiani“, „tochar“ es „sabarol“ nepekröl

De Haussig also deals with much older events in his dissertation. In particular, he examines the ethnic composition of the Kushan Empire in eastern Iran, the Bactria-Indus region, which flourished in the 2nd-3rd centuries AD, since the various ethnic groups of this empire undoubtedly played a very important role in the Hephthalite-Hun empire that later developed in the same areas, of which the European Avars, as we saw above, are very important for us in many respects, because they repeatedly point to very remarkable connections with our own prehistoric questions,

var" and "chunni" strains were derived. The results of these studies

Let us first take Strabo's account, which is also the starting point for Haussig's investigations. According to Strabo, in the second half of the 2nd century BC — modern research places these events around 130 — the "Sakauraka" people came from the north, from the area around Lake Aral, and occupied Sogdia and Bactria. Strabo also gives the meaning of the ethnonym: "saka-uraka" = kings of the Sakas". Accompanying them appeared the "Asi", "Pasiani" (v. "Asiani", or "Paskin"), "Tochar" and "Sabarol" peoples, who initially settled north of the Sakauraks, but later, around 6.30 BC, defeated the latter and occupied their country. All these peoples were displaced from their previous homes by the westward-moving Asian Huns, the "Hiung-nu" of the KfnaI sources, and driven before them in a west-southwesterly direction.

In this communication, the expression "sakaurak" = "kings of the sakas", i.e. "lords of the sakas" or "saka-lords" - if it were to occur alone - could undoubtedly be dismissed as a coincidence. What is surprising, however, is that it does not stand in isolation, but three of the other four ethnic names mentioned with it show very close phonetic parallels, with the names of peoples who appeared later, which, according to the sources available to us, but also the results of recent archaeological research in southern Russia, were very closely related to the Hungarian history of the pre-conquest era.

270

===== PAGE 267 =====

The name "asi" is almost identical to the ethnonym "üz", "pa- siani" or "paskin" to the name of the Pechenegs (v. 6, also the form "pacinak"), 8 and finally + but not least -- "sabarol" to the old "szabir" of the Hungarians [ýýýý]. Haussig himself emphatically points out this latter similarity. However, in close connection with all this, the name of the Scythians preserved in Assyrian cuneiform inscriptions; "as-guz-ai" also appears, as well as the already mentioned ethnonym "aspasiak" given around 230 BC in the area of the Aral Sea (L. p. 237).

What should we think about these striking name parallels? Could all of them be coincidences? We cannot accept this in any way. In one of the five mentioned folk names, we can experience a perfect phonetic and semantic while in correspondence with the common word "urak" (even the plural is formed identically!), in the second it is almost completely Hungarian in its phonetic form, it is identical to the old name of the Hungarians "szabir", the other two it is with the folk names of the Uz and Pechenegs, which are closely related to Hungarian prehistory. — It should also be noted that the "kings of the Sakas".

The meaning of the ethnonym "sakauraka" "kings of the Saka" can be derived from Chinese sources, in which the name of the same people is "sai-wang", i.e. also,

(Franke: Contributions from Chinese sources...)

Under such circumstances, it would be a very superficial procedure to simply throw the entire complex of questions into the trash, citing chance.

But on the other hand, from Haussig's further statements, we get more clues, regardless of the above considerations, that there is obviously much more to this than mere coincidence. Haussig says that at about the same time, at the beginning of the 1st century BC, the "Asi" and the Alans appear on the eastern and northern shores of the Caspian Sea and north of the Caucasus, always mentioned closely alongside each other. Their migration to the west is as much connected with the western expansion of the Huns as the southward migration of the previously expanded Turkmen peoples, he states.

"as" people are undoubtedly a group of the former "Asi" people - he writes, These two peoples

Here are the Alans and the Aes, whose modern name is "osset" (Osset), that is, the Hungarians before the "os" (as) + Turkish plural: „j“, In a special way, also closely related to this people conquest stood for: 6. Osset, with our "indigenous words", about which we would like to say a few things later.

Haussig's statement is fully supported by the results of Tolstov's excavations in the Syr Darya estuary, where the Huns migrating westward appear in Kesken-Kujuk-Kalä, but also in several other settlements - as Tolstov writes - "at the latest around AD" (ýý)ýýýý: Auf den Spuren der altchoresmischen Kultur).

But this is also confirmed by contemporary Chinese sources, according to which in the 1st century BC, during the reign of Hun King Cistik, the Parthian Empire itself

271

===== PAGE 268 =====

also paid tribute to the Western Huns. Chinese annals from the same period repeatedly complain that Hun embassies were received much more favorably in Western Asian countries than Chinese ones (de Groot: Chinesische Urkunden zur Geschichte Asiens).

These data clearly show that, starting from the 1st century BC, the Huns shifted their center of power further and further west. We cannot therefore speak of a frantic, "nomadic" migration in their case, but rather of a gradual, tenacious, conscious conquest of territory.

About the Kushans

Turning to the Kushan Empire itself, Haussig shows that this state — like the horse-riding empires of the steppe — took its name from the leading tribes. The name of this leading tribe sounded like "kü-shi" or "kui-shang" to the Chinese, and the tribe itself belonged to the Var or Hua people. The population of the Kushan Empire was made up of the four peoples mentioned above, in addition to them: the Asi, Pasiani, Tochar and Sabarol peoples. According to Haussig's investigations, the population was a mixture of Altai and Iranian ethnicities.

Then he discusses in detail the connections between the Kushan Empire and the Hungarians before the conquest, which he considers indisputable. He emphasizes that the Hungarians had a lot in common with the Kushans, but also with the Hephthalites.

This is evidenced by two of their dignitaries' names, in the form of "jaula", and later also on Hephthalite coins. In addition, a "Kusan" and "gyula" are also the same. The title "gyula" occurs several times in Kusan inscriptions,

common feature is the alliance of seven tribes, which we find in the "Jue-csi" people, from whom the Kushans descended, in Western Turkmenistan, as well as later among the Hungarians. The old name "Sabarot Asphalo" of the Hungarians is associated with the name of one of the peoples that formed the Kushan Empire,

a identical to the "sabarol" strain.

Finally, Haussig summarizes the results of his research, according to which five major migrations can be identified on the West Eurasian steppe between 127 BC and 558 AD:

1. Around 127 BC, the migration of the "Great Yue-chi" people to Western Turkmenistan, Sogdia and Bactria, with the participation of the "Arsi", "Wu-sun ýýý", "Kü-f si" ("Kusân"), "Hua" ("Var"), "Sabar", "Asi", "Alan", "Paskin" ("Pasian") and "Tochar" peoples or tribes.
 2. At the beginning of the 1st century BC, the migration of the Ascs and Alans to the Yaik (Ural) Volga and Kuban regions.
 3. Between 310 and 453 AD, the great Hunnic migration from Western Turkmenia to: Nistân, Bactria (Hephthalites) and Europe (Attila's Huns),
 4. Between 440 and 470 AD, the first so-called "älavar" migration from Western Turkmenistan to the southern region of the Caucasus, around Dagestan.
- 272

===== PAGE 269 =====

5. Between 558 and 568 AD, under pressure from the Kók Turks, the second "41-Avar" migration took place from northwestern Iran (around Dagestan) through the Caucasus, first to the Kuban region, and then through southern Russia to Pannonia.
With the exception of the last, all of these movements were triggered by the expansion of the Asian Huns and their gradual westward advance.
It is unbelievable, after all, that it would be completely unnecessary and superfluous for us to thoroughly examine these questions and the numerous Hungarian-related connections that involuntarily arise in connection with them.

However, our historical research, built on the thesis of our exclusive Finno-Ugric origin, has nothing to do with these southern data, because they directly contradict the dogma of the northern, Vogul-Ostiak origin of the Hungarians. Thus, they prefer not to take such things into account. We ourselves have been able to establish that Haussig's dissertation, published in 1953, has been consistently ignored in Hungary ever since. We therefore consider it necessary to show the other side of the coin, in addition to Haussig's findings, in more detail. We begin our review with Hungarian researchers.

Hephthalite-Huns, Avars, Hungarians
The association of the Hungarians with the Hephthalites can be traced back to György Pray, who, more than 200 years ago, interpreted the expression "Sabartoi Asphaloí" as a composite, Greekized name for the Sabirs and the Hephthalites. The point here is not whether the interpretation itself was correct or not, but that Pray's conclusion, based on the Hun-Avar-Hungarian continuity — which was also taught by our chronicles, but also clearly revealed by numerous contemporary ancient sources — was completely reasonable and well-founded, since we could see from Bálint Kiss's work that the Hephthalite origin of the Avars and the Hunnic nature of the Hephthalites had been well-known historical facts that could be proven by contemporary sources since the 18th century. This is how historians knew it until European historical research, intoxicated with Sanskrit language comparisons and the discovery of the Indo-Germanic language family, "scoured" the entire Eurasian steppe in the 1930s and 1940s, following Klapproth: starting from various assumptions of linguistics, searching everywhere for the ancestral homeland of Indo-Germanic (O. Schrader: Sprachvergleichung und Urgeschichte).

At this time, among other things, the Hephthalites were named Iranians and the Avars were named Zhuangzhuang, with an effect lasting for more than a hundred years, hopelessly confusing the historical picture that had already been clearly emerging before. The ancient Indo-Germans, and the later ones, 273

===== PAGE 270 =====

According to this historical perspective, there simply could not have been any other kind of people in the area of the supposed Central Asian habitations of the East Indies or 6sárjás, no matter what the contemporary well-known sources said. At most, these were subjected to "source criticism", in the manner already described (1. e.g. Zeuss, Müllenhoff, Tomaschek's critique of Herodotus in Chapter 2 of the next volume).

We must emphasize that such a Pan-Indo-Germanic perspective Within the framework of "source criticisms", the method of removing a number of well data that did not fit into the was expressed as a "topos". This applies primarily to the identifications of Scythian-Hun, Hun-Avar, Avar-Hungarian and Parthian-Hun in contemporary reports, which were in sharp contrast to the Indo-Iranian ideas that flooded the steppe. This is the other side of the "topos" question, which we should not neglect in the light of new knowledge. Here too, a thorough revision is necessary, an objective, unprejudiced decision as to what can still be considered a topos and what not. The source criticisms with an Indo-Germanic perspective of the 19th and early 20th centuries can be called anything but objective. This is also proven by the new discoveries that have already in many cases confirmed the assumptions of Hungarian historians before 1848. For example, R. Rolle, in his book "Die Welt der Scythen", states that new archaeological finds have often led to the confirmation of various ancient sources that were previously (19th century!) considered unreliable. "fantastic" views of those who worked with these later discarded sources.

With the 19th century "conquest" of the steppes by the Indo-Iranian horsemen, the campaign against the medieval Hungarian chronicles began in parallel - logically - because their Scythian-Hun-Hungarian identifications contradicted the thesis of the dominance of the Indo-Iranians in the steppes, as did the similar statements of other ancient sources - which were successively declared to be topos -. From the perspective of 19th century Indo-Germanic historiography, we cannot find anything special in this, since, starting from a sacredly believed thesis - which the results of comparative linguistic studies at the time seemed to effectively support - the Hungarian chronicles could not be considered anything other than tales. After all, if even Herodotus himself had to get out of the way, we cannot expect that our chronicle would have been spared.

However, the Hungarian aspect of the question is not quite so simple. In our country - as we have seen - a very considerable amount of data had already been collected before 1848 by two generations of our historians, since Pray and Katona. All this data testified that we should continue to research our prehistory in the South Russian steppe, around the Caucasus, in Transcaucasia, in Southwest and Central Asia, in the areas where the history of the Scythians, then the Parthians, Huns and Avars took place. This is why Sándor Csoma Kyrösi set off towards Persia and Turkmenistan in search of the Hungarian ancestral homeland.

274
===== PAGE 271 =====

Pál Hunfalvy and his school threw all these not to be underestimated previous results into the wind and joined the perspective of Indo-Germanic historiography without reservations.
However, they, Hungarian scholars, had a considerable advantage over their foreign colleagues, because relying on the research work of their Hungarian predecessors, they would have been much more likely to have noticed that Indo-Germanic historical science stood in stark contrast to the written sources of the time in several ways. Here we are not primarily thinking of the Hungarian chronicles, but of the countless Greek, Latin, Persian, Syrian, Arabic and Chinese sources, a considerable part of which had already been collected by Hungarian researchers before 1848 and used with more or less success — but in any case in accordance with the level of science at that time — in their historical works.
Apart from this, however, the most striking thing is that Hunfalvy and his colleagues considered the most extreme, openly anti-Hungarian, German-Austrian trend of Schlözer's type within Indo-Germanic studies itself to be authoritative, and did not even consider more sensible, objective Western European researchers — for there were such, for example A. Thierry.
We do not wish to deal with this issue in more detail, but we considered it necessary to record the fact itself.
The Hunfalvy school gained ground more and more rapidly, not least thanks to the always psychologically very effective argument that we represent the current position of science, our views are in agreement with the opinions of scientists all over Europe, so to speak. Anyone who claims otherwise has inevitably fallen short of the requirements of systematic science, and therefore follows unscientific, amateurish ideas. Independent minds who persist in their own convictions under such circumstances, if they inevitably bring upon themselves the odium of unscientificity, are very rare indeed.

The opposition to the Hunfalvy school around the turn of the century
However, in Hungary there have been many such independent spirits over the past hundred years. It will not be uninteresting to get to know some of them. All the more so because the dominant academic history and linguistics consistently ignores the work of all our researchers who did not hold the thesis of the exclusively Finno-Ugric origin of the Hungarians. Károly Fiók sharply opposed Hunfalvy and refuted Hunfalvy's arguments point by point with thorough counterarguments, with which the latter attacked the credibility of our chronicles (Ancient History and Criticism). Among other things, he stated that "the land of Evlâth" is nowhere to be found in the Bible. Hunfalvy simply invented the idea that it was a biblical expression in order to prove it; look, Kézai compiled the ancient history of the Hungarians from the Bible,

275
===== PAGE 272 =====

We have looked into this ourselves, and we can state from our own experience that there is indeed no "land of Evlath" in the Bible. The only name that sounds similar to "Evlath" is "Havilah", but this is not a specific earthly area either, but rather a mythological region around the Garden of Eden (Genesis 2.11), and the name of one of the sons of Cush (Genesis 10.7 and Chronicles 1. 1.9) and one of the great-grandsons of Shem (Gen. 10.29 and Chronicles 1. 1.23).
Hunfalvy therefore obviously identified this mythological "Havilah" around the Garden of Eden — completely arbitrarily — with Kézai's "Evlâth".
Fiók, by the way, quite correctly points out that "Evlâth" preserves the name of the Hephthalites in Kézai. We will return to the question of Kézai's alleged "biblical" sources later.
It is very remarkable that Fiók interprets the so-called "Ugric-Hungarian" languages as a separate category, considering them as a proto-linguistic group between Altaic, Finnish and Aryan languages. In his opinion, this is confirmed by the so-called Aryan and Turkic loanwords of the Hungarian language, a good part of which cannot be considered ordinary loanwords, and the direction of borrowing cannot be clearly determined. Our Turkic loanwords are more than likely the results of reverse borrowing: the Turks took them over from the "Ugric" languages during their expansion in the 6th century AD, when they conquered several Ugric-Hungarian peoples.

Fiók's line of thought is that the extinct languages today called "Western Turkic", "Bulgarian-Turkic" or "Chusvian-like" would have formed the "Ugric-Hungarian" language group, to which he also classifies the languages of the Scythian peoples of the 1st millennium BC. Due to their central position, these "Ugric" languages (note the sharp difference between Fiók's "Ugric" peoples and the contemporary ethnic content of the term "Ugric") could easily have transmitted their linguistic influences to the Turks in the east, to the Finnic peoples in the north, and to the Aryans in the south and southeast. He supports this by saying that the older Indic sources we take, the more "Ugric" similarities they have, the stronger the "Ugric" linguistic influence. These "Ugric" linguistic influences exerted on the ancient Indic languages are dated from the 1st millennium BC. It is dated to the 2nd millennium BC, at the latest around 1500-1000 BC.

Today we look at Fiók's argument differently than his contemporaries. Since then, we know that the development of the Indo-Germanic languages most likely began after the turn of the 3rd/2nd millennium BC, i.e. almost exactly in the period when Fiók concluded the massive "Ugric" language influences on the ancient Indic languages. Moreover, Indo-Germanic studies have also established that the development of the Indo-Germanic languages was greatly influenced by the languages now called "Finno-Ugric".
Let us be content for now with recording these new insights, which clearly show how clearly Fiók recognized the real connections in the development of language families already at the turn of the century. 276

===== PAGE 273 =====
József Thürý . In his article "The Origin of the Hungarians, Their Migrations and Migrations", he exposes the Hunfalvy School for its methodological errors. He states that, so to speak, never fully examine the individual sources from which they draw their arguments. Their historical criticism is unreasonable because they simply take the silence of the sources as evidence (argumentatio éx silentio) if it fits their historical ideas, but at the same time, they do not take into account those contemporary writers who contradict their view. Hunfalvy, for example, categorically states that Leo the Wise and Constantine the Great wrote that the Hungarians were Turks because they did not know their real origin. On the other hand, Rubruk of the 13th century and Herberstein of the 16th century, who "heard" that the Hungarians had once migrated from Bashkiria or "Jugria", are first-rate evidence for Hunfalvy. This is unique reasoning, notes Thürý. Their lack of method is further culminated by the fact that they interpret the sources completely arbitrarily, and if necessary, even misrepresent them. Last but not least, the ethnonym "Ugor" ("ugur") or "ogur" ("ogor") is used unjustly and

It is confused without any basis with the name "Jögira", "Jogrà" or "Jugra". The ethnonym "Ugor" and its variants in contemporary sources from the 5th, 6th and 7th centuries AD always, without exception, unambiguously designate Turkic peoples, namely in the region between the Sea of Azov and the Caspian Sea. The name "Jugra", however, first appears in the Nestorian Chronicle at the beginning of the 12th century, and not only does it always refer to northern areas, but at its first occurrence it is sharply distinguished from the ethnonym "Ugr" also found in the Nestorian Chronicle, with which it has nothing to do: in the Nestorian Chronicle the name "Ugr" also designates a people (White-Ugr) that appeared in the foreground of the Caucasus at the beginning of the 7th century, and then at the end of the 9th century the Hungarians (Black-Ugr).

A few words about the color change of the "Ugric" people
Bulgarian, Onogur- The name "Ugric" has a separate history. All indications point to the fact that the Voguls and Ossetians received the name "Ugric", which in contemporary sources was indeed used to name the Hun-Bulgarian or Bulgar-Turkic peoples of southern Russia, so that the thesis of their exclusive Finno-Ugric origin could be more easily smuggled into the public consciousness. We emphasize that this is not a simple suspicion, but a statement that can be traced point by point in the scientific publications of Hunfalvy and his school.

The Hunfalvies gradually stole the name "Ugric" into the Finnish language family. At first they argued that these "Ugrians" = by whom they still primarily meant the Onogur-Hunugur, Ogur-Ugric Hungarians with Turkish connections (v.6. the Hungarians with the names "Ungar", "hungar" and "ugri") er
271
===== PAGE 274 =====

They form a kind of linguistic link between the Finnish or Uralic and the Turkic or yyyyy languages, within the larger framework of the Uralic-Altai language family. At this time, Jordanes' Hun-Ugurs and Theophylactos' European to those who did "Alavars" were also counted among the "Ugrians", whose real name was Theophylactos's "Ugric" or "Ugric" specifically. (Hunfalvy's Ethnography of Hungary, 41st par.) This position was naturally also fully acceptable not exclusively seek the origin of the Hungarians among the Uralic peoples, as well as the affinity of our language, because as we saw earlier from Károlyn Fiók, no serious researcher denied the close Finnish or Uralic connections of the Hungarian language, but they saw in this "Ugric-Hungarian" language group the connecting link between the Altaic-Torican and the Uralic-Finnic languages.

In the next stage, the "Ugric" languages have increasingly shifted towards the Uralic-Finnic language family, mainly based on the languages of our "closest language relatives", the Voguls and Ostyaks. At the same time, the real "Ugric", i.e. the Hun-Ugric, Onogur, Ogur peoples, were gradually excluded from the concept of "Ugric languages", and this term began to designate only Vogul, Ossetian and Hungarian.

Finally — after the term "Ugric" had become common and customary for these latter languages — the Altaic languages began to gradually "distancing" from the Uralic languages, until we reached the current position of Finno-Ugric studies: although the relationship between the Uralic and Altaic languages is supposedly "unprovable", one branch of the Uralic languages is still called "Ugric", which refers to the Hungur-Onogur-Bulgarian-Turkic peoples, without any doubt.

Regarding the real historical meaning of the name "Ugor", we refer to Bernát Munkácsi. The ethnonym "Ugor" or His dissertation "The Origin of the "Ugric" Folk Name", where all the different source data are ~~object and he also states in the preface to the book that he is not a Turkic peoples.~~
The "Ugric" question.

We have already touched on this point when presenting Thúry's article, which is also an excellent example of the characteristic methods of proof of the exclusively Finno-Ugric perspective. In the Nestorian Chronicle, and later in other Russian annals, the terms "Jögira", "Jugra", "Jugria" appear several times as the name of a territory located in the high north, somewhere between the White Sea and the lower reaches of the Ob. Then, in the first half of the 16th century, Baron Herberstein, Ferdinand I's envoy to Moscow, "heard" in Moscow that the Hungarians had migrated from "Jugnà" to Pannonia. This information was used by Schlözer at the time to state that the name "Jugra" is identical to the names of the Hungarians "hungar", "Ungar", "ungri", "vengre" that were widespread abroad. Later, his followers, Zu He claimed that

278
===== PAGE 275 =====
uss, Büdinger, Rössler and Dimmler also argued diligently with this agreement. From them it was transferred to Hunfalvy's repertoire and over time became one of his most beloved riding horses. We should not be surprised at this, because if it could have been proven, this would be the only source data on the northern origin of the Hungarians that could be traced back to at least the Middle Ages. However, as Thúry has already clearly shown, the two terms have nothing to do with each other (1. above). However, this did not prevent Hungarian historical and linguistic research with a Finno-Ugric perspective from re-examining this Schlözer-like idea through Zsiral up to the present day (Péter Hajdú).
It is extremely instructive to compare this convulsive effort and waste of energy, which was spent ~ and is being spent -- on the completely improbable "Ugric"—"Jugra"—agreement, with the kind of light hand gestures with which our historiography sweeps aside, for example, the thirty-seven sources collected by Thúry between the 6th and 13th centuries, which all call the Avars Huns (The Origin of the Székely). Despite the fact that recent foreign research has already established for 30 years that the Avars originated from the Hephthalite-Hun empire, so they had a lot in common with the Huns - not to mention the name of one of their tribes "chunni", i.e. "Hun".

Finally, we emphasize again: the naming of the Voguls, Ostyaks, Ugric peoples is completely arbitrary, misleading, and even downright false terminology. Neither did Géza Nagy accept the assumptions of the Scythians, Sarmatians and other steppe Netics, or of the Hunfalvy school. In several of his papers, for example, we find an exact prototype of Gyula László's "double conquest" thesis, naturally without Gyula László's archaeological support, which Nagy could not have known about at the time (The Prehistory of the Magyars; The Conquest of the Hungarians 16k). Gyula László himself pays tribute to Nagy's insight: he repeatedly emphasizes that it was Géza Nagy who first raised this idea. Nagy also clearly takes a stand in favor of the close historical and ethnic relations between the Huns, Avars, and the Hungarians. In his article "Our Ancestors Who Conquered the Land," he points out that the Hunnic word "vatni," recorded by Stephanus Byzantinus at the end of the 4th century, which means "desert," is identical to the Hungarian word "vadon." Similarly, Jordanes' word "Hunnivar" is not the Hunnic name for the Dnieper, as many claim, but clearly indicates from the text that it means a fortified place of the Huns, so it can be identified with the Hungarian word "vadon."

Géza Nagy's Jordanes analysis of the names of the Huns' descendants after Attila is also very important. According to Jordanes, the Huns were divided into two parts: the "Kutziagir" or "Ultziagir" and the "Sabir" people. The Sabirs were then divided into several groups.

===== PAGE 276 =====
He does not mention them, but instead begins to speak of the "hunugur" people. From this it is obvious that "sabir" and "hunugur" are synonymous names denoting the same people, just as the names "kutziagir" and "ultziagir" are also azes - Nagy states. According to Jordanes, these "shunugurs" transported the precious furs of the northern region to the Byzantines in the previous ae (cf. what was said on page 264).

We would also like to note something about this folk name "hunugur". The name "hunugur" appears in later sources mainly in the form "onogur" or "unugur". However, according to the findings of our linguistics, our name "hungar", "Ungar" used abroad, comes from the folk name "onogur", and -- this is always emphasized = "it has nothing in the world to do with the name of the Huns",

repeatedly explicitly , onogur", "unugur" and "hunugur" names applied to the same Pontic-Caucasian people group in contemporary sources. Well, considering the fact that, in addition to Jordanes, Byzantine writers also call both the Onogurs and the Sabirs Huns (e.g. thus: "onogur-huns" or Sabir-huns"), and on the other hand, the Huns are often referred to as "unni" or "ounno" (i.e. without the "h"), we certainly see little objective basis for this categorical distinction. Even less so because Constantine, born in Babor, calls the Hungarians directly Sabirs.

However, this meticulousness, which goes beyond hair-splitting, has an even more crude purpose: the prehistory of the Hungarians must be separated at all costs from the history of the Huns and Sabirs, because — after all — our Finno-Ugric origin is incompatible with such connections.
We cannot leave without mentioning that in the same paper, Géza Nagy emphatically warns Hungarian historians about the one-sidedness of the approach to Finno-Ugric derivation. We should not forget, he writes, that "the greater part of our linguistic relatives cannot be considered our blood relatives at the same time, but were formed from tribes of partly Aryan (Slavic and Iranian) and partly Arctic origin, which adopted the Ugric language that had been squeezed into their midst." (Cf. Károly Fiók's opinion; I. 276, p.)
In his book "The History of the Conquest of Hungary", Gyula Rohonyi deals in detail with the question of the authenticity of our chronicles, mainly with Anonymous, as a result of the objective of his work. In a calm, sober voice, with thorough reasoning, he rejects Hunfalvy's views on our chronicles. In the introduction, he mentions Anonymous's foreign attackers, against whom, in his opinion, we should already be suspicious, because, "they mostly write against him so passionately that their presentation makes racial hatred disappear at first glance." Rössler, for example, writes about the Hungarian chronicles in this way; "They must be thrown into the fire." Then he states that Hunfalvy fully agreed with Rössler's opinion.
280
===== PAGE 277 =====
and he also dismisses our chronicles as fables. Namely, Anonymous is generally not considered a reliable source for two reasons: 188 because he does not know much that the Byzantine writers know about? 2. Because he also writes about things that the contemporary (Byzantine and Frankish) chroniclers do not know about.
It is worth reading Rohonyi's considered, 161 well-founded objections, with which he nullifies Hunfalvy's argument on pages 108-109 of his book. As a final conclusion, he states that Anonymous is as credible or not as other contemporary foreign chroniclers. All of them should be measured by the same standard, not by criticizing Anonymous from the perspective of a modern historian, while remaining silent about the other parallels.

We believe that there is nothing special to add to Rohonyi's clear, objective sentences, except that it would certainly not have been to the detriment of Györfy himself if, for example, he had approached the chronicle of Aventinus based on the principle of "equal measure".
Károly Antal Fischer also did not accept the Iranian origin of the Scythians, Sarmatians, Dakhas, and Sakas. In his book "Erklärung der skythisch-sarmatischen Namen und Wörter aus der ungarischen Sprache", similarly to Géza Nagy, he refutes with sound arguments Zeuss, Müllenhoff, Tomaschek that the identification of the Sakas with the Huns and then with the Turks cannot be simply dismissed as a topos, because numerous sources also clearly state that the latter were relatives of the Scythians. Likewise, the Hun-Avar and Avar-Hungarian identifications of the contemporary well-heads are not fables, because according to our historical knowledge these peoples had a lot in common with each other.
He states about the Finno-Ugric theory that we can only speak of linguistic kinship, but not of racial kinship. Language comparison can be of great help to the historian, but only if he starts systematically from historical data and sources, and never ignores them in his linguistic-historical conclusions. However, this very methodical approach is missing from Hungarian linguistics and historical science with a Finno-Ugric perspective. This has led to the conclusion that, according to our linguists, the Hungarians did not have a language until not so long before the conquest, because almost all their words had been borrowed from various foreign peoples since then. It is also a strange phenomenon in our country - he writes - that our scholars are struggling with hands and feet against the repeatedly demonstrable historical data that # at least partly ~ from the Huns, and through them the Scythians

We come from them, but even more strange is their effort to prove that every Hungarian characteristic - whether it be an artistic achievement, an industrial object, an intellectual concept or a word indicating higher culture - is borrowed from other peoples and languages.

281

===== PAGE 278 =====

Fischer's syrsin theory in the light of recent research

We know very well that Fischer is often called a dilettante or a fantasist. He was considered such even in his time in the Hunfaly circles. He had - among others - a "madness", which was particularly useful: he explained it in the sources. He found out that while in the central areas of Hungary the white sztyr was largely dominant, in Transylvania and in the western part of Transdanubia, in Göcsej and űrség, they almost exclusively wore black sztyr. According to him, these were the "black Hungarians", i.e. the border guards of Avar-Szekely origin. The meaning of the names "black" and "white" Hungarian is related to the color of the clothing – namely the scarf.

In support of this, he also points out that the areas of the two main dialects of the Hungarian language also correspond exactly to the distribution zones of the black and white Szürk.

Continuing on this line, he claimed that within the two main colors, the colors of the borders and decorations indicate the seven tribes: black, vermillion, scarlet, steel green, water green, dark blue and sky blue. This can be followed so precisely -- he writes -- that we know, for example, that the Székelys had only six tribes or clans and lo and behold: steel green is missing from the border colors in Transylvania.

Having reached this point in his thinking and investigations, he asked the competent ministry to attach a questionnaire to the next official circular, in which he wanted to ask all the Hungarian-populated villages in the country about the decoration of szűrók and other folk costumes, and the exact color composition of their patterns. The ministry asked the Academy of Sciences for an opinion on this. The Academy's response called this experiment completely unscientific and even ridiculous, whereupon the ministry naturally rejected Fischer's request.

Let us stop here for a moment. We do not have the opportunity now to examine in detail and decide whether Fischer's theory was ridiculous or not. But that is not the point for now, but rather that it would have been so ridiculous and unscientific to obtain a complete picture of Hungarian folk costumes, their motifs and their color combinations, practically free of charge and without any effort. We would not even have had to spend research time on it, because he would have thrown himself into processing the received material with the energy of a man possessed.

Later, in the 20th century, the same folk costume collections were carried out by our folk artists with incomparably greater effort and expense, not to mention the fact that the material is far from complete, because in the meantime, as a result of urbanization and the spread of bourgeois fashion, folk costume objects, data, and even

282

===== PAGE 279 =====

even their memory. Unfortunately, we cannot find out about this. On the contrary, we must say that the German-speaking man in his childhood certainly had more love and respect for the people of his chosen homeland than our scientific life at that time. The steel green, as well as the light green border color, which only occurs sporadically in the central regions of Hungary, can be found abroad, for example in "fantast" continued to weave the endless thread of his imagination and revealed that the missing from Transylvania Dalmatia, in the vicinity of Ragusa vecchia (now Dubrovnik), precisely in the region where, according to the sources, the Avar kagan Baja settled 10,000 Kutrigur warriors, who, according to the testimony of Constantine born in Bitorban, remained there. In this region — writes Fischer — Hungarian decorative methods, light Hungarian trousers, various Hungarian customs, such as the funeral pyre, the guarding of the dead, can still be found today, accompanied by many Hungarian-sounding place names.

Fischer also finds it very remarkable that in Bavaria, as well as in the folk costumes of Upper and Lower Austria, the green border on a dark gray or black background is very common. These regions were also once part of the Avar empire, so we can count on the remains of the scattered Avars here too, he writes. Moreover, in his book *Die Hunnen im schweizerischen Eifischthale*, he directly states that a strong Avar admixture can be detected among the Bavarians and Austrians.

These latter "delibābs" probably provoked not only simple amusement, but even loud laughter at the time of We do not laugh, but we do not even argue. As mentioned, we are "competent" in scientific circles.

not in a position to delve into the issue of Fischer's theory of szürszin in detail. Therefore, we will only briefly refer to Gyula László's recent studies on the very significant role of the Avars, and then the Onogur-Bulgarians — the "griffes-indäs" — in Dalmatia and the northern Balkans in the 7th and 8th centuries. (For more, see 1. Gyula László: The story of the sons of Kovrat Khagan.) Then we will present the relevant opinions of some 20th-century German and Austrian researchers.

In his article "Langobarden, Bajuwaren, Slaven", E. Klebel states that the origin of the Bavarians has not been discovered to this day (1939). However, it is certain that they were a combination of many small tribal groups from the Migration Period. He says that in the so-called "Rabenschlacht" legend, the guardian of Attila's sons is the Bavarian Elsan. The guardians are killed by the Lombard Wittich, and a Bavarian-Lombard war breaks out — to avenge this —

Klebel's explanations about the names Chiemsee and Chiemgau are very interesting. The word "chiem" cannot be etymologized from the German language, he writes. Origin-283

===== PAGE 280 =====

It could have sounded like "kim" or "gim", or maybe "k&m" or "gem". Given the marshy surroundings of the Chiemsee, Klebel suspects it to be the name of some water-loving marsh animal,

And we raise the question: isn't the Hungarian word for marsh wader, "heron" (Reiher), hidden in the name of the Bavarian Chiemsee?

F. Ertl writes in his book *Topographia norici* that the Bavarians are a very mixed people, with significant Avar and Slavic components in addition to the Germanic. He shows that several places called "Obemdorf" in Lower and Upper Austria should not be interpreted as "Oberes Dorf" (upper village), but as "Oborendorf", i.e. "Awarendorf" (Avar village). This can be clearly proven, for example, in the case of Obemdorf, located on the Krems River in Upper Austria, south of Linz, by the Avar cemetery found next to it. He also points out that the Austrian Slavs were under the rule of the Avar ruling class even in very late times, in the Carolingian period.

This is also proven by the results of the research of the Slovenian toponym researcher Primus Lessiak on the origin of the Carinthian "Edlings". The "Edlings" formed a separate order in Carinthia until the end of the Middle Ages. In these later times they were merely free peasants, but their name (Edling = "noble offspring") and their important role in the order constitution of the Carinthian duchy – for example, at the inauguration of the new duke – make it clear that in earlier times they performed some kind of people-leader, kenezig function and belonged to the nobility.

Well, Lessiak showed that their Slovenian name, "kazaze", cannot be etymologized from Slavic languages, but in Altaic Turkic languages it has a clear meaning: "kazak" or "gazaq" means a free, independent person ("ein freier", "unabhängiger Mensch"). This probably includes the name of the Cossacks, Lessiak notes. He then states that the word could only have come into Slovenian from the Turkic languages, namely through the Avars living among them, who formed the dominant stratum of the Western Slavs in the early Middle Ages. The Carinthian "Edling"-

Therefore, in all probability, we must consider them to be descendants of the Avars. (Lessiak, P.: Edling-Kazaze.)

F. Altheim's work 'Geschichte der Hunnen' in volume V shows that many Avar place and personal names are found in Bavaria and Upper Austria. According to several researchers, the Bavarians were heavily mixed with the Avars -- he writes. In volume IV of the same work he shows that Edekon, Attila's chieftain, was not a Skyr-Germanic, as Hungarian researchers claim (Hammatta), but, according to Priscus' clear words, a Hun: "aner Skythēs and 'tou Onnou genous" -- Priscus writes about Edekon. Likewise, Eskas, Berichos, Onegesios and Skottas were Huns, whom Hammatta also considers to be Goths -- he writes (id. m. vol. IV, pp. 283-284).

Well, we have come this far in our convulsive efforts to meet the requirements of 19th century Indo-Germanic studies in every way,

284

===== PAGE 281 =====

We should devalue the hunks as much as possible, and consider their leaders to be Goths, because, after all, only Indo-Germans can lead (cf. the "Herrenschicht" theory).

Here again, the decline of our historical science is palpable. It seems that we have not yet noticed that the wheel of world history has turned a great deal since the beginning of our century, and with it the approach to Indo-Germanic linguistics and history. As a result, we can state with mixed feelings that a German scientist is instructing our researchers not to look for "Indo-Germanics" where there are none.

But we learn even more interesting things from Altheim. Also in the "History of the Huns" (Volume I) we can read that the word "reges" mentioned by Olympidoros for the Huns is simply the plural of the Latin "rex", meaning "kings", not the Gothic "reiks" ("rich"), as Harmatta claims without any basis (The Disintegration of the Hun Empire). "Man ist erstaunt zu sehen, dass dergleichen erwogen werden konnte" — writes Altheim (id. m. Vol. I, p. 364), which in Hungarian means "One is astonished to see that such a thing could have been thought of at all".

Yet Harmatta did nothing else, merely starting from the fundamental methodological rule of 19th-century Indo-Germanic studies, according to which kings cannot even be assumed among Huns, Avars, and other similar wild barbarian nomads — he simply applied "textual criticism" to Olympiodoros. Thus, the inserted Latin word "reges" in the early Byzantine Greek text became - also strictly in the sense of the older "Indo-Germanic" logic - Gothic "reiks".

Incidentally, we note that in the time of Olympiodoros, at the beginning of the 5th century AD, the state language in Byzantium was still Latin, Latin rather because the Greek word "basileos", originally meaning "king", was already the title of the emperor at that time. The insertion of the word "rex" is therefore completely obvious.

"was, so it could not be used to designate any barbarian kings."

It is high time to weed out such 19th-century Indo-Germanic "methods" from our historical and linguistic research. However, there are serious obstacles to this revision.

All signs point to the fact that — as we have already mentioned — the doctrine of our exclusive Finno-Ugric origin can only be maintained by these one-sided methods, just as this thesis owed its conquest and dominance to these methods in the early 19th century.

H. Mitscha-Märheim's treatise "Die Herkunft der Baiern" sheds light on several very important questions of early medieval history. It provides much data that is also important to us, so we will describe it in a little more detail.

285

===== PAGE 282 =====

We learn that the "Skir" tribe, generally considered Germanic, had remarkably close ties with the Huns, much more so than the other conquered Germanic peoples. While the latter all turned against the Huns after Attila's death, the Skirs (, Angiskirs") remained with Dengizli to the end. They also participated in Dengizli's Gothic campaign and were

After 1000, with the consent of the Goths, they settled in the area between the Sárviz and the Danube-Tisza 6 (reported by Jordanes), in the vicinity of the Hunnic tribe called the "Sadag", who had been living between the Danube and Lake Balaton since the time of Attila, in the area of the counties of Pilis, Fejér and Veszprém.

In passing, we note: it seems that the Goths could have thoroughly beaten Dengizik — as Jordanes also writes — if after the campaign the victorious Goths were forced to cede part of their settlement lands to Dengizik's allies, the Angiskirs. In this case, there was no need for "source criticism", which would have revealed this glaring contradiction of Jordanes.

The prince of the Scyri, Edekon (or Edeka), was probably a Hun - writes Mitscha-Márheim, referring to Priscus (cf. above: p. 284). His sons, Odoacer and Hunwulf, played a prominent role in the history of the last decades of the 5th century. After 470, Odoacer marched into Italy accompanied by Turciling and Herul troops. In 471, the Goths also moved out of Pannonia. The Scyri, Szadags and Sarmatians took their place. The Scyri took over the leadership. The old Edekon and his other son, Hunwulf, were the only significant personalities in the Pannonian region at that time. Hunwulf was sent by his father to Byzantium, where in 475 he was appointed military commander (magister militum) of Illyria. This then included Pannonia, i.e. Hunwulf's own territory. With this appointment, Eastern Rome practically recognized Hunwulf's rule in Pannonia.

Around 488, the Ostrogothic king Theodoric's march against Italy cut off the direct connection between Hunwulf's Pannonia and Odoacer's Italy, and even the road from Italy to Noricum.

It also threatened the roads. Odoacer then took the Romanized population of Noricum across the Alps to Italy, while Hunwulf led his peoples—the Scythians, Szadags, Sarmatians, and fragments of Suebi and Heruli—west along the Danube. These tribes settled around the Enns, Isar, and Lech rivers. They formed the nucleus of the later Bavarians.

In summary: the Bavarians are a mixed people, with a significant amount of Altaic ethnicity. The art historian Oettinger had already suspected this historical factor long ago, due to the fact that late medieval Bavarian art was completely different from the style of the surrounding German and Austrian regions and had oriental characteristics. Our investigations, writes Mitscha-Márheim, have found this suspicion to be historically correct. The Bavarians were indeed a mixture of peoples from Pannonia after the death of Attila, and they include a very significant Hunnic-Turkic element.

286
===== PAGE 283 =====

Long before Altheim and Mitscha-Márheim, similar ideas were formulated by American researchers RL Reynolds and R. S. Lopez in their book "Odoacer: German or Hun?"

In their dissertation, they showed that Germanocentric historiography, but mainly philology, very often records names from the Migration Period as Gothic or other Germanic names, which, from the data and circumstances available to us, can be clearly established with common sense that they could only be Hun names. Edekon, as well as his sons: Odoaker and Hunwulf, for example, were demonstrably Huns, and Edekon was directly a member of the Hun royal family, Attila's nephew. His father or grandfather was a Hun king, mentioned in the sources as "Rova" or "Ruga", "Roga", Attila's uncle: "de genere Roga, (v. Rova)" -- Priskosz writes about Edekon. This data was interpreted by Rua", Germanism, so that Edekon would have come from the Germanic tribe called "rugi",

We can add to this by saying that Priscus, with the generosity that 19th- In addition to the expression "de genere Roga", he also wrote about Edekon that he was "Ounnou gé- nous", i.e. of Hun nationality. This century Indo-Germanic historical scholarship is accustomed to when it comes to sources that point in a non-Indo-Germanic direction, skimmed over the second Priscus data.

On the other hand, the "Rugi" origin would also mean that in this case Odoacer would have exterminated his own people in 488, when he led a war against the Rugi in defense of the Romanized Jakoss4g of Noricum and, according to the sources (Vita Sancti Severini), he organized a horrible bloodbath among them.

In relation to the above, we only note that it would perhaps be a more fruitful occupation for Hungarian historical science if it devoted its time to similar research instead of "Gothizing" Attila's chieftains and "enriching" the Huns' vassal kings.

In his book "Dunkler Jahrhundert goldene Spuren", Mitscha-Márheim again deals with the issues of the Migration Period. He supplements the results of his previously presented dissertation in several respects. Based on his research, Odoacer's "Turci-ings" were most likely Altaians, not Germanic, as previous research had assumed. Their name, "Turci-ling", is clearly Turkish. The "Sadag" tribe is also undoubtedly Hunnic, since they formed Attila's bodyguard at the time.

We also learn that a permanent chancellery operated at Attila's court, and all events were regularly recorded in Hunnic script and language. "Attila himself knew Hunnic script. The Hunnic army towered above the European warfare of the time. The alleged Roman victory at Katalaunum is in all probability a lie.

After a lost war, Attila could not have been in Italy, in the heart of the empire, in the spring of the following year. But there is direct evidence for this: the description of the battle 287

===== PAGE 284 =====

Textual analysis showed that Jordanes copied Herodotus almost verbatim, namely the description of the Battle of Salamis (Altheim).

So, in the 1960s, historical research — not Hungarian, but German! — reached the self-evident conclusion that Géza Gárdonyi had already firmly stated at the turn of the century in his comments on "The Invisible Man." What is most saddening, however, is the extremely primitive pettiness with which our "competents" reacted to these foreign insights: the comments in question were removed from the 1971 edition of "The Invisible Man," citing flimsy arguments.

— Like an ostrich...

This incidental phenomenon alone clearly shows how far our historical science has fallen behind the international level. We must fear even poor Gárdonyi, because the youth reading the book might notice the glaring contradiction between Gárdonyi's logical line of thought and the approach they are supposed to learn in school. Indeed, one might even ask why in the 1960s the Germans had to "rediscover" what a self-taught Hungarian novelist had clearly demonstrated with methodical argument a good half a century earlier.

But let us continue our introduction. The Avars brought the Slavs to Central Europe and the Balkans - Mitscha-Márheim writes further - and yyyyyyyyyyyyyyy

They were settled on their western and southern borders towards the Franks and Byzantium. Around 670, after temporary internal disturbances — during which the Slavs of the western steppes were able to briefly gain independence from the Avars under the Frankish Samo — a new wave of people appeared from the east, with new motives. This was the so-called , griffes-indás" style. 110710261 they arrived in Pannonia from the Caucasus foreland. This is also confirmed by the close Iranian connections of their art. After 680 these griffes-indás late Avar settlements expanded far to the west. They firmly established themselves in the entire Alpine foreland, they re-absorbed the Slavic country of Samo, and even conquered the Bohemian basin. During this period, the Avar settlement area up to the middle of the Vienna Woods was closed. But he also emphatically points out that Samo and its country are mentioned only in Fredegar's chronicle, and its "empire" is archaeologically "intangible".

In 631, the Avars expelled 7,000 Hun-Bulgarian warriors from their empire, who had fled with their families to the Bavarians. They were allegedly killed by the Frankish king Dagobert, according to Fredegar. The Bulgarians who escaped the massacre settled in Carantania, with Prince Walluk (l. Lessiak's findings presented earlier). However, according to several archaeological finds and some Bavarian place names, these Bulgarians were probably not killed.

288
===== PAGE 285 =====

In connection with this question, we refer to E. Schwarz's work "Die Orsnamen des östlichen Oberösterreich", in which he derives the 4 1 . place name "Pulgar" from the name of these refugee Bulgarians. He writes about two places, now called "Oberndorf", that in 888 and -889 they can be given in the form E,Obrinindorf", which does not fit the sound of the Old German word "obaro" (ufelso"), (V. 6, Ertl megállapításával, 130- . 1313 Old.)

Mitscha-Márheim also points out that the Onogurs and Bulgars can hardly be separated from the Huns. Contemporary sources repeatedly mention them as descendants of the Huns. Already during the Huns' western conquest, in the 4th-5th centuries, the Onogurs migrated with them. ,proto-Bulgars?"; Similarly, contemporary sources report large Onogur-Bulgar contingents among the Avars;

. Regarding the data, we can also add that that according to the sources described above, the Avars themselves had difficulty can be separated from the Huns: they came to Europe from the Hephthalite-Hun empire, Their tribal name was "var-chunni" and 37 contemporary or nearly contemporary writers call them Huns (1, Thüry: The Origin of the Székelys).

._ Having reached this point, we leave it to the reader to decide whether ... how fantastic was Károly Fischer.

Moreover, in light of the recent foreign research presented above, it would perhaps not be a completely useless pastime if our ethnographers also thoroughly reconsidered their opinion that Fischer's assumption is wrong and does not correspond to the color scheme of the sub-decorations (1. Gy. László: The ethnonym material of the southern and eastern Káspi region in the early double conquest", p. 147).

Parthian period and the Sabir-Hungarian question).

We considered this more extensive digression necessary, because only with the knowledge of the above can we assess in their full perspective and importance the eminent significance of the southern route of the Onogur (Hunugur)-Sabir-Avar migrations shown by Haussig and Altheim for our research on prehistory, and on the other hand — in addition to this — the statement of Constantine, who was born in Bilborban, about the old Sabir name of the Hungarians. It is undoubtedly clear from the above that these Sabirs belonged to the Hunnic peoples according to contemporary sources, and their name is also known by several contemporary sources in the form of "Hunugur", "Unugur" and "Onogur". We must look for their ancient places of residence in Transcaucasia, around Dagestan, as well as in the neighboring areas connecting to the East, in the region of Northern Iran and Southern Turkmenistan. They migrated from this region in the 1st century BC. Around 460 BC, to the west, under pressure from the Avars, who were none other than a significant part of the Hephthalite-Hun empire,

289
===== PAGE 286 =====

Their tribal group, and even their name, apparently coincides with the ancient Greek name of the Parthians (1: Haussig). This is not . Therefore, we must look for the ancestors of the Magyarság named Sabir among the "Hun" peoples, in the Urals or the area around the Ob. Some of their groups only migrated there later, The Sabirs were undoubtedly pushed westward by the large-scale expansion of the Hephthalite empire that occurred precisely at that time, that is, by the "Avats." At the same time, the Hephthalites conquered the Indus region and made Sassanid Persia their tributary.

These peoples, who appear to be of "Altaic character" - we use this term for lack of a better term, because it does not exclusively refer to Turko-Mongol ethnic groups - lived in the region of Turkmenistan-Northwestern Iran even in the Parthian period, and for centuries they had very close political and ethnic relations with the other peoples of the Parthian Empire.

We have seen that the name of the Avars more than likely coincides with the original name of the Parthians, "Aparnoi". In addition, to support this . The name "Sabir" is also closely associated with the Parthians.

we must go back to the Seleucid times, night.

Speaking about the events of the 3rd century BC, the nearly contemporary sources are Strabo and Justin (both writing after the turn of the 2nd/1st century BC, following Apollodorus of Artemita, a Hellenic-Parthian by origin) — north of Khorasan, between the eastern shore of the Caspian Sea and the Aral Sea, lived the Scythian "Dahā" tribes, closely related to them, the "Aparnoi" or "Parnoi" tribal group. East of the Aral Sea, the Scythian "Saka" tribes also lived. — It should be noted that the real name of the tot, and then next to the latter the "Sparnoi" people are mentioned (Altheim-Stiehl; id. m.). The "Aparnoi" tribes emerged from the Dahā, these were actually Parthians. The "Ska" was, and the "Scythes" sound form is only the Greek Scythians is a distortion. V. 6. , szkyt"; just like "asi" + "-t" = "osset" (1. Altheim: Geschichte der Hunnen, vol. 1).

"saka" + "-t" ("altāji" plural sign) = When around 230 BC

Arsaces I or the Great was forced to retreat from the invasion of Seleucus II, he retreated north, to the "Aspasians" people living around the Aral Sea, and later with their help he reconquered the lost territories.

From the middle of the 2nd century BC onwards, the pressure of the East Asian Hunnic Empire became increasingly felt in the Iranian region. Around 140 BC, some Sakas tribes invaded the Parthian Empire, but the Parthian king Mithridates I defeated them and - as we have already mentioned - gave them a place to settle in what later became Sakastan. Then, around 130 BC, the Asi, Pasiani, Tocharians and Sakauraka peoples invaded Sogdia and Bactria. Strabo also mentions the Sabarol people. The pressure of this nomadic wave was extremely strong. Within five years, two Parthian great kings fell in the battles against them; Phraates II in 129 and Artaban II in 124 (Altheim: Weltgeschichte Asiens... etc.)a

290

===== PAGE 287 =====

We have already mentioned that the folk names are in very close phonetic parallel with the names "üz" ~ and "as" (osset) =, the names "Besenyý" and "Sabir". Let us now take the names of the tribes "Sparnoi" and "Aspasiak" mentioned about a hundred years earlier, which again seem to be only distortions of the names "Sabir" and "üz-Besenyý" ("as-pasiak" — "üz-pacinak"). ý

Here, the surprising Old Persian and Neo-Assyrian, and even earlier Transcaucasian-South Caspian, ethnonym parallels, which we will describe in our next volume, are connected to this remarkably close ethno-onomastic ensemble. We show there that in Transcaucasia and Northwestern Iran, from the middle of the 2nd millennium BC onwards, the continuous occurrence of the folk names "mita", "mata", "mada", "matieni", "machar", "makron" and the folk names "subar", "subir", "sapir", "sapardai", "saspeir" can be continuously recorded, up to the Parthian times now discussed, and even further. These serially occurring folk name connections can in no way be considered a game of chance. It seems that there is much more here, a certain degree of historical-ethnic continuity. — These questions require further, very thorough investigations in any case.

Returning to our above line of thought, in addition to the ethnonyms "Sparnoi" and "Aspasiak", in the 3rd century BC the original name of the Parthians, "Aparnoi" or "Parnoi", also appears, which is difficult to separate from the later ethnonyms "Apar", "Abar", "Var". This similarity of names is also confirmed - to a very large extent - by Theophylactos' statement that "Avar" or "Avars" are approaching them, who were renowned as invincible warriors throughout Asia. This is because the Onogurs and Sabirs undoubtedly still vivid in the memory of the peoples of the Middle East, South-East Europe - the Kuban - frightened when they heard that this could only mean the former Parthians, whose half-a-millennium rule in pre- and Central Asia was Caucasus region at that time.

Going further, according to the unanimous opinion of the research, the peoples who occupied Bactria around 130-128 BC later formed the Kushan Empire in the 2nd-3rd century AD, when the Parthian power had already weakened, the obvious Hungarian connections of which were pointed out by Haussig in his presented dissertation. It is very important to mention Markwart's statement here that the Persians called the Kushans Turks (Wehrot und Arang).

In addition to the above-mentioned traces of the Pechenegs (Uz), we also have a very remarkable piece of relevant data from the 1st century BC. Altheim-Stiehl state in their book "Geschichte Mittelasiens im Altertum" that Crassus's successor, the Parthian "Suren", was the governor of Szakaszta. Now, the Pechenegs ("Pasiani", "Pasiak"? also had a tribe called "Sur" (according to Constantine the Great), and on the other hand, the one, 291

===== PAGE 288 =====

The core of the Kushan Empire — whose people included the "Pasian" tribes — was precisely in the region of Bactria-Sakastan. The much-debated question of whether the Parthian "Suren" or "Surena" was a personal name or a title is irrelevant in the present context. However, it is a fact that the Suren clan was among the most powerful oligarchs throughout the existence of the Parthian Empire — Suren, who defeated Crassus, for example, fielded 10,000 horsemen and belonged to the leading aristocracy in Sassanid Persia (Altheim-Stiehl: id. m.). —, it even survived the fall of the Arsacids, and

But the name of the "Kang-kü" empire around Turkmenistan, mentioned several times in Chinese sources, also belongs to this category, which, in the light of the parallels known so far, can very easily be connected with the old "kangar" name of the leading tribal group of the Pechenegs. This "Kang-kü" was located on the northern border of the later Kushan Empire in the 2nd/1st century BC, roughly around Bokhara, Samarkand and Tashkent (de Groot: Chinesische Urkunden zur Geschichte Asiens). That is, precisely in the area in which the Asi, Pasiani, Tocharian and Sabarol peoples lived until around 30 BC (according to Altheim, until around 70 BC; 1. Altheim-Stiehl: id. m.), when they defeated the Sakaur and moved further south. -

These Turkmen and eastern Iranian peoples, according to Chinese sources, were mostly Hunnic tribes. The Chinese repeatedly state that their clothing, customs, lifestyle, and appearance are similar to those of the Huns, but their languages are also very close to each other. Chang-Kien specifically emphasizes that they mutually understand each other's dialects (de Groot: id. m.; cf. 0. Eberhard id. m.).

It should also be mentioned that we know from coins from the 1st century BC that two royal names from the Indian "Sakaura" dynasty are known. These are: 1. TURANNONTOS 'ERAOY; reverse: SAKA KOGGANOY

(on some coins: SAKA KOYYANOY)

2: YRKODOY MAKAROY; back: SK WR (i.e. "sakaur")

The early occurrence of the title "kagan" is very important here, but we can also consider the word "makar" to be equally noteworthy, especially because it appears together with the phonetic form "sakaur", i.e. the singular version of "sakaur": while the name of the tribe - similar to the name "royal Scythians" - sounded like "sakaur" (plural), the title of a certain king was "sakaur" (singular).

As the Huns moved west, they took over the leadership of the steppe and northeastern Iranian "Altaic" peoples after the Parthian Empire was overthrown by the Persian Sassanids in 226 AD. However, these Persian rulers were only able to temporarily extend their power to the northern and eastern fringes of Iran, because by 310 AD the Huns already ruled Turkmenistan, and around 350 they conquered Bactria and the other eastern Iranian-Afghanistan regions.

===== PAGE 289 =====

They also occupied the province of Nistani, and from that time until 550 AD - as we have already mentioned - nothing could happen in Iran against their will (Altheim: Das Alt: Iran).

The question of the ethnicity of the Nevolino culture and the late Avar "Erffic-indás" style parallels in the Volga region

Let us now return to our starting point, the question of Lomovato culture. The migration of the Harina group of the Lomovato ethnic group (or ethnic groups) to the north can therefore be examined — following from the historical circumstances we have learned about above — only within the framework of the Hun supremacy in the Eastern European steppe that was reaching its peak at that time, and in their ethnic composition we should not look for the Ugrians in the false sense of the word today, but for one of the ethnic groups that made up the Hun Empire, or perhaps the real Ugrians of the contemporary sources, the Onogurs or Hunugurs. All the more so because Bartha himself states that "the appearance of the majority of Iranian Ezid preparations occurred simultaneously with their appearance", and these Lomovato tribes maintained almost regular contacts with the southern areas". We can therefore safely say that they maintained "regular relations" with the Hun Empire. However, Bartha carefully avoids this. But he also keeps silent about the fact that the Late Avar period was found in the cemetery of Nyevolino, around 670 BC, of the Lomovato culture.

"with southern territories" is the correct Hungarian translation. ý

even the mention of the word "hun". That is, it consciously obscures and disrupts the obvious historical contexts of the time.

the closest parallels of the "griffes-indás" style, of which Gyula László is the

"Griffes-indás" suggests the K4ma-Middle Volga origin of a group of settlers, and from this, their Finno-Ugric ethnicity.

In this regard, we raise the very plausible possibility that the Late Avar, Griffes-indás parallels of the Nevolino cemetery and the Volga bend region are not the legacy of the autochthonous peoples there, but only reached them in the 6th century AD, with the southern, Caucasus-surrounding ethnic groups of the Nevolino cultural horizon that was emerging at that time (Onogurs? Hungurs? Saragurs? Sabirs? Hungarians?). Regarding the origin of the style, we recall the Parthian animal style, about which Altheim writes that some of its pieces are deceptively similar to the later Avar style (Altheim-Stiehl: id. m. V. 0. a Partusok Aparnoi és az Avarok Apar nevevel 15).

This question would be worth examining in detail, because we might be able to find one or two clues to resolve the seemingly insurmountable contradiction that exists in the 293

"Ugric" peoples - closest language-

===== PAGE 290 =====

Our relatives - and the social, cultural and anthropological picture of the conquering Hungarians - lie between us, and which our historical science, after a whole century of painstaking work, has not been able to satisfactorily explain to this day.

Namely: the presence of "Ugric" (Onogur, Hunugur) and Sabir (Hungarian) ethnic groups in the middle Volga and Karna regions in the middle of the 1st millennium AD — if this can be proven, or at least highly probable — could provide an obvious and reasonable explanation not only for the close Hungarian-Vogul-Ostiak language parallels, but also for all those folk tales, legends, and mythological images living in the folk memory of the Voguls and Ostiaks that have Hungarian equivalents.

Linguistic taboos in our prehistory research

Bartha, on page 142 of his book, discussing the significant differences between the Bulgarian-Turkish and the Hungarian archaeological finds of the time of the conquest, states: "From the fact that the facts of linguistics and archaeology do not overlap in the given case, we must conclude that the Bulgarian-Turkish loanwords of our language and the components of our culture of the time of the conquest that can be extracted from archaeological finds shed light on different aspects of the effects on the Hungarians."

We have already discussed this issue in detail in connection with our alleged Bulgarian-Turkish loanwords. Therefore, we would like to point out that it takes its revenge on historical science if unproven linguistic hypotheses are treated as facts. In such cases, there is nothing left but to state with resignation that indeed "the facts of linguistics and archaeology do not coincide in the given case..."

There would be no problem with scientific methodology so far, because it is only a very short way from this statement to the recognition that consequently something is wrong with the two facts. And since the findings of archaeology are objective, tangible facts in the strictest sense of the word, we can only draw the conclusion that according to them the so-called "facts" of linguistics can hardly be called facts. In our humble opinion, a historian thinking with scientific methodology could reach this point by simple deduction, even without knowledge of Benzing's article cited earlier.

We cannot assume that Bartha would be beyond his intellectual capacity to draw the above reasonable conclusion. In this case, too, it is simply a question of the fact that questioning the 19th-century dogmas of Finno-Ugric linguistics seems to be considered sacrilege in our historical science, even if the material findings of archaeology directly contradict it.

294
===== PAGE 291 =====

They also argue with these linguistic theses. Therefore, in the second half of the sentence quoted above, Bartha is forced to resort to a banal phrase that explains nothing in the world.

However, we consider the most strange phenomenon to be that it is precisely these Finno-Ugric linguists, who thunder anathemas against the concrete facts of archaeology, who most often mention the concepts of "scientificity" and "methodology". Although in the end they are right from their own point of view: the most effective defense is the attack. =
Once again about the "Sassanid-irdni" style, in its Caucasian aspect
"During their continuous campaigns in the 7th century, the Khazars encountered a world of tastes in the Caucasus that grew out of Sassanid-Iranian forms," writes Bartha on page 143.

We have already dealt in more detail with the issues and antecedents of so-called Sassanid art in the appropriate place.*
However, we cannot ignore two very important objections regarding the narrower scope of Bartha's above statement.
First: from the study presented by Haussig, but also from several other data (e.g. Altheim: Geschichte der Hunnen Vol. I), we could see that the Khazars re-acquainted themselves with what Bartha called "Sassanid-Iranian" Caucasian campaigns in the 7th century. With this statement, Bartha once They moved from the "world of taste" to the Caspian-Volga region, and consequently they had no need to leave this "world of taste"
again hit the mark when discussing the agricultural knowledge of the Hungarians. hits himself in the head with a "cultural history boomerang", which is

Our second objection is that, as we have refers to the superficial use of the term "Sassanid-Iranian" in a context where the Caucasus regions are specifically mentioned. As seen above, the technical term "Sassanid-Iranian" is not even a precise terminology in a general sense — Bartha himself mentions this when discussing the Sassanid style — and it is therefore even less applicable specifically to the Caucasus region, since the Caucasus region was — according to our current archaeological knowledge — one of the most ancient foci of the metal art motif ensemble, the late developments of which in the 3rd-9th centuries AD are called "Sassanid-Iranian" or "Late Sassanid" called it — we might add: very hastily, not yet knowing the older and more distant connections — the 19th century art historical research. These metalworking traditions

* See pages 86-89 and 258-259. y | 295
===== PAGE 292 =====

They have been archaeologically detected and continuously traced in the Caucasus for thousands of years; even in ancient times when "Iran" was not even known.
This brings us to note 86 on page 144, in which Bartha himself writes that "the metalworkers of the Dagestan Kubachi-aul used Sassanid-Caucasian decorative elements almost until recent times." So we can also refer to Bartha when we claim the existence of ancient and tenacious metalworking traditions in the Caucasus region. We would only like to add to the above quote that those used, for the most part, can be found in the finds of Uralian excavations in these Caucasian-Transcaucasian areas as early as the 9th-8th centuries BC. "Sassanid-Caucasian decorative elements", which the metalworkers of the Kubachi aul have been using even recently

We considered it necessary to briefly present the actual facts, lest Bartha's "Sassanid-Caucasian" terminology support false cultural and art historical ideas in the unsuspecting reader.

About Georgian church jewelry
In the same note no. 86, Bartha draws attention to a circumstance that is particularly significant for us: "The motifs of early Georgian church jewelry (between 996 and 1010) are dominated by palmette ornaments. It is striking that the palmette ornaments of Georgian church jewelry show more affinity with the artistic traditions of the steppes, and especially with the Hungarian, than with the — territorially expected — Caucasian form."

Bartha evaluated this truly striking fact on page 143 as follows: "The surprising similarity of its Georgian Christian religious objects made at the turn of the 10th and 11th centuries are the result of our metalwork from the time of the conquest. decorative elements also proves that our art during the conquest period received its inspiration directly from a Caucasian-Iranian source, without any mediation."
These two quoted sentences are all that Bartha writes about this issue of eminent importance.
Bartha's above statements, although in a veiled form — we could say: in scientific jargon — point in exactly the same direction to which we ourselves have tried to draw attention in our previous comments — in less scientific, but hopefully more generally understandable language. We do not therefore wish to criticize Bartha's opinion this time. We only note that if he devoted almost ten pages of his book to a detailed description of the most diverse archaeological finds in the Volga-Kama-Ural-Perm region, to describe (pp. 132-141), to discuss all their possible or supposed connections, and then to state as a final conclusion; in these areas no finds related to the art of the conquering Hungarians are to be found anywhere; – then perhaps it would not have been useless
296
===== PAGE 293 =====
It would be a waste of time if one devoted a little more space and energy to discussing the striking and surprising Hungarian origins of Georgian church jewels, and to exploring and presenting their possible further connections, than the few lines of superficial references cited above.
Far be it from us to say anything against the detailed archaeological excursion to the Volga-Ural region. We know very well that a thorough examination of the aforementioned areas is very important from the point of view of the Finno-Ugric concept. However, we can also safely say that it is no less instructive from the point of view of the questions we have raised, because we can learn from it that in these northern regions there is hardly any path that could lead us to the sources of the unresolved questions of Hungarian prehistory in the 8th-9th centuries. So we do not object to the length of the 9-10 pages dealing with the Volga-Ural region, but to the fact that where there are undoubtedly connections and parallels, Bartha suddenly becomes remarkably terse, avoiding any closer discussion.

Unfortunately, Georgia lies much further south than the 50th degree of latitude north, the demarcation line established by 19th-century Indo-Germanic studies for Hungarian prehistory research, and so Bartha hardly dares to step into the forbidden territory. Yet, if nothing else, he could at least have mentioned the undeniable Armenian-Georgian parallels of our early Árpád-era churches. In any case, we can conclude from this Georgian episode that it is not our approach that should fear any detailed, complex prehistory investigation, but —, if you were talking about Christian church jewelry anyway, rather that the proponents of the thesis of exclusive Finno-Ugric origin turn away from any data and research areas that are not compatible with their prefabricated, 19th-century dogmas.

We calmly accept the fact of the Hungarian-Finno-Ugric language relationship, and we await with particular interest every new excavation result in the Ural region, and even a few Georgian pilgrimage crosses will give the representatives of the Finno-Ugric school goosebumps.
We could raise the question: which behavior is more in line with the requirements of methodical science and which is more in line with irrationalism and the pursuit of mirages?
Again about the "Pechenoy-run"
On pages 160-161, Bartha states; "We can refer to the powerful Pecheneg tribal confederation..., but the Hungarian society, which is much weaker militarily..."
It's getting boring, but we still have to ask Bartha the stereotypical question again; what does he prove with this statement? That circumstance
297
===== PAGE 294 =====
The fact that this opinion prevailed for a long time in recent Hungarian history is by no means proof in itself.
We have already pointed out that several people have recently strongly questioned this assumption, based on very compelling arguments. However, Bartha is apparently not bothered in the least by this: he simply "states"; he probably considers himself a sufficiently renowned historian to be able to state unproven things in a declarative manner, without reference.

This habit has spread in our historical science - unfortunately - almost beyond eradication. Academic membership or a university chair is more than enough legitimization for someone to make unsubstantiated categorical statements in scientific work. Moreover, such unfounded statements - having been trampled by the eager citations of students - often appear after a few years as indisputable prehistoric facts, on which further similar hypotheses can be built.

With this question we have once again come to the well-known methods of 19th century Indo-Germanic studies. This procedure, which is based solely on authority and is otherwise mostly poorly supported and cannot be called scientific, also escaped to us from the depths of Indo-Germanic studies, together with several similar phenomena. We have already established countless times that the conclusions of 19th century Indo-Germanic historiography were largely based on this kind of "evidence".
We hope that this has shed some light on why we consider a detailed critique of Indo-Germanic historiography to be fundamentally important and essential.
Bartha's a and to the reference to the "huge Pecheneg tribal alliance and the much weaker Hungarian society in terms of military power", we can only add that Bartha himself puts the number of the conquering Hungarians at 400-450,000 (p. 149). The Pechenegs, who were much stronger than these, should therefore have numbered at least 600-650,000. This quantity would have represented 6-6.5% of the population of Europe of about 10 million at the end of the 9th century, which corresponds to a population of about 38-41 million in today's terms. Is it conceivable that this mass of people would simply disappear after a few generations? We would not believe it. We can therefore consider it much more likely that the situation was the opposite; the Pechenegs must have been much fewer than the Hungarians.

Anyway, the dry facts of history ruthlessly expose the perversity and unhistoricity of Bartha's views: "the mighty Pecheneg tribal alliance" disintegrated without a trace after a century, while the "much weaker Hungarian society" was a first-rate great power in Central Europe for more than 600 years. What is the point of this greedy effort, which wants to devalue the historical role and significance of the Hungarians at all costs, in direct contradiction to the obvious facts? What kind of view of history is this?

===== PAGE 295 =====

On the "bourgeois positivist" view of history

On pages 162-168, Bartha briefly summarizes the development and main trends of Hungarian historiography in the 19th and 20th centuries.

We cannot leave without mentioning his statement that in the era of dualism, "the bourgeois positivist conception of our early history, based on diligent and respectable collection work, was not simply pushed out of scientific life, but the criticism and even the attempt at the concept of the millennial nation was ruthlessly suppressed by blindness." (p. 164)

What is this supposed to mean? Maybe Bartha doesn't even believe that Hungary and the Hungarian nation existed in the Carpathian Basin for 1100 years? Or is his weak side showing again, which we could have noticed earlier: he cannot formulate things precisely. This latter possibility may be possible, because we don't want to assume the first one.

As for the other aspect of the quoted sentence, we confess that it is simply incomprehensible to us. Let us examine more closely what Bartha wants us to believe. Obviously something like the fact that the representatives of the Hungarian prehistory and linguistics trend he calls "bourgeois positivist", i.e. Pál Hunfalvy, József Budenz and their school, their statements, articles, books, and thus their scientific work in general, were "mercilessly suppressed" by the government of the time or the ruling class or someone else.

However, not only do we not have such data, but on the contrary, from almost all scientific journals and publications of the time, but also from the lists of university professors and academic members, it can be unequivocally established that it was precisely this school that dominated our scientific life in the field of historical and linguistic research, to such an extent that representatives of other scientific opinions had hardly any opportunity to communicate. And what they were still able to communicate was either the "bourgeois positivist" school, or, due to its dominant position in the editorial offices of scientific journals, it ensured, or simply ignored, that the appropriate, derogatory criticisms could appear in the same issue in which the study in question was published. This is evident at every turn when leafing through the pages of our scientific journals.

"method" is older

In light of these irrefutable data, we cannot make sense of Bartha's attempt to portray this largely arrogant group, which considers itself almost infallible, in the role of persecuted martyr saints, by reversing the real situation.

If this approach still prevails in Hungarian historical research today, then they will never reach the point of seeing the need for a critique of 19th-century Indo-Germanic studies. It is precisely because of this

299

===== PAGE 296 =====

The Hungarian representatives of the "civil positivist" historical school - whom Bartha obviously considers sacred and inviolable - were those who, in the strict sense of the word, bowed down before the Indo-Germanic studies of their time, uncritically accepted all its so-called "scientific results" - achieved with the help of "methods" that were already well-known by now - and, without expressing an independent opinion, adapted their own historical and linguistic research to the Indo-Germanic scientific approach.

In this way, the Székely-Hungarian Hunnic traditions in Hunfalvy simply became a takeover of the Nibelungenlied (first written down around 1200, and in the Austrian region that belonged to the former Avar empire, and therefore had Avar traditions!), but this is how several thousand words of our language also became, without any more thorough examination, almost a priori Slavic, German, Turkish and other loanwords. This is how the "sly legend of the Hungarians destroyed by the Pechenegs" arose (Padányi: Dentumagytaria) and how the conquering Hungarians became primitive Asian hordes. But this is how, for example, the country of Svatopluk expanded to the Szerémség.

We could continue this list for a very long time, about the similar achievements of the "ruthlessly suppressed" bourgeois positivist historical science - as we can see, it was built on the very diligent and respectable work of collecting - the inadequacies of which we have to correct with laborious work in most cases today. A detailed criticism of the "bourgeois positivist" school cannot be done in a few lines.

However, we do not have space for this in this framework.

For now, we can only refer to the numerous errors and contradictions that have already been pointed out, all of which can be traced back to the diligent and respectable compilation of this bourgeois positivist view of history, which we could not find if we searched for anything more one-sided.

As for who suppressed the 1907 edition of "Centuries" at the time, let us head up to the equally respectable Bleyer lines there against Ja- kab Károly Fiók, because the latter committed the boundless audacity of disagreeing with Bleyer in several places in his work "The Germanic Elements of the Hungarian Hun-Monda". Let us especially observe what a high level of "bourgeois positivist" scientific education Bleyer demonstrates when, despite Fiók's criticism and clear proof, he continues to insist that there is no "a" + "e" sound transition in the Hungarian language.

Every elementary school child should know that, for example, the ethnic name "Hungarian" used to have the suffix "megyer" and even "mogyer", but the Bleyer who is not even aware of this is simply-300

===== PAGE 297 =====

He ridicules Fiók, precisely because in his criticism Fiók suggested — and methodically proved! — that there is indeed a "transition" to "e" in Hungarian.

So there was room for publishing such tangible nonsense in "Századok", if these came to the editorial board from "bourgeois positivist" ranks. Moreover, the editors of the editorial board did not even bother with Bleyer's brazen tone-deafness, despite the fact that Bleyer was undoubtedly completely wrong when he asserted the impossibility of the "a" + "e" sound transition in the Hungarian language.

For the sake of completeness, we should also note that in the work in question, Bleyer tried to prove the above-mentioned views of the Hunfalvy school about the Nibelungen origin of the Hungarian Hunnic myth. Only in this way will the deeper connections be clearly recognizable: that is why he had to get there. In the columns of "20th Century", Bleyer's subcortical argument was replaced by

Summary

In summary, we can conclude that almost the only benefit of Bartha's book lies in the provision of a wealth of new Soviet archaeological data. This is undoubtedly of the highest order.

important, especially for those who do not know Russian.

However, as far as the topic given in the title, the outline of Hungarian society in the 9th and 10th centuries, is concerned, Bartha says almost nothing new here, mostly discussing things that have long been well-known, and only becomes truly informative where he can show off his impressive archaeological knowledge.

This could be the end of our brief overview, if we did not consider it absolutely necessary to say a few words about Bartha's view of history. This view runs like a red thread through the entire book, is clearly expressed almost everywhere, but unfortunately cannot be called objective. Even the unbiased reader must have noticed that we must constantly ask Bartha the question: on what does he base this or that statement or assertion, what does he prove?

Amazingly, in all cases of such questions, we found our author trying to present the Hungarians at the time of the conquest, their level of cultural or social development, or their historical role, in the most unfavorable light possible by presenting uncertain assumptions in a factual manner. On the other hand, in all cases where archaeological finds or other circumstances almost unequivocally indicated that we should attribute a much more significant role to the Hungarians in the cultural and historical context of 9th-10th century Eastern and Central Europe than the academic Hungarian historiography ed-301

===== PAGE 298 =====

It was assumed, well, in such cases Bartha either simply gave up drawing the obvious conclusions or avoided expressing his opinion with meaningless phrases.

In contrast to this consistently applied, explicitly negative tendency from a Hungarian perspective, Bartha seized every opportunity where he could highlight foreign peoples in one way or another, and enhance their significance -- very often even by obviously forced misinterpretation of the historical data, sources, and archaeological finds available to us. Let us think of the "vast Pecheneg tribal confederation" near Lebegyin.

to a "Slavic" settlement, or to

We are convinced that the Academy's history department did not make the most fortunate choice when it chose a person with such a historical perspective.

entrusted the compilation of the prehistoric chapters of the Hungarian historical synthesis that was being prepared to a historian.

for the day

===== PAGE 299 =====

Chapter 3

SCIENCE AND UNSCIENCE IN OUR PREHISTORIC RESEARCH

In the light of István Fodor's book "Verecke's famous journey..."

On pages 33 and 34 of his work, Fodor considers it necessary to thoroughly shake off the dust from the representatives of various Hungarian prehistory views that were not based on the exclusive Finno-Ugric origin theory, which Zsirai summarily called "prehistoric miracle bugs", including the adherents of the school of thought that claims the Sumerian affinity of the Hungarian language.

He states that all these views - in full agreement with Zsirai, are grouped together - that their ideas are just as . He calls them "distorted theories". He also discusses the researchers of the Sumerian-Hungarian linguistic connections separately, confused and unscientific as those of their predecessors, and that their astonishing "discoveries" can hardly evoke anything other than good-natured cheerfulness. " (p. 34)

We confess that after reading these confident lines, we studied the book with excited interest, thinking that we would certainly not find "distorted theories" written with a truly scientific methodology, or "confused and unscientific ideas", but that we would only find crystal-clear professional work in which we could delight in logic, proven facts, or at least in thoroughly, from all sides, highly probable conclusions.

Our opinion was confirmed not only by the aforementioned words of appreciation from András Róna-Tas, but also by Fodor himself, who had previously assured his readers in the introductory pages that "to dress them in the robe of solutions" (p. 28) We do not try to conceal difficulties anywhere, or to present our proposed assumptions with more or less probability in the final

After these methodologically very important statements, let's take a closer look at how much and to what extent Fodor applies these methodological principles he himself has established,

We could have noticed. It is obvious that Fodor wants to hammer this unproven Finno-Ugric basic thesis into his readers' heads at all costs; that is why he repeats it so often. Well, let us follow him in this method and state, for the third time, but even more unequivocally: the mere fact of the kinship of two or more languages does not in any way make it beyond doubt that the languages in question derive from a common ancestral language. A does hundred years ago, this was the basic thesis of Indo-Germanic linguistics, but since then the Indo-Germanists themselves have refuted this thesis. It is most regrettable that Fodor seems not to have become aware of this.

The "Ural" homeland and paleobotanical research
But let's move on. On page 39 we find the following lines: .
We know mainly from the research of Péter Hajdú that in the old Uralic and Finno-Ugric base languages we find without exception the names of the trees of the taiga. (The most important of these are »spruce«, »the »fir« and »the »elm« and »the »red pine«.) However, only one of the trees of the Central European mixed deciduous forest, the »elm«, has a Finno-Ugric name. The ancestors of the Uralic peoples, therefore, in the period immediately preceding their separation, lived in the western edge of the Siberian taiga, where the easternmost extension of the European deciduous forest reached, i.e. at the point of contact of the two types of forest. According to the results of pollen studies, this meeting point can be placed in the northern part of the Ural Mountains, roughly in the source region of the Pechora River, the two forest zones met in this region around the 6th millennium BC. Thus, based on biogeographical data, it is fully justified to suggest that Hajdú
Peter concludes that the ancestral homeland of the Uralic peoples in the 6th-4th millennium BC was in Western Siberia, approximately in the area between the lower reaches of the Ob and the Ural Mountains."

Postponing our own comments for later, let us first compare Fodor's argument with the studies of Gyula László on the same issue (Earliest Sections of Our Prehistoric History, 1961 and 1971),

* (See more details in the next book.)
308
===== PAGE 305 =====
Fodor", in the old Uralic and Finno-Ugric basic languages we find without exception the names of the trees of the taiga. (The most important of these are »luceny6«, »cirbolayafeny6«, »jegenfeny6« and »red fir«.)"

László (id. m. pp. 66-67): "The trees and names of the taiga forest in the Ob-Ugric - Samoyedic group.
Larix (larch): Manyasi, Khanty, Komi.
Pinus cembra (stone pine): Mansi, Khanty, Samoyed,
Abies pichta (fir): in Udm., Komi, Manyasi, Khanty, Samoyed and Mordvinian it means forest.
The names of the trees of the taiga forest are therefore either absent from Finnish, Lapp and Volga languages, or only with a change in meaning (forest), while the names of taiga trees are completely absent from Hungarian."

Fodor: "Of the trees in the Central European mixed deciduous forest, only one, the 'elm', has a Finno-Ugric name."
László (id. m. p. 66): "Ulmus Tree species of Central European mixed forests and their Finnish-Volga-Permian-Hungarian names.
(elm) varieties: f., mordv., mari, udm. and in Hungarian.
Of these tree names, with the exception of Alnus (birch), not a single one is known, even in its derivatives, in the Ob-Ugric — Samoyedic languages, and only one (Pinus silvestris) in Lappish.

Fodor: "The ancestors of the Uralic peoples, therefore, lived in the western fringe of the Siberian taiga, just before their separation, into which the European deciduous forest extended.
reached its easternmost extent, i.e. at the point of contact between the two types of forests. According to the results of pollen analyses, this point of contact can be placed in the northern part of the Ural Mountains, roughly in the area of the source of the Pechora River, the two forest zones met in this region around the 1st.6th millennium BC."
László (id. m. p. 64—65): Fortunately, the Moscow Institute of Botany has summarized its half a thousand pollen studies in two maps that divide the pollen zones into two eras: the Paleo-Holocene and the Middle and New Holocene... These two maps (Figures 11—12) are discussed below, divided by plant species, according to the two eras...

Abies picha. Unknown in Europe in the Early Holocene, it just reached the Urals from the east in the Middle Holocene...
Larix. In the Early Holocene it spreads through the Urals to the Kama Elbow, in the Middle Holocene it retreats to the Tobol (as we saw with the pollen of the 6th section of Gorbunovo, it appears again in the Urals around 1700, so its western spread must have been much later than this)...
Pinus cembra v. 5101108. Unknown in Europe in the Early Holocene, as we saw in the Gorbunovo peat, it reaches the Urals around 1000 BC and reaches the Ob line in the north...
309
===== PAGE 306 =====
Quercus-Ulmus-Tilia (QUT). It does not yet appear in the Early Holocene, and in the Middle Holocene it is found only west of the Kama. It is a typical European forest. We have seen that it also appeared temporarily (between 2000-1000) in a small percentage in the Urals."
From Gyula László's list, we only cited the trees also mentioned by Fodor, because for our comparison purposes, only these are of significance. ;

László Gy. then explains that, according to our linguists, the reason why the names of taiga trees are missing among the Western Finno-Ugric peoples, and the names of deciduous forest trees among the Ob-Ugric peoples and Samoyeds, is that during their migrations, the individual ethnic groups forgot the names of trees that were not found in their new places of residence. Based on this assumption, they also searched for their ancestral homeland in the areas where the two forest types came into contact.
But let's give the floor to Gyula László again (ibid., p. 65):
"...Quercus-Ulmus-Tilia forests in ancient times did not come into contact with the Pinus cembra-Larix and Abies taiga at any point, and in fact, they were separated by thousands of kilometers, and thus there could not have been common names for these species in the assumed ancestral language, neither if the Urals had lived in the east, nor if they had lived in the west."
Fodor: "Thus, based on biogeographic data, Péter Hajdú's conclusion that the ancestral homeland of the Uralic peoples in the 6th-4th millennium BC was in Western Siberia, approximately in the area between the lower reaches of the Ob and the Ural Mountains, is completely justified."
László (id. m. pp. 65-66; direct continuation of the previous quote): "As we mentioned, there is no area where these two types of forest would have come into contact with each other in the time of the supposed proto-language. It seems, therefore, that we cannot use the argument of the extinction or forgetting of the word, at least at this stage. Although there are plenty of examples of the extinction of words, there are also the opposite, namely that the existing word does not die out, but its meaning changes with the change of the environment, or perhaps for cultural-historical reasons it narrows down to some nuance of the former meaning, but remains (e.g. the name of a tree as forest or twig, or bark, or shrub remains in other languages, we know plenty of such examples from Finno-Ugric languages). If there were at least one such change in meaning of the trees of the taiga forest in the western Finno-Ugric languages, or at least one similar remnant of the trees of the Central European deciduous forests in the eastern ones, then we could still assume that most of the names had been forgotten and lost. But such a remnant is not found in either group (not counting the transfer of the name Abies to Mordvin). Based on this, our assumption that these words originated during the separate life of the languages seems much more justified than that they had been lost. The suggestion of this possibility, which is outlined below, is

===== PAGE 307 =====
"The detailed description of this is very significant for my further thought process, because it contains the following: the Urals did not develop in their current territory, which was once covered with ice, but not even in one or another spot of this territory (e.g. the Kama region), so they did not migrate from there (at least in large units), but they settled in their current territory in a very ancient time (the Uralic period) from somewhere else, but in any case from pine-covered areas."
Going point by point, detail by detail, we can therefore establish that the views of Fodor — or rather Péter Hajdú — and Gyula László are diametrically opposed, and even outright mutually exclusive.

This fact does not change even if we take into account the discussion between Péter Hajdú and Gyula László (Archaeol. Ért. 1964). Hajdú objected that Gyula László had forgotten that the deciduous forest reached the Urals in a narrow strip in the north around 5000 BC.
In his response (On the research of the Uralic homeland), Gy László states that in the Paleolithic and Transitional Stone Age — that is, in the assumed age of the Uralic language unit — according to the clear findings of Soviet archaeology, no people lived in either Eastern Europe or Western Asia north of the line of the west-east flow of the Volga.
The aforementioned stretch of deciduous forest, however, extended far north of this line to the Urals. And where there are no people, there can be no ancestral homeland, notes Gyula László.
Not even if in these northern regions the deciduous forests partly extended to the Urals. In the words of Gyula László: "Unfortunately, in the area that Péter Hajdú designates, there is no trace of human life in the period he mentions. Archaeologically, therefore, the linguistic conclusion has again come to a dead end! I could not continue the discussion here, because it would be difficult to discuss what does not exist."

We would like to add that from a biological point of view it is practically impossible that the deciduous forest reached the Urals earlier in the north than in the south. Therefore, it is almost certain that some error in the method of the pollen tests in question occurred.
Our first thought, if we should not be suspicious of Fodor from the outset — due to his one-sided approach, which we have repeatedly experienced above — could be that the László
The results of the more recent Soviet pollen studies, which differ significantly from the paleobotanical picture analyzed by Gyula, and even outright refute it, led Fodor to accept Hajdú's assumption of the Uralic homeland in northwestern Siberia. However, upon closer examination of the issue, it soon becomes apparent that while Gyula László gives his source precisely, the results of pollen studies of the Moscow Botanical Institute summarized by M. 1. Neustadt in 1957, Fodor only cites a few Soviet authors.
lists his name in his notebook - munká-311

===== PAGE 308 =====
without their titles -- and Neustadt's name is missing from these. Since in our given situation we cannot rule out the possibility that among the named Soviet researchers there may be an author of another work on pollen analysis, we refrain from expressing a definitive opinion, but we reserve the right to verify. ;

All we can say now is that we find Fodor's repeated mention of Péter Hajdú's 1962 work (Finno-Ugric Peoples and Languages) highly suspicious, because he seems to be drawing his arguments from this work, and not from some new Soviet paleobotanical publication. But it is also important in this regard that

that although we used the 2nd edition of Gy. László's cited book from 1971, we found no indication that the pollen test results presented above had become outdated in the meantime.

And as for — regardless of all this — according to the statement (on page 40 of his book) between the lower reaches of the "Ural" homeland in the 6th millennium BC, while, also according to Fodor's statement, Ob and the Ural Mountains, the Kama region, which lies much further south, was only populated in the 3rd millennium, Fodor, let us express our very strong doubts.

Not even with a scientific reference for now (we will come to this later), but purely in the name of common sense, we would like to ask Fodor whether he really seriously believes that at that time — we are talking about 6000 BC! — when there was still plenty of uninhabited space almost everywhere on the globe, a group of people would settle precisely in the Arctic Circle region, literally on the ice, and only after thousands of years would they migrate to regions further south, with a more favorable and pleasant climate (Kama)?

However, let us not dwell on such considerations, where a final judgment cannot be made in purely linguistic and historical terms, but we should extend our investigations to the fields of geology and paleoclimatology. In the future, we will have another opportunity to say something about this question.

In connection with the Hajdú (Fodor)-László contrast described above, we must also recall the recent findings of Indo-Germanic linguistics and prehistory research, according to which the so-called "linguistic paleontology" is a fundamentally flawed method. The correctness of this statement is demonstrated more clearly than ever by the previously presented, mutually contradictory conclusions of Hajdú (Fodor) and Gyula László.

However, there is a very important difference between the working methods of the two researchers. Whereas Péter Hajdú - and Fodor, following in the footsteps of 6 - rejected the 19th-century proto-language hypothesis of Finno-Ugric (Uralic) linguistics, the supposed Finno-Ugric

*(See more details in the next book.) 312

===== PAGE 309 =====

treats the inferred phantom words of the (Uralic) basic language as given facts, and completely ignoring archaeological research, he forcibly adapts the concrete results of paleobotanical studies to these linguistic concepts with stilted reasoning (e.g., the assumption that tree names are "forgetful"), while László Gy. uses the data of pollen studies and the latest results of archaeology to correct the purely linguistic assumptions of Finno-Ugric studies with the help of the findings of paleobotanics and archaeology, and to place them on a basis more appropriate to the real situation of the past,

There can be little doubt as to which workflow can be called more methodical and complex.

The "Ural" primitive people hypothesis and anthropology

Further examining Fodor's book, on page 40 we again come across a very characteristic, exclusively Finno-Ugric apodictic statement: "He agrees (emphasis added) that the Neolithic (4th-3rd millennium BC) archaeological culture in the Ural region should be considered the legacy of the Ural peoples."

We readily allow that within the school of historical thought that Fodor represents, Fodor is tempted to generalize. In the discussion of the above-mentioned paleobotanical "everyone agrees", but that doesn't even remotely justify it questions, we have seen that, for example, Gyula László holds quite different views.

However, it is also conceivable that for the author of "one of the most outstanding works of the latter period of Hungarian prehistory research" (Róna-Tas's statement about Fodor's current book), Gyula László's investigations are not authoritative and thus he overlooks them.

In this case, we can warmly recommend to Fodor that he should check out the briefly "Handbuch der Orientalistik" published its relevant volume in 1966 (Jettmar et al.: Geschichte Mittelasiens), where summarized opinion of international prehistory research on this issue. And wonder of wonders, this scientific opinion does not agree with Fodor's ideas, and is even in direct contradiction to them. For we can read there that the Uralic human race most likely only developed in the second half of the 2nd millennium BC, through the mixing of Mongoloid, Paleosiberian and Paleo-European ethnic groups.

From this, however, we dare to conclude that the Uralic peoples, who were still in the process of formation in the 2nd millennium BC, could not have had any archaeologically tangible remains in the 4th or 3rd millennium BC.

313

===== PAGE 310 =====

But we can also refer to the results of Lipták Pál's anthropological studies in this regard, according to which the Uralic variety appeared anthropologically at the earliest in the Ananjin6 period, i.e. in the middle of the 1st millennium BC. However, Lipták also emphatically points out that the comb-pitted pottery previously attributed to the Finno-Ugric peoples is characterized by the predominance of the Lappish race. The Lapps are Paleo-Europeans, and the varietal characteristics of the Lappish race are still dominant in certain groups of Poles and Ukrainians (Lipták: Questions of Hungarian Prehistory Based on Anthropological Research).

So, in this Paleolithic-Ameneolithic Ural archaeological culture, defined by Fodor as dating from the 4th-3rd millennium BC, we see the remains of the scattered Paleo-European and Paleo-Siberian people groups who, in the 3rd millennium BC, fled to the north, northwest and northeast from the pressure of the Swede ethnic groups penetrating the forest belt: in the west the Lapps, and further east the ancestors of the Voguls, Ostyaks and Samoyeds. The so-called Finno-Ugric (Uralic) languages apparently developed from the complicated interactions of these people groups, as well as the Swedes and the southern, Caucasian-Transcaucasian colonial waves that appeared in the Volga-Ural region in the first half of the 2nd millennium BC. in the times after the turn of the 2nd millennium. However, in this paleolinguistic process, we must attribute a decisive, dominant role to the southern ethnic group or ethnic groups that arrived in large numbers, with advanced agricultural and animal husbandry skills and a bronze culture.

We would like to point out right now that the dating of the archaeological culture in question in the Ural region to the 4th-3rd millennium BC will most likely have to be drastically lowered.

(More details later, on the question of the chronology of the ancient seminary culture.)

Fodor's archaeological terminology

On page 42, he writes about this culture in the Urals region, which he consistently calls "Neolithic", as follows: "...at this time the population obtained its food exclusively through fishing and hunting..." Then on page 47: In contrast, international archaeology and prehistory have for over thirty years - especially since Gordon Childe's "Most of the objects we have left are made of stone... The tools were made by clicking." fundamental insights - used the term "Neolithic" exclusively to designate food-producing cultures, but never to fisher-hunter cultures.

For the sake of the weak - this expression is by no means intended for our readers - we must also note that the Neolithic is older, predating G. Childe-314

===== PAGE 311 =====

The name of the period was "polished stone", in contrast to the "Old Stone Age" or Paleolithic, which produced chipped stone tools, which was also called "chipped stone" at the time because of the way the stone tools were processed. Between these two periods lies the "Transitional Stone Age" (Mesolithic), which is characterized by the extremely fine, meticulous processing of chipped stone tools, as well as the strikingly small size of the tools, which is why the age is sometimes also called "microlithic",

We considered this brief digression necessary because Fodor clearly has rather immature views on the archaeological terms used to designate certain periods of the Stone Age.

Under such circumstances, the childlike innocence with which our author can so "tastefully" (our quote is from page 34 of Fodor's book) is "confused and unscientific" thoughts (I. to clarify certain, surprising.

The "Ural" homeland and archaeology

In the following, Fodor, in discussing the archaeological finds he attributes to the hypothetical "Ural" 6 primitive people, draws attention to Central Asian parallels — especially in the case of stone tools and clay vessels — and then writes on page 54: "When reviewing the characteristics of tool making and pottery, we also saw that this population maintained close ties with the developed Central Asian East-Minar civilization from the earliest beginnings that are currently palpable, and received more than one motive for its economic and general cultural progress from there. (There can be no doubt, for example, that they also learned pottery from their southern neighbors.)"

Unfortunately, we cannot agree with Fodor again.

Since the earliest archaeological evidence of this Ural region civilization dates back to around 4000 BC, and the earliest evidence of the Eastern Minarian civilization — the 4th settlement of Dzambaskala — dates back to the first half of the 3rd millennium BC at the latest, there are very serious chronological obstacles to Fodor's assumption that the Eastern Minarian civilization would have radiated culture to the Urals and the lower reaches of the Ob around 4000 BC. We can safely leave the solution of this difficult question to our author.

In connection with the age of the Early Minarian civilization, the date 4, 6702160161 BC has never been mentioned (except for Chernyvetsov; 1, later). The highest dating is the excavation It comes from Tolstov, who originally estimated the 4th layer of Dzambaskala to be around 3000 BC (Auf den Spuren der altchoresmischen Kultur). However, it has since become clear that the settlement is at least 2-300 years younger (Gy. László: Östört. legkor. szakazai, p. 105, with the relevant literature; and ; Propyläen Kunstgeschichte XIII. B. „Lurkestan" 6, chap.).

315

===== PAGE 312 =====

These would be the chronological facts. However, since there are undoubtedly numerous parallels with the Paleolithic-Mesolithic culture of the Ural region, and since the radiations from the south to the north can also be clearly demonstrated (1. Tolstov, ed. and Prop. Kunstg. XIII. B. "Turkestan" chapter), it would certainly be more in line with reality if these were to be considered as historical facts. However, this seems to have linguistic obstacles, in comparison with which the facts of archaeology naturally pale — at least according to the "Uralians" would be placed a good 1500 years later, roughly in the second half of the 3rd millennium BC.

typical mindset of our exclusive Finno-Ugric view of origin.

On the other hand, it has also become obvious from the findings of excavations in Turkmenistan and Central Asia in recent decades that the culture of the East Minarians was far behind that of the surrounding areas, remaining at a primitive Mesolithic-Neolithic stage even in such late times when copper and even bronze were already widespread among its neighbors.

However, after our experiences so far, we do not want to set the standard too high, and therefore we do not refer to these new archaeological results when we state that we can in no way speak of a "developed Central Asian East-Minar civilization", as Fodor does. We merely quote Gyula László, who wrote on pages 105-106 of his aforementioned work as early as 1961: "...the inhabitants of the 4th settlement of Dzambasz-kala, far from representing a center of civilization, but on the contrary, lagged behind the other tribes of the same age who populated the steppe areas in a wide area, and who already largely knew and used copper, and even the beginnings of animal husbandry can be observed among them, for example in the Kazakh lands."

However, in 1961, the excavations in Turkmenistan that led to the discovery of early civilizations there, and which fully confirmed the above opinion of Gyula László — deduced through careful analysis of scattered finds — had not yet begun. At that time, only the Anau settlement was known from the prehistoric period of Turkmenistan, but despite fierce debates, researchers could not agree on its chronological classification.

According to our current knowledge, the Neolithic Age began in this area around the turn of the 6th/5th millennium BC (unlike Fodor, we always apply this term to food-producing cultures). The first detectable Neolithic culture, the so-called "Chagall" culture, is characterized mainly by painted pottery, while the following "Mondjukli" period is already of a Chalcolithic nature and is in close contact with the contemporary cultures of East Asia. It seems to represent essentially the Hassuna culture of Mesopotamia, which penetrated to the northeast, and the Sialk I culture of western Iran.

From the perspective of our current question, the most important lesson from the results of these excavations in Turkmenistan is that the 3rd millennium BC

316
===== PAGE 313 =====

and the finds from the settlements in the Ural region, which are at most contemporaneous, but rather somewhat later, can most likely be linked to the appearance and expansion of the Proto-Asian agricultural colonists in Turkmenistan. In this way, we must attribute these archaeological find complexes to the legacy of the Turkmen-West Asian indigenous peoples, who were confined to the impassable swamps of the Amu Darya estuary, on the one hand, and to the legacy of the Turkmen-West Asian indigenous peoples who originally lived on Paleolithic cultural levels and who had learned a few things from the Proto-Asian settlers (e.g. the craft of pottery making) in the meantime.

Fodor did not consider all these considerations, nor did he even take into account the fundamental work of Gyula László, although he mentions it in his bibliography. Instead, he speaks of the effects of "advanced" Eastern European culture in the Urals, more than a thousand years before the appearance of the earliest finds of Eastern European culture. We cannot assume that this is a matter of lack of prior education, no matter how outrageous Fodor's conclusions may seem at first glance. Once again, we are faced with the same effort that sweeps aside all new prehistoric discoveries or archaeological finds, which wants to maintain the one-sided and outdated doctrine of our exclusive Finno-Ugric origin by force. However, this hypothesis can only be supported today — at least apparently — if they simply ignore all prehistoric, archaeological, cultural-historical, or any other data that contradicts their assumptions, and, rigidly adhering to the 19th century Indo-Germanic thesis of primitive language and primitive people, they try to prove the phantom of a hypothetical common primitive language deduced solely on the basis of "sound laws", and vary its supposed connections and variants endlessly, without regard to almost all other circumstances. But they do not even take into account the thousands of years of pre-Asian linguistic monuments that have been discovered in the meantime, meaning that they do not work with careful, complex methods even within linguistics in the narrowest sense.

Well, we don't need to go into detail about how much such a procedure has to do with systematic science. It is also related to this that every opinion or research direction that differs from their own views, instead of being refuted systematically and with objective arguments, is simply labeled as confusing, unscientific, a historical miracle bug, a distorted theory, and so on.

We do not want to indiscriminately attack the various prehistoric ideas that are proliferating like mushrooms. We ourselves know that most of these experiments cannot be taken seriously. We simply do not consider it permissible for them to be the ones to throw the most

===== PAGE 314 =====

with bigger stones, who - as we have seen repeatedly - are themselves sitting in a greenhouse. By way of analogy, we refer to the method of Gyula László, who did not need to know the results of the new excavations in their entirety when, as early as 1961, he was able to conclude, solely from the scattered finds that had surfaced up to that point, that the ancient Minyan civilization was nothing more than a primitive, Mesolithic-Early Neolithic cultural stage stuck in the swamps of the Aral Sea and the Amu Darya, at a time when the other neighboring peoples around them were already living in the Copper Age. However, he only managed to reach this realization by carefully considering all the data that could be counted and weighing them as a whole, not according to which one could be fitted. into a pre-made hypothesis. In any case, we consider Gyula László's broad-sighted research method to be far more scientific than the "snapped and fisher-hunter Neolithic" or "advanced" Eastern European civilization of the trend represented by Fodor, which is moreover forced — for the sake of the favorite hypothesis — to radiate culture as far as the Arctic Ocean a good millennium before its emergence.

Working hypotheses are undoubtedly necessary, but — please allow me — everything has its limits. The "Ural" homeland and paleoclimatology Also on page 54, Fodor writes — in parentheses — the following: "It was free from the Let us not forget that this region in this era — due to a much warmer climate than today — tundra." Our author wrote these lines in the millennium he assumed to be based on Hajdú. "Ural" homeland, i.e. the region of northwestern Siberia quite close to the Arctic Ocean, dates back to the 5th/4th century BC.

Leaving aside all climatological or geological hypotheses, which in many cases are in direct contradiction to each other and are still very far from certain, we have only one question for Fodor: how could it be that the ice-frozen mammoth carcasses, which are still found in large numbers in Northern Siberia today, did not disappear with the thawing of the tundra in the 5th/4th millennium BC?

It seems that the latent energies of the hypothetical Uralic proto-language are comparable to the thermal power of a supernuclear reactor, and are also capable of selectively melting the tundra along the Lower Ob, of course, only to create what linguistics has inferred for the 5th/4th millennium BC. For the "Uralic indigenous people" their ancestral home in the middle of the ice field was

318
===== PAGE 315 =====

Chernyvetsov's theory In the following pages, Fodor describes the supposed breakup, the "Uralic" and "Finno-Ugric" migrations, i.e. the alleged separation of the Samoyeds (c. 4000 BC), and then the "Finno-Ugric unity" separation of the Finnish peoples 68 from the "Ugrians" around 62000 BC. During this entire period, thought to be about 2000 years, the "Ugrians", or their predecessors, always lived east of the Urals, in Northwestern Siberia, he notes. In his line of thought, he follows everywhere - clearly recognizable - Chernyvetsov's views on the development and migration of the Finno-Ugric peoples (for a brief summary of Chernyvetsov's thoughts, see 1. Gy. László: id. m. 1961., p. 110).

After reading this part, we finally understand Fodor's "developed" Kelteminar civilization, which already radiated culture to the north around 4000 BC. So this was nothing more than a warm-up of Chernyvetsov's former chronological ideas. Chernyvetsov — to his credit: before the recent excavations in Turkmenistan — believed that Kelteminar civilization originated from 4000 BC, or perhaps even earlier. That is why he placed the settlements he discovered in the Ural region in the 4th-3rd millennium, based on the aforementioned Kelteminar parallels.

We must not forget that at that time the age of the only Neolithic settlement known from Turkmenistan — Anau — had not yet been satisfactorily clarified. Many authoritative researchers believed that Anau dates from the 6th, 7th, or possibly 8th millennium BC, and quite a few even claimed that Turkmenistan was the birthplace of food production. the very first focus of civilizations, and the Neolithic cultures would have spread from there to Near East Asia (see Rehork: Lübbs Enzyklopädie der Archäologie under the heading "Anau"). Chernyvetsov started from these ideas when he placed the age of the Kelteminar civilization before 4000 BC. It was undoubtedly possible to establish that very strong parallels with Anau can be found in the Kelteminar finds (I. Tolstow: id. m.). Kelteminar could therefore not have been much later than the Anau civilization. On the other hand, we also understand that the culture of the East Indies at that time -- in the absence of knowledge of other cultures in the area in question -- could indeed be called "advanced".

However, as we saw above, Chernyvetsov's assumed chronology has not been confirmed by recent research. The age of the 4th settlement of Jambas-kala is dated to the first half of the 3rd millennium BC. It was determined to be in the second half of the 19th century, and Anau dates from the 4th millennium, contemporaneous with the Namazga I and II periods. With this dating, the possibility of 4th millennium Uralian outgrowths of the Eastern Minarian culture also became illusory. However, Csernyecoy's indelible merit is that he emphatically pointed out the importance of the Mesolithic artefacts of the Eastern Minar and the Ural region he excavated.

===== PAGE 316 =====

close connections between these cultural influences: that the first cultural influences undoubtedly arrived from the south, from the Aral Sea region, to the eastern slopes of the Urals. He was only mistaken in placing these cultural impulses at the beginning of the 4th millennium BC instead of the second half of the 3rd millennium BC. However, in the absence of other comparable finds, we cannot accuse him of this mistake.

We learn nothing from Fodor about all these chronological corrections that have occurred in the meantime. Even in 1975, he still presents Chernyvetsov's former time definitions as if they were still valid in international archaeology. There is a good reason for this. Chernyvetsov's theory — but only in close connection with his original, outdated chronology — belongs to the extremely small number of archaeological-prehistoric concepts of foreign origin of recent decades, which, although with difficulty, can still be fitted into the hypothesis of a "single Uralic primitive people". Chernyvetsov's finds from around 4000 BC in the East Urals are excellently consistent with the common Chernyvetsov chronology inferred from Finno-Ugric linguistics, which is why he is profoundly silent about the drastic reduction of the datings, and why the tundras along the Lower Ob also melt — presumably from the heat of Fodor's enthusiasm — in the 5th/4th millennium BC. to the beginnings of the disintegration of the "Uralic Proto-Language", also thought to be around 4000. That is why Fodor took it up so enthusiastically

On Fodor's Andronovo "Ugors" and their metalwork On page 94 of his book, Fodor places the Ugrians in the western part of the northern regions of the Andronovo civilization (ca. 1700-1300 BC), and the Samoyeds further east, in the Tomsk region.

M. Grjasnow, in his volume "Südsibirien" of the "Archaeologia mundi" series, on the other hand, writes about the population of the Andronovo culture: "Die gesamte Bevölkerung war europid" (p. 94). In Hungarian: "The entire population was europid." But K. Jettmar also describes the Andronovo population as "plump europid" (Geschichte Mittelasiens).

According to the definition of anthropology, the Voguls, Ostyaks, and Samoyeds belong to the so-called Uralic type, which was formed from the mixing of Mongoloid, Paleosibirid, and Proto-Europid groups, but with the dominance of Mongoloid-Paleosibirid elements, i.e., it is much closer to the Mongoloids than to the Europids. The Mongoloids, on the other hand, only appear around 1000 BC in the western areas of pastoral Siberia, during the time of the Karasuk culture in the Yenisei-Altai region, i.e., long after the Andronovo culture (Gryasnow; id. m.).

In other words, it is very difficult to imagine how the population of the northern areas of the Andronovo civilization, which was exclusively Europid, could have been formed by

320

===== PAGE 317 =====

predominantly Mongoloid-Paleosibirid Voguls, Ossetians and Samoyeds > if only we do not assume, after the experiences so far, that this is certainly also the case. In the only salutary chapter entitled "Metalworking", discussing the bronze industry of "Theory" demands it.

A Andronovo, Fodor describes the handle decoration of a bronze knife as follows (107, p.):

"And at the end of the handle of the Rostovka knife we see an artistically arranged sculptural composition: the horse is pulling a skier. The skier's dynamic figure in particular impresses the modern observer with its artistic beauty. The meticulously sculpted face reflects unmistakable Mongoloid features. The horse's figure came out of the casting in a different formulation: its standing, calm posture is not in harmony with the skier's rapidly moving figure. Despite its internal contradiction, the composition offers a delightful sight. (It is very interesting that the key to solving the representation is given to us by a folk tale of the Ossetians, who still live in Western Siberia today. It tells the story of a hunter who went hunting in winter on skis pulled by a horse, then tied the prey to the skis on the way back and dragged it home with the horse. So this folk tale on the Rostovka knife is almost three and a half We see the revival of a thousand years.)"

Our author could only be praised for this exhaustive and lofty artistic opinion, if he had not made the blunder of including a picture of the knife in question on page 106, with a separate enlargement of the end of the handle. Looking at the illustration, anyone can easily conclude that few people apart from Fodor would be able to draw a dynamic,

The male figure behind the horse is in a sitting (or crouching) position, with his head and upper body slightly held aside, extending both arms forward, and reaching with his hands under the horse's tail, between its hind legs.

With this brief description, we have said everything about the composition: the man is not skiing, but is quite simply milking the horse from behind, as is the Scythian custom. There are several examples of this milking method among the Scythian finds.

The horse's "standing, calm posture" is therefore very much in keeping with the figure of the head man, and the composition has no "conformity" with the object it There is no "internal contradiction". It is just Fodor's imagination, describes, and its forced interpretation has an internal contradiction."

But it is also very characteristic how he immediately associates this skier, who only appears in his own imagination, with the Ob-Ugric people, referring to the clichéd analogy of a banal, featureless, and relatable narration of a contemporary Ossetian folktale.

Well, we hope we have managed to get the skier off his snowshoes once and for all, but we are still left with an unpleasant aftertaste; wasn't the man milking his horse supposed to be buried because...

321

===== PAGE 318 =====

Could this serve as support for the assumption of a "Ugric" population of the northern Andronovo areas through that certain Ostiak mountain? Or perhaps, milking his horse from behind Looking at the figure of the man from Andronovo, did Fodor have unpleasant associations with certain Mesopotamian depictions of the same nature? We cannot know.

However, we do know that this method of milking can be traced back to the Sumerians, from the first half of the 3rd millennium BC, when a reed tube was inserted into the anus of the animal. The exact depiction of this process can be found on numerous seal cylinders and on 1600 reliefs. We have already mentioned that it also appears on Scythian monuments from the 1st millennium BC, and Herodotus even describes it in detail, and according to Gyula László, it was still used by Hungarian shepherds in the Great Plain at the turn of the century, together with the castration of animals according to the Scythian method (l. The Life of the Conquering Hungarian People; pp. 298, 111, 328).

So, between the analogous Sumerian depictions of the 3rd millennium BC and the identical Scythian finds of the 1st millennium BC, it is the handle decoration of the Rostovka knife that proves the continuous survival of the tradition of this milking method in the 2nd millennium BC.

We know very well that we have no right to such conclusions, according to Fodorék, and our opinion can only be a mirage — even if it is icy. Fodor's figure of a man skiing, however, remains a strictly "mirage" in archaeological finds scientific interpretation even if the man is also milking a horse.

So far, everything would be fine with the rules of the game. The trouble only begins when we are not alone with such views." Recent research brings the development of the Afanasyev and Andronovo civilizations into very close contact with the advanced, already urbanized, Mesopotamian-Sumerian-type Turkmen cultures of the 3rd millennium BC. We have also seen that the settlements of the Turkmen civilizations were depopulated between 2500 and 1500 BC. At the time, we explained the depopulation of southern Turkmenistan and the almost simultaneous appearance of the Afanasyev and Andronovo civilizations by the fact that the population migrated north to the Eurasian steppe and developed the aforementioned cultures there.

In the meantime, we have come across B. Stawiski's work "Mittelasien — Kunst der Kuschan" (1979), in which we can read the following in relation to the above; "Mitte des 2.

In the millennium BC, this development was interrupted: Events that have not yet been explained apparently led to extensive migrations of tribes who settled in the vast steppes of Eurasianism, southern Ukraine, and the Volga region.

+ (See more details in the next book)

322

===== PAGE 319 =====

and the southern Ural region as far as the northern districts of Central Asia, East Kazakhstan, South Siberia and possibly also Mongolia" (19th al.)

In Hungarian: "In the middle of the 2nd millennium BC this development was interrupted, events that have not yet been clarified, by all indications, led to large-scale migrations of peoples. In the Volga region, in the southern Urals region, up to Kazakhstan, southern Siberia, and possibly even Mongolia." to the northern territories of Central Asia: Eastern migratory tribes settled in the vast steppes of Eurasia, in southern Ukraine,

These were not the first migrations, however. The limited arable land and pastures of the oases had already forced the rapidly growing population of the agricultural and animal husbandry cultures of southern Turkmenistan to constantly send out new swarms of settlers, writes Stawiski a few lines above, also on page 19.

This statement — from an authoritative scientist — not only fully justifies our above opinion, but also strongly supports our conviction that agricultural-animal husbandry cultures, compared to the fishing-hunting-gathering way of life, led to the development of incomparably larger, compact populations. These Neolithic, then Chalcolithic and Early Bronze Age Near Eastern ethnicities, due to overpopulation, initially flooded the areas of South Asia (Turkmenistan, India) and South Europe (Aegean, Balkan, Carpathian Basin), and then — after the population had also overgrown there — the more northern regions of Eurasia; in continuous waves, fleeing from famine and always searching for new and new farmland.

However, Stawiski's studies also make it clear that whether the distant ancestors of the ethnic groups now called Ugric lived in the northern areas of the Andronovo civilization proper, or whether they lived only beyond the northern borders of this culture, they certainly came into contact with a civilization of Mesopotamian-Sumerian origin through the mediation of the Andronovo people.

From all this it follows that the Andronovo "preszkita" milking his horse from behind fits far better into the cultural historical picture that can be drawn based on our current knowledge than the skiing "preoszjak".

In fact, we can even risk the opinion with a clear conscience that we can look for it in the shepherds of the Great the key to the solution of this representation, rather than the tradition of this way of understanding Plain who preserved it until recently, rather than in Fodor's tale of the Ostyaks.

4 On pages 108 and 109, Fodor argues that we have very serious reasons to assume that at least some of the artistically executed Andronovo bronzes should be considered the work of "Ugric" bronze craftsmen. He also claims here that these "Ugric" bronze craftsmen were the Andro-323

===== PAGE 320 =====

In the novói period - that is, in the middle of the 6th to 2nd millennium BC = they already had developed firearms, evidence of which, according to him, are the firearms found on the Sejm-Turbin bronze casting samples, but also the handle of the Rostov knife discussed earlier, with the horse drawn by 51616 men.

[These "evidences" - as we have seen - are based solely on the unsubstantiated a priori assumption that the population of the northernmost areas of the Andronovo culture was formed by the ancestors of the "Ugrians" and the Samoyeds in the modern sense. We recall Gryasnow's statement in this regard: "Die gesamte Bevölkerung war. europid." Apart from this, however, we also have very clear data that the modern "Ugrians" did not know metalworking even in the 18th century AD. Tsar Peter the Great, for example, in a separate decree instructed the governor of his Ural province to prohibit trade in metal objects with the Voguls and Ossetians, because then these would not fight in the absence of metal weapons - since they could not make such weapons themselves. emphasizes that the Voguls and the Ossetians were still at the Stone Age level of culture in the 19th century. Not only were they not able to develop any kind of their own culture, but they were also unable to appropriate the culture of the neighboring peoples, even in some way. Before the Tatar period, they did not even know weaving (Sirieliuss ; István Zichy gr., in his work "The Prehistory and Culture of the Hungarians until the Conquest" emphatically states report). The finds from Ananjin also show that the Stone Age indigenous population was settled by a conquering people who came from the south and had a Bronze and later a Vase culture, or by a people who established colonial settlements. At these colonial and commercial centers, cattle, sheep, goats and pigs were also domesticated, but only for their own use, he writes. According to Zichy, the archaeological remains of the Ananjin animal husbandry testify to colonization for economic reasons. We know that Zichy's theory is considered outdated in Finno-Ugric circles. It is also very characteristic that already in 1923, it was clear, and according to the latest archaeological data, it came very close to the real situation. Let us look, for example, at János Matolcsi's article "The Animal Husbandry of the Central Volga-Vidéki Finno-Ugric Peoples in the Iron Age" (Magyar ystört. tanulm 1977). According to Matolcsi, the uneven distribution of animal bones on individual sites greatly complicates the picture and makes it very difficult to explain the Ananjin animal husbandry. | A realistic interpretation of the nature of Jatiartás, Mostly one type of animal dominates each settlement: e.g. 59.3% cattle or 67.8% 16 § or 50% pigs. From this fact, which is clearly evident from the tone of the article § — ultimately incomprehensible, various hesitant comments;

He tries to draw conclusions about the "transitional" animal husbandry of the Ananindi people, or the special preference of different tribal groups for certain animal species; for example, he directly connects the 50% of pigs in the Svinogor settlement with the "traditions of our people's attachment to pigs." The fact that in Kiryuchsky Gorodok, located directly next to Shvinogor, "only horses were bred," according to his own words, does not bother him at all." This is despite the fact that a few pages earlier, based on thorough osteological analysis and measurements, he himself showed that the breed of cattle bred in the area of the Ananino culture was closest to the southern, steppe cattle of the same era and was much larger in size than the forest-belt cattle breeds of a thousand years later. Moreover, the Ananino horses are closest to the Avar horses of Hungary of the 1st millennium AD, and show almost identical breed characteristics, he writes.

All these data - when compared with the general cultural and historical picture of the Eastern European region in question in the 1st millennium BC - only allow us to conclude that the animal husbandry of the Ananino civilization, the characteristic ratio of animal breeds varying from settlement to settlement, and the far-reaching dominance of a particular animal breed at individual sites were nothing more than the well-thought-out, organized, animal farm-like animal husbandry practiced in the northern colonial settlements of the Scythian-Sarmatian civilization, specifically oriented towards the division of labor. This is also proven by the steppe origin of the 16 and cattle breeds, in contrast to the different types of livestock in other areas of the forest belt.

We would have thus once again arrived at the results of Zichy's research formulated in 1923, if the doctrine of our Finno-Ugric origin, and in close connection with it, the a priori assumption of the Finno-Ugric ethnicity of the Ananino culture as a whole, had not closed off broader historical perspectives from our researchers and had not narrowed their horizons only to the regions north of the aforementioned demarcation line of 50 degrees north latitude.

Let us now return to Fodor's "Ugric" bronze artists from Andronovo. We have already seen that the relevant volume of the "Handbuch der Orientalistik" places the beginnings of the formation of the Uralic(!) peoples in the second half of the 2nd millennium BC. According to Fodor, by the middle of the same millennium these Uralic peoples had long since split up, not only into Finno-Ugric and Samoyedic

* However, if you had thought through your previous idea consistently, you might have realized that here, on the other hand, our people's well-known love of hunting is revealed; and so on: each settlement in Ananjin has preserved a characteristic Hungarian characteristic, similar to the two-headed calves and other wonders preserved under spirit in the curio cabinets of older museums.

325

===== PAGE 322 =====

but also to Finns and Ugric peoples. We only wanted to show once again how far our historiography has fallen behind the international standard, and that the Finno-Ugric trend really does not take into account any new insights that contradict its 19th-century linguistic beliefs,

Lam, Fodor's single-minded gaze, like a rabbit enchanted by a cobra, is directed exclusively at those "Vogul roots" that Hungarian Finno-Ugric studies, following Budenz and Hunfalvy, deduced on the basis of so-called "linguistic historical facts". And these "linguistic historical facts" in an irrefutable way command Fodor to view and evaluate the ancient development of almost the whole of Western Asia only according to the extent to which it is compatible with the Vogul roots. And since, according to our linguists, the 1st in the middle of the 2nd millennium -- that is, in the age of the Andronovo culture -- we must undoubtedly reckon with a "separate" "Ugric" proto-language, and consequently, of course, with a separate "Ugric" people. Fodor has no other option than to "align" the archaeological remains of the Andronovo culture with these 2nd millennium linguistic phantom Ugric people. However, he is far from content with - let's say + speaking only of Andronovo-Proto-Ugric relations, of Andronovo cultural influences among his Ugrians, which - although with very strong reservations - would still be acceptable. No, with surprising hussar-like boldness, with the slogan "straight into the thick of it", he rushes into the very heart of the Andronovo culture at the head of his Ugrians and discusses "at least some" of the truly artistically executed Andronovo bronzes as the products of this hypothetical people.

This secondary assumption, which was originally only shaky, one-sided, and based exclusively on "linguistic-historical" assumptions, therefore has no theoretical or methodological obstacles according to the norms accepted in our historical science. The inferred Vogul roots are more than sufficient for scientific support. Otherwise, Róna-Tas Fodor could not have written about this book that it is another work of our prehistory research, one of the most outstanding works.

Additions to one or two theoretical and methodological questions,

Let's think about this scientific method for a moment, but let's also take a look at the other side of the coin.

We almost involuntarily remember that we have already read everything about how the Hungarians of the time of the conquest undoubtedly had an outstandingly high level of color and artistic refinement.

whether his metal objects were made by goldsmiths from Alans, Caucasians, Khorezmians, Bulgar-Turks, Kyrgyz from the Minusinsk region, Tatars, or who knows what nationality. Respect for the exceptions, but we must state that the vast majority of our historians.

326

===== PAGE 323 =====

It is obvious that he considered it impossible to assume from the outset -- and still largely does today -- that those silver and bronze ingots could possibly be the work of Hungarian metalworkers. This assumption > seems to have theoretical and methodological obstacles,

However, as Fodor's above line of thought and conclusions clearly show, these scientific inhibitions dissolve the moment we are not specifically talking about Hungarians, even if these are "our closest linguistic relatives", the Ugric people, who can undoubtedly be shown not to have known metalworking even in the recent past.

And we -- with our simple-minded peasant -- see a theoretical and methodological error in this very approach. Namely, if there is no obstacle to hypothetical Ob-Ugric and Samoyeds producing bronzes of excellent quality and workmanship around 1500 BC, why do we have to struggle with the idea that the conquering Hungarians, who really existed around 900 AD, during the Azid period, could have had their own goldsmiths?

We would like to emphasize that we are only asking this question in general terms, on a theoretical-methodological level, and we are consciously not going into the details, the migrations, the origins of the metalwork style of the conquering Hungarians, etc. Our next reflection is also connected with such fundamental theoretical questions, to which Fodor's comments on pages 125-126 of his book prompt us. There, speaking of the alleged northern migration of the Ob-Ugrians around 1000 BC, he explains that although they became fishermen and hunters again in the harsh climate, having reached the high north, they have preserved the memories of their former animal husbandry and farming and equestrian life in their folk poetry and customs almost to this day.

We note in passing that this method of reasoning is exactly the same as the 19th century Indo-Germanic "proving" procedure, according to which the northern Indo-Germans also "lost" the previously known razor. Moreover, the starting point of the reasoning is the same: in both cases, allegedly common proto-language words deduced by linguistics. Namely, in the case of the Indo-Germans, the parallel phonetic forms of the name for razor can be found in several Indo-Germanic languages, and in the case of the Voguls, words related to horse keeping that are related to Hungarian. The only difference is that while Indo-Germanic studies have since revised this 19th-century argument and shown that the similar sounding of the name for razor in some Indo-Germanic languages does not even remotely prove that this concept should have existed in the Proto-Indo-Germanic languages, but can rather be explained by later borrowings; Finno-Ugric studies still apodictically claim that the Hungarian-Vogul parallels of the words for horse keeping are only in the co-327

===== PAGE 324 =====

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They may originate from a common language, 131161 Viktor Padányi's one-sided approach was very characteristically expressed by our linguists, who "reduce the Hungarian language to the Vogul roots". Well, as we saw above, nowadays we have already reached the point of reducing not only our language, but also the Andronovo culture, which extends to half of Western Asia, at least in part", as Fodor writes - to the Obi-Ugric,

-- Returning to the theoretical and methodological issues of historical science, we can state that, following Fodor's more vivid argument, there is no obstacle to our historical science being able to assume, without further ado, 3,000 years of uninterrupted folk memory and oral traditions among the Voguls and Ossetians.

However, if we look at the other side of the coin in this case, we only have to see that the same historians who treat the folk memory of the Obi-Ugrians, which goes back three millennia, as a fact, deny the possibility of the survival of oral traditions of the Hungarians even in the future, even in the future. We are thinking here primarily of the destructive criticism of our medieval chronicles, which can be found in legions of scientific articles and books, where one of the main arguments is almost always that the Hungarians had long forgotten everything by the 13th century, not only from the Hun-Avar times, but also from the time of the conquest.

To give a concrete example: from the death of Attila to the mention of the so-called "Attila sword" given by Solomon's mother to the Bavarian prince Otto of Nordheim, only 600 years passed, but the Hungarians -- according to our historians -- had already forgotten the world-beating Attila, whose empire they had to belong to according to all historical data, wherever their territory was in the 5th century, so much so that the German chroniclers explained to them: "You are relatives of the Huns, and your kings are descendants of Attila" (see 6, György Györfy, in his book "Our Chronicles and Hungarian Prehistory" with his views on the German origin of the tradition of the "Attila sword").

We ask the question again: what theoretical and methodological obstacles prevent our historians from taking 3,000 years of folk memory for granted among the Voguls, while in the case of the Hungarians they do not even recognize the possibility of the survival of oral traditions for 600 or 300 years? Can such a view of history, which measures by two different standards, be called objective?

We consider it necessary to emphatically point out in connection with this issue that, according to Sirelius' research, the heroic songs and folk memories of the Voguls and Ossetians hardly spread beyond the era of Tatar influence, i.e. the 13th-15th centuries (reported by Zichy I. in his aforementioned work).

But Gyula Németh also established that their oldest tangible traditions are about the Sabirs, whom they speak of with great respect and humility as superior.

on race (The formation of the conquering Hungarians), |

328

===== PAGE 325 =====

The known data in this direction can also be supplemented by the results of Patkanov's studies, according to which the Voguls and Ossetians of the Tobolsk region consider the Sabirs to be a superior race and boast that they are descended from them (The Nationality of the Sabirs). However, since we also learn from Patkanov that the Tatar inhabitants of the Tobolsk region also attribute all old relics that they find during excavations or by chance to the legacy of the "Siberian" people, it is very doubtful to what extent we can speak of original, unadulterated Sabir traditions among the Voguls and Ossetians.

However, if we are talking about truly original traditions, we can also look for the origin of the Vogul words related to horse breeding, which are related to Hungarian, here, and not only in the hypothetical "Proto-Ugric" language. In order to shed more light on the validity of this idea, we would like to point out some very remarkable circumstances.

First of all, it is a well-known fact that the older name of the Hungarians was "sabartoi", i.e. "szabir". Consequently, we cannot rule out the possibility that the possibility that the Sabirs, who appear in the memory of the Ob-Ugrians and who in all probability ruled them at the time, may be hiding Hungarians who, having separated from Árpád's Hungarians in earlier times, preserved their old "Sabir" name.

On the other hand, we can refer to the fact that the common Hungarian-Vogul horse-keeping words are missing from the other Finno-Ugric languages (Mészöly: How long have the Hungarians been a horse-breeding people?). Finno-Ugric linguistics naturally explains this with the separate "Ugric" proto-language, but it is much more likely that they could have entered Vogul directly from the Hungarian language, sometime between the 8th and 13th centuries AD; namely, from the Magyars of the Volga region, "Magna Hungaria" found by Julianus, who more than likely migrated to the region of the middle Volga and present-day Bashkiria together with the Volga Bulgars at the end of the 7th century, after the collapse of the Onogur-Bulgarian empire of Kovrát.

Thirdly, we must not forget that it is precisely our "closest linguistic relatives", the Ob-Ugric peoples, who are anthropologically the most distant from both the Hungarians and the other Finno-Ugric peoples. For this reason, but also based on linguistic considerations, several Finno-Ugric linguists strongly doubt that the Voguls and the Ostyaks were originally Finno-Ugric. They consider them to be a Paleosiberian people who only adopted their present-day Finno-Ugric language from the Finno-Ugric peoples in relatively late times, as a result of their proximity to them and their massive linguistic influence.

Our linguistics and historical research have not paid much attention to these possibilities, although the importance of the question is undoubtedly profound and prudent. would require investigations.

Returning to the question of the 3000-year-old folk memory of the Voguls, we must also refer to one of the most prominent figures in Finno-Ugric linguistics

329

===== PAGE 326 =====

representative, József Szinnyei, who himself was forced to admit that the "Ugric" peoples - the Voguls, the Ossetians - had not even recently reached the level of culture of personifying natural phenomena in the form of gods (Die Herkunft der Ungarn, ihre Sprache und Urkultur).

Evidence of extremely tenacious traditions is also provided by Kandra Kabos in his book "Magyar Mythologia". In particular, the creation myth, on which Kandra bases almost all of his further explanations, was first recorded by Reguly in 1843. However, Bernát Munkácsi - barely 45 years after Reguly - heard this myth in a completely different way from the Voguls (1. On page 30 of the book mentioned by Kandra).

This Vogul creation myth is apparently so "ancient" that we could not find anything better to use as a basis for reconstructing the lost Hungarian mythology, as Kandra does - with a zeal worthy of a better cause. Thus, for example, the heaven of Hungarian traditions, the starry sky, also receives its Vogul parallel: the Vogul paradise, which is imagined on an island in the Arctic Ocean, is connected to the starry sky by the high-rising rays of the northern lights, writes Kandra. Similar, often embarrassingly primitive comparisons can be found in abundance in the relevant literature.

It is already clear from these few excerpts that it would be much more expedient if our researchers did not start from some 3000-year-old Vogul traditions, but tried to approach and explain the words and traditions related to horse keeping found among the Ob-Ugric people in a more obvious, historically more understandable way. All available data indicate that the Ob-Ugric people never had their own independent horse keeping, but simply adopted some words from the equestrian terminology of the steppe horse-riding peoples and their horse cult at some point, but by no means 3000 years ago, but - according to the evidence of their traditions related to the Sabirs - very likely between the 6th and 13th centuries AD. century, namely either from the Sabirs (Hungarians?) or perhaps from the "Ugric" in the true sense, i.e. Ogur (Onogur, Hunugur) ethnic group, which appears at the beginning of the 6th century AD in the region of the middle Volga and the Kama, Byelaja, Chusovaja, in the finds of the Nevolino group of the Lomovotovo culture,

These questions still require a lot of research, free from any kind of Finno-Ugric prejudice. But the adherents of the Finno-Ugric movement, who value ancient Vogul traditions so highly, might also consider that the Voguls believe that they are descended from the Sabirs and not the Sabirs from them.

330

===== PAGE 327 =====

The assumption of the former high cultural level of the Ob-Ugrians and their assertion at all costs is also a consequence of the linguistic historical ideas of 19th century Finno-Ugric studies. Since not only the Hungarian language, but also the Hungarian ethnicity was derived from these Ob-Ugrians, who were still living in a state of primitiveness in the 19th century, it was necessary to somehow bridge the enormous cultural gap that existed between the 19th century Ugrians and the conquering Hungarians. Despite all the efforts of Schölzer, Rössler, Büdinger, Hunfalvy and their colleagues, it gradually became obvious that the cultural and social development of the conquering Hungarians was towering above the level of the 19th century Ob-Ugrians, even 1000 years earlier. That is why it was necessary to set up various made-up hypotheses about their culture, which they had forgotten during their wanderings through harsh landscapes? —

"Ancient Ugrians" once high

, similar to the Indo-Germanic razor.

The "Uralic", "Finno-Ugric" and "Ugric" migrations

Let us return to Fodor's book. Regarding the alleged Ob-Ugric migration mentioned earlier, on page 126, he writes the following: "The above data prove beyond any doubt that one branch of the Ob-Ugric peoples were communities that migrated from the south, from the Ugric homeland, to the north. The other branch was the surviving local Uralic population.

were ethnic groups:" Then below: "The Ob-Ugric peoples carried the signs of their dual-rooted origin until recent times. The development of their language was determined by the language of the Ugric peoples who came from the south, and the indigenous people gradually assimilated. However, in their physical constitution, the Uralic type, characteristic of the indigenous population, prevailed." (pp. 126-127).

This completes the confusion in Fodor's conception of prehistory. He weaved and weaved the motley patterns of his assumptions until he managed to weave the threads together in such a way that he no longer recognized himself among them. But there must be a man on his feet who would be able to navigate among these language-changing, species-changing, pendulum-like fragments of people who migrated now south, now north.

The "Preugorians", who broke away from the Mongoloid Urals, first migrated south, where they changed their race, presumably becoming "plump Europid" Andronovians - Fodor forgot to explain this at the time - and then, migrating north again, they "changed their shirt" and took on their original Uralic Mongoloid racial characteristics.

This would be fine so far, as far as the extremely improbable idea that peoples repeatedly change their anthropological constitution and their racial characteristics over a short millennium, like underwear, is fine = as Fodor's line of thought clearly implies.

331

===== PAGE 328 =====

(Migration south around 2000 BC, migration north again around 1000 BC). However, the argument - taken in itself + is at least still reasonable.

The trouble begins with the fact that Fodor rarely contradicts himself. It seems that he was also troubled by the aforementioned archaeological-anthropological finding that the Andronovo people were European, while at the time he attributed the northern areas of the 6 civilizations to the Ob-Ugric and Samoyed peoples, who, as is well known, belong to the strongly Mongoloid Uralic race. He is now therefore attempting to explain his evidence to a certain extent, and even if he is completely silent about the Samoyed people, he is at least trying to Mongolize the Ob-Ugric peoples in later times. The fact that the skier from Andronovo emphatically emphasized on page 107 the "Ugric" face of the skier, well, Fodor has completely forgotten this poor skier Mongoloid "Ugric". In this he shows himself to be a true Hungarian, and even significantly surpasses our ancestors, who unmistakable Mongoloid features: "röz", a circumstance that is reflected in the Andronovo "Ugric" needed another 2-300 years to forget something. A short 20 pages is enough for Fodor.

However, there are other, much more far-reaching contradictions here. It clearly follows from Fodor's line of thought quoted above that they were Mongoloids. Apart from the really unimportant question of whether the phantom Ugrians "ancient Urals" placed by Fodor in the area of the Andronovo culture became Early European over time or not - we do not get an answer from Fodor, because as we have seen, he argues one way and another way - the anthropological fact that, apart from the Ob-Ugrians and the Samoyeds, all other Finno-Ugric peoples are Europid remains unexplained. And speaking in terms of numbers, this means that 99.75% of the descendants of the Mongoloid, "primordial" Uralic people became Europid over time. We do not believe in such miracles.

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But it is equally inconceivable that the common ancestral basic language of all these Europid Finno-Ugric peoples would have been the language of the Mongoloid Ősuraliak. In other words, the people groups that migrated after the breakup of the alleged proto-Uralic unity would have all found themselves in such an overwhelmingly Europid environment that they all lost their original Mongoloid racial traits, yet miraculously they all preserved their Uralic, Finno-Ugric language. And such a thing could have happened only in two cases: On the one hand, if the Europids in question, who constitute the overwhelming majority, had themselves spoken languages closely related to the Uralic basic language -- which is certainly hardly conceivable yy

—, on the other hand, if the ancient Urals had a much higher level of culture than the aforementioned Europids, and as a result the latter, 41 they would have taken the Ural-Finno-Ugric "cultural language." However, this second possibility is clearly ruled out by archaeological finds.

332

===== PAGE 329 =====

Summarizing what has been said so far, we can once again state that the convulsive adherence to the 19th-century theory of a "submerged Ugric" ancestral languages and their corresponding ancestral peoples, "Finno-Ugric", even "Finno-prehistoric historians to invent the most unreasonable and far-fetched assumptions.

Archaeological traces of the development of Finno-Ugric peoples in Eastern Europe

Naturally, we will probably never be able to fully understand the paleoethnogenetic processes that took place in such ancient times, with great accuracy.

This follows from the essence of the matter. However, we already have enough data today to be able to outline, at least in broad outline, the Eastern European population movements of the 3rd and 2nd millennia BC. Gyula László partly completed this task; his archaeological research has in many cases strongly supported Gyula László's In his fundamental work "The Earliest Stages of Our Common History": Two decades have passed since the publication of the book concept. Consequently, as long as possible new archaeological and prehistoric discoveries do not make Gyula László's prehistoric picture obsolete, we can safely follow his footsteps in researching the Mesolithic, Neolithic and Early Metal Age development of Eastern Europe.

We don't need to have "Ugars" migrating back and forth to the assumption of "Ural" ancestral homelands, nor to the search for Eastern Minarian cultural influences around 4000 BC on the shores of the Arctic Ocean, nor to the across Western Siberia, changing their anthropological characteristics like underwear.

Archaeological research in recent decades has clearly shown that West Eurasian culture originated in East Asia, from where it gradually spread to the surrounding areas. to more northern areas, tracking the flora and fauna that are penetrating further north as the weather gradually eases. In the 3rd millennium BC, the Upper Paleolithic or Mesolithic cultural groups living in the northern fringes of the agricultural zone of the Caucasian, Cuban, Pontic cultures and the Tripoli culture were gradually forced further north as a result of the expansion of the aforementioned agricultural and partly metalworking cultures. Around the turn of the 3rd/2nd millennium, these agricultural and animal-breeding cultures — originally largely ethnically from East Asia — reached the southern borders of the forest belt. Meanwhile, the native population of the steppe and forest-steppe areas (the Swedes) partly assimilated and partly penetrated the forest belt. As Gyula László writes, there they mixed with the earlier inhabitants, the Paleolithic hunters of Kunda and Sigr, and together with them they formed the earliest nucleus of the ethnic group that 333

===== PAGE 330 =====

We call them Finno-Ugric today. However, some groups of the Kunds and Sigrs moved away from the Swedes to the northwest, north and northeast. In these we must, in all probability, see the direct ancestors of today's Lapps, Voguls, Ostyaks and Samoyeds (Gy. László: id. m. 1961, pp. 193-194). In the region in question, no large-scale migrations to the south can be found anywhere in these times, but archaeology always finds traces of expansion to the north, east or west. This global process continues almost to the present day. Since the turn of the 1st millennium AD, for example, the slow but tenacious and constant expansion of the Russian-Slavic ethnicity towards the north and east can undoubtedly be demonstrated. Over the last thousand years, more and more Finno-Ugric peoples and tribes have disappeared and merged into the Russian population. According to archaeological evidence, this was no different in prehistoric times. But this is actually obvious and natural. Let us not forget that ancient people also had at least enough common sense not to go "on ice" as long as there was room for them in friendlier, milder climates, ice-free areas.

We can therefore assume with great probability that the various ethnic groups from which the bulk of the later, so-called Finno-Ugric peoples were amalgamated during the 2nd millennium BC can be identified with the inhabitants of the Paleolithic-Mesolithic settlements of the 6th-3rd millennium BC discovered in the region bordered by the Vistula-Baltic, the middle Volga-Kama and the Ural Mountains, who were forced further and further north as a result of the expansion of southern agricultural cultures, but at the same time — previously immeasurable in extent their hunting grounds were drastically reduced — they were forced into ever closer contact with each other. The smaller the area became, the more intense their relations naturally had to become. In the period when these contacts increased to a significant degree of ethnic mixing, we can first speak of the development of a more or less uniform contact language — or languages — covering larger areas in this region, as it were, in the manner of a "lingua franca". The signs therefore indicate that the Finno-Ugric languages are not a matter of the branching of some original, unified proto-language, but rather of the gradual equalization and, to a certain extent, unification of the originally different languages of many small tribal groups. But these questions belong to linguistics, which we will deal with in detail later.

Simon of Kézai, the Bible, Nimrod and the Hungarian Scythian tradition
Let us return to Fodor's book again. In the chapter "In the Southern Urals" (pp. 139-157), the pre-Hungarians around 500 BC are assumed to have been farmers-334

===== PAGE 331 =====

He discusses the social, cultural and educational relations of the ancient Hungarians. He states that the ancient Hungarians lived in the area of the Scythian-Sarmatian civilization at this time and their way of life was certainly not much different from theirs. He also preserved much of the spiritual treasures of the Scythian-Sarmatian world = he writes - and for example, the legend of the miraculous deer or the frequency of eagle depictions in Hungarian art during the Hungarian period are Scythian traditions, which the Hungarians have preserved for more than a millennium. In this regard, he quotes Kézai's version of the miraculous deer legend, and then notes: "Master Kézai also used the text of the Bible, since he made the ancestor of the Hungarians the giant Ménró, the offspring of Japheth." (p. 154)

We would like to take this opportunity to correct this untruth that has haunted our historical literature for over a hundred years, and has been passed on uncritically from one author to another. In short: Kézai did not use the text of the Bible, as he likes to emphasize. Let us look at the texts in question themselves. "In search of glorious ancestors" — as our literature has said in most cases since Hunfalvy

Genesis, chapter 10, verses 2, 3, 4: The sons of Japheth: Gomer, Magog, Madai, Javan, Tubal, Meshech, and Tiras. The sons of Gomer: Ashkenaz, Riphath, and Togamah. The sons of Javan: Elishah, Tarshish, and the Danites." Genesis, chapter 10, verse 6: he The sons of Ham: Cush, Mizraim, Put, and Canaan." Genesis, chapter 10, verse 9: Kittim. Cush begat Nimrod. He was the first ruler on earth. Great was a hunter before the Lord. Hence the saying: "a great hunter before the Lord, like Nimrod." Genesis, chapter 11, verse 4: Then they said, "Come, let us build ourselves a city and a tower whose top reaches to the heavens. Let us make a name for ourselves, and let us not be scattered over the face of the earth." "His In the two hundred and first year after the flood, the giant Menrot, son of Thana, descended from the blood of Japheth, having learned from the dangers of the past, built a tower with all his kin. hands: he began to build it, that if the flood should be repeated, they might flee to the tower to escape the avenging judgment." After the beginning of the confusion of languages, the giant Meenrot moved to the land of Evilath, which region was called Persia at that time. What can we see from the quotes? That there is no question of Kézai copying the relevant part of the Bible, but rather that he dared to contradict the Bible when he reported several circumstances that are either not found in the Bible or that directly contradict its statements.

A significant contradiction is first of all the derivation of Menrat (or: "Menproth" or, Nemproth" is a corruption of the modern text): in the Bible he was descended from Ham. 335

===== PAGE 332 =====

yes, Kezainál 141616. His father in the Bible is Cush, in Kezainál Thana - whose name does not even appear in the Bible. Kézai also contradicts the Bible in that, while he believes the Tower of Babel was built so that people could escape there if the flood were to happen again, the Bible explains the building of the tower as people's desire for fame and glory. The Bible does not state the date of the construction of the Tower of Babel, nor does it state that Nimrod built it. The Bible also does not mention that Nimrod later moved to the "land of Evilath," and what's more, this expression is not found anywhere else in the Bible, not even in any other context. It is also worth mentioning that Kézai's consistent departure leads us The Bible calls Menrot a "giant" ("gygas"), and the Bible - just as consistently - calls Nimrod a "hunter". This is to the realm of the most ancient traditions, the heroic legends, and mythology, because he claims that Kézai has passed down to us the traditions of an independent, non-biblical Nimrod heroic legend circle. From the similarities and differences of the "permanent decorative epithets" (epitheton ornans) of the legendary heroes or mythological figures, we can very likely conclude the common or different sources of the legends in question. The explanation for the construction of the Tower of Babel also points to an independent source, but one that is also older than the Bible. In the Mesopotamian plains, the Sumerian tower temples did indeed serve as shelters during the frequent floods of the Tigris and Euphrates. The ruins of these ziggurats still rise like islands from the sea of water during the spring floods. In contrast, the editors of the Bible were apparently no longer familiar with these ancient traditions, and were instead looking for a more informed explanation. So these would be the facts. Now, what should we think of a historical science that fails to notice these clear, glaring textual discrepancies, and even contradictions, even after a hundred years? This is simply unthinkable. Therefore, we must think of other reasons. All signs point to the fact that we are dealing with a dogma that is sacred and inviolable. We have a particularly strong suspicion that this dogma was adopted by the council of our historians during the reign of Pope Pál Hunfalvy. And since Hunfalvy and his followers have since been canonized and are revered as martyrs of "bourgeois positivist" historiography (let us remember Bartha's gracious words), there can of course be no question of a possible revision of the dogma. However, Hunfalvy -- demonstrably -- mainly passed on the revelations of the various Indo-Germanic deities of Valhalla (Schlözer, Zeuss, Büdinger, etc.) to his Hungarian followers, thus casting doubt on the roots of this dogma.

===== PAGE 333 =====

We can look for it in 19th-century German historical scholarship, where the thesis that the Hungarian language and ethnicity are of exclusively Finno-Ugric origin was also born. We do not want to get involved in these questions of the history of science now, because even a cursory overview of them would require a thick volume. Let us instead look at the eminently important connections pointed out in 1898, in Sámuel Krausz's dissertation, in connection with Kézai's "Biblical Aspects of Our National Chronicles" alleged Bible exegesis. Krausz showed that Kezai did not draw from the Bible, but from Babylonian pagan myths. According to the Bible, Nimrod is not a descendant of Japheth, but in pagan tradition he is. Neither the Bible nor the church father Jerome contains the detail that Nimrod began the construction of the Tower of Babel, but this is authentically found in pagan sources. For example, the Babylonian Chaldean historian Berossus (around 270 BC) presents this so similarly to Kezai that the connection is completely obvious. He only differs from Kezai in the date of the construction of the tower, writing 131 years after the flood, which, however, does not represent a significant difference compared to Keza's 201 years. The main emphasis here is on the statement of the date itself, which is found only in these two authors. Berossus also talks a lot about the Scythians in connection with Nimrod, and even mentions the Tanais River (cf. Ménrot's father's name, Thana, in Kézain). According to the church father Epiphanius, he was the founder of Bactria. Isidorus Hispalensis also writes the same. We should know about Eviláth that in ancient times there was a small and a large Eviláth. Later this country was called Hunnia. Kosmas The Hellenes consider that Nemrot to be Zoroaster, who, having moved further towards the eastern regions, Indikopleustes traveled to South Arabia, Abyssinia and India around 510 AD, and in his work "Christian Topography" he writes in one place: that is, Eviláth." (We would like to remind you that Károly Fiók showed in 1895 that the name Eviláth preserves the name of the Hephthalite-Huns in his hands.) The testimony of pagan sources is therefore that, Krausz concludes his explanations, Nimrod was the founder India and Hunnia, of the Scythian religion, and he was also the first king of the Scythian regions, just as he was of Assyria. In connection with the Old Testament it is worth noting that it contains many elements from the Babylonian world of thought. In this way, the Nimrod legend does not originally reflect purely biblical traditions. — So far Krausz.

We also note that the prehistoric, theological, and moral chapters of the Old Testament, its psalms, hymns, symbolic allegories, in short, all those parts that do not specifically deal with the history of the Palestinian Hebrew states (Judah and Israel) of the 1st millennium BC; research shows again and again that almost without exception they are ancient Sumerian legends, myths! cosmogonic images, religious ideas,

===== PAGE 334 =====

prayers, simple adaptations of parables, which, however, only reached the Hebrews indirectly, through the transmission of the Sumerian traditions of the Babylonians and the indigenous population of Canaan (Kramer: The Sumerians; among others: Die Wiege der Kultur; among others: Geschichte beginnt in Sumer; Schmökel: Das Land Sumer). We should especially highlight the fundamental work in this field of SN Kramer, a Sumerologist at the University of Jerusalem, who is today the most recognized expert on ancient Sumerian literature. He showed, for example, - among many other parallels - how the Bible included the statement that God created Eve from one of Adam's ribs. This is nothing else - writes Kramer - that the cuneiform sign "i" (, til") has the meanings of "rib, splinter, arrowhead", on the one hand, and "to be, to exist, to live, life, to conceive, ~~to give birth to a child~~ (The Sumer and drag) the Sumerian language. In Sumerian, the

Kramer's argument can be found in more detail in András Zakar's fundamental work "The Sumerian Beliefs and the Bible" (pp. 48-49), in which the author gives an excellent and detailed summary of the entire complex of issues.

In connection with Nimrod, we also mention that according to the latest findings of Bible research, in the Bible's view, all the enemies of the Hebrews were among the descendants of Noah, including Nimrod, bad ýý" -- Kam — who is none other than the greatest archenemy of the Semites: Ninurta (or Nimurta, or Nimruda) the Sumerian god of war (L. Deimel: Akk. — Sum. Glossar). H. Schmökel's brilliance is enormous) begins with a tank composition of 16 clay tablets, in which Enlil's son, Ninurta, the Sumerian In his book "Das Land Sumer" he describes the "King, evil weather, whose Glanz is gewaltig ist..." (the king, the evil weather, whose culture hero, leads the Sumerians' constant struggle against the surrounding barbarian peoples of the desert and mountains (id. m. p. 150).

In light of these data, we must certainly look at Kézai with a completely different eye, who — correcting the Bible! — clearly states that Nimrod (in his case Ménrórt or Nemproth) was not a descendant of Ham. He had to rely on very good sources (1. Krausz's opinion on the agreement between Berossos and Kézai), or on very well-known, stubborn and tenacious traditions of origin, in order to dare to correct the Bible in the middle of the Christian Middle Ages, because otherwise he would have had difficulty escaping this heresy with unscathed skin.

"You see my half with your own eyes": here the dog is buried. It does not take a very vivid imagination to imagine what undivided enthusiasm such perspectives could have aroused in German historical scholarship a hundred years ago, [676821 based on a solid methodological basis of the assumed Indo-Germanic cultural priority. In the light of this perspective, the very idea of gon-338

===== PAGE 335 =====

It was also intolerable that the Hungarians, who were called marauding hordes, could have had traditions of origin dating back to Babylonian times even in the 13th century. Moreover, these traditions seem to have been so strong that they could compete with the Bible itself. We do not think we are far wrong in suspecting that similar considerations may have played a role in the devaluation of our medieval chronicles.

About our Scythian connections

Let us now continue with the chapter of Fodor's book entitled "In the South Urals", in which — as we have seen — he discusses the former Scythian relations of the Hungarians, and lists very significant Scythian-Hungarian parallels in both the intellectual and material spheres. His concluding statement on page 157 is all the more surprising, when he apodictically states: "However, the uryinary relationship between the two populations — from which the theory of our "Scythian origin" emerged in the Middle Ages — has no scientific basis."

If we were Fodor, we would be wary of such contradictory statements. In support of this opinion, we can offer the following considerations.

Fodor himself writes that:

1. the Scythian-Sarmatian period relics of the ancient Hungarians cannot be separated from the legacy of the Scythians (Sarmatians);
2. territorially, we must look for the places of residence of the ancient Hungarians at that time in the Scythian-Sarmatian region;
3. We find not only similarities in material, artistic, and lifestyle aspects, but also a one and a half thousand-year tradition of some of the spiritual treasures of the Scythian-Sarmatian world among the conquering Hungarians.

From these Fodor's own statements, it almost involuntarily follows that:

4. We can assume with a probability that borders on certainty that the Scythian-era ancient Hungarians lived together, or at least lived in close proximity, with the Scythians and Sarmatians for several centuries;

5. As a result of this long coexistence, we can also state with an equally high degree of probability that there must have been ethnic relations between the Scythians and the ancient Hungarians, and a certain degree of ethnic mixing must have occurred during the period in question, which spanned centuries.

By this we do not mean to claim that the majority of Hungarians are ethnically descended from the Scythians, but rather to point out the obvious methodological unfoundedness of Fodor's loud statement.

339

===== PAGE 336 =====

Finally, it is important to know that:

6. The alleged Iranianess of the language and race of the Ascites and Sarmatians is merely one of the unfounded assumptions of 19th-century Indo-Germanic studies — primarily German linguistics and history — for which the competent experts have remained in debt to this day.

Regarding this point, we refer to the contradictory data already mentioned repeatedly in this volume.

However, we can also refer to the testimony of Hippocrates, the father of medicine, who undoubtedly describes the Scythians as having an Altaic-Mongoloid anthropological constitution (Corpus Hippocraticum: De aere, agus, locis). Perhaps even Fodor does not deny that Hippocrates had something to do with the human body and was able to describe its characteristic features realistically.

With these few observations, we have managed to show that the former relationship between the Scythians and the ancient Hungarians is not as definitively and clearly decided as Fodor would have his unsuspecting readers believe.

We can thus once again draw the conclusion that Fodor has no intention of adhering to the statement he made on page 28 ("We are not trying to conceal the difficulties anywhere...etc."). On the contrary, this commitment apparently only served to ensure that his readers would not even have a shadow of suspicion regarding the scientific validity of the various "statements" of the book. That is why we considered it absolutely necessary to show the deplorable one-sidedness of such "scientific results" that are applied step by step in the Finno-Ugric approach.

Thoughts on On the "Magna Hungaria" question and Julian's Volga Hungarians

The next chapter of Fodor's book is entitled "Magna Hungaria" (pp. 158-171). In this section, he devotes considerable space to the discussion of the metal plates covering the eyes and mouths found in the Kama region, Bashkiriya, the Ob region, and in Hungarian graves from the time of the conquest. His conclusion is that these metal plates are essential evidence that the ancient Hungarians lived in the region of present-day Bashkiriya for several centuries before the beginning of the 700s of the 1st millennium BC; and -- as he writes, our ancestors never left Magna Hungaria before that time. (p. 170)

The remaining pages of the chapter are devoted to considering whether the period between 400 and 300 BC and 600 AD was a

===== PAGE 337 =====

The Hungarians may have moved to Baskfriä, i.e. the region of Magna Hungaria, in the 1st century BC. He cannot decide definitively, but ~ after long deliberations ~ he indicates the most acceptable date as around 550 AD.

We delved into the study of the above-mentioned lines of thought with a pleasant surprise, because at first it seemed that Fodor was finally discussing an open historical question in a truly methodical manner, with a thorough examination of the pro- and contra-data and circumstances that could be counted. Unfortunately, our joy did not last long. It soon became clear that with the detailed, full-blown discussion of the two aforementioned topics, our author was only trying to create the appearance of scientific methodicality and conscientiousness.

By filling the entire chapter on "Magna Hungaria" with the analysis of these two questions, he creates the belief in the uninitiated reader that the Magna Hungaria problem of our prehistory revolves exclusively around these questions, that these are its Archimedean points. However, about the actual essence, that is, that the majority of our ancestors stayed in the Bashkir countryside in the middle of the 1st millennium AD, is itself only a much-debated assumption, and that the majority of our historians do not look for the Hungarians to settle in Bashkiriya during the period in question, but elsewhere, Fodor does not say a single word in the entire chapter.

Our opinion is amply confirmed by even a cursory review of the last 60 years of Hungarian prehistory literature, therefore we do not consider it necessary to provide further support here.

Fodor therefore — with a clever trick — once again misleads the bona fide reader interested in Hungarian prehistory by verbosely expounding on two secondary circumstances that are irrelevant to the fundamental question.

The question of the possible ancestral homeland in the Bashkiriya region, "Magna Hungaria", cannot of course be separated from the problem of the Eastern Hungarians found by Julian, since our historians who are looking for the pre-9th century settlements of the Hungarians in the area of present-day Bashkiriya all start from Julian's reports and base their arguments on them.

Julian's report undoubtedly indicates that somewhere in the middle Volga region we must certainly reckon with the presence of Hungarians, not only in the immediate time of Julian, in the 13th century, but also in the times before that. We do not wish to dispute this for a moment.

However, some questions arise in this regard,

First, how long have Julianus' Hungarians lived on the middle Volga? As Fodor's explanations in this regard clearly show, historical research from a Finno-Ugric perspective cannot provide any tangible evidence for this, apart from cautious probing.

341

===== PAGE 338 =====

Secondly: where did these Hungarians move from to the Volga bend? The doctrine of our exclusive Finno-Ugric origin can only answer this question with tentative hypotheses.

Some believe that our "Ugric" ancestors have lived in these regions since ancient times, originally somewhat further north, in the Kama region, and slowly moving south, in the 1st century AD.

They reached the middle Volga and Bashkiria regions by the second half of the 1st millennium AD. Others, such as Fodor, place our ancestors migrating from Western Siberia, from the Ob, Irtysh, and Tobol regions — also in the middle of the 1st millennium AD — to the Bashkiria regions between the Volga and Ural rivers. The sole basis for all these assumptions is that our language is Ural-Finno-Ugric, more specifically "Proto-Ugric" — i.e. Vogul-Osztyak ~ affinity, from which the shockingly one-sided categorical conclusion is drawn that the ancestors of the Hungarian ethnic group "language development" — to They can only come from the forest belt areas inhabited by the Finno-Ugric peoples. However, this 19th-century, exclusively linguistic — support beliefs in other directions so-called — study has not been able to find any contemporaneous historical sources or archaeological finds that can be clearly linked to the ancient Hungarians. The third question is to what extent can we consider the fact that this breakaway Hungarian group resided in the Middle Volga region — or indeed in Bashkiria — as evidence that the majority of Hungarians lived in the area around Bashkiria in the times before the 9th century?

According to contemporary sources, the Hungarians of the 9th century undoubtedly first appear in history in the southern Russian steppe, more specifically on the shores of the Sea of Azov, in the Kuban-Caucasus region and in southern Ukraine. Moreover, we have very serious indirect evidence that at least as early as the 1st century AD. Since the 5th century, we have had to look for the ancestors of the majority of the conquering Hungarians in this southern, steppe region, namely among the peoples and tribal alliances called Hun-Bulgar, Hunugur-Unugur-Onogur or Ogur-Ugric, living with them in very close historical, political, cultural and ethnic relations - only not under their later individual national name, the name "Magyar", but mainly under the name Hunugur-Onogur or Ogur-Ugric. This is also confirmed, with great probability, by our name still commonly used abroad today, which has the versions "Hungar", "Ungar", "Ongri", "Vengri", "Ugri".

On the other hand, our old name, fortunately preserved by Emperor Constantine the Purple-born, "sabartoi asphaloi", i.e. the "sabit" folk name, also points to the same areas, which - as we have already mentioned - can be considered a dead end in Hungarian prehistory research. Masudi's testimony about the real name of the Khazars "sabit" is also of primary importance in this connection. The 342 With the help of the ethnonym "sabit", we can trace the history of the region back to much earlier times than the 5th century AD.

===== PAGE 339 =====

The history, migrations, places of residence, historical, political, cultural and ethnic relations of this most important ethnic group of the Hungarians who conquered the country, which was a mixture of several groups (1. numerous data in this direction that arose during the presentation of Bartha's book). In short, the presence of Julianus' Hungarians in Bashkiria does not even remotely imply that the Hungarian homeland before the 9th century was there. All those who claim that the majority of Hungarians lived in the area around Bashkiria before the 9th century are in great contradiction with the contemporary sources. However, if we base our considerations on a single, detached Hungarian group that moved north, these contradictions will be resolved by themselves.

Numerous contemporary reports indicate that, starting at the end of the 4th century AD, various steppe peoples from southern Russia, the Caucasus-Kuban region, and the Maeotis region repeatedly penetrated northward in several waves. Moreover, in more than one case, we can also demonstrate these migrations archaeologically. Let us recall the Harina and then the Nevolino "Ugric" groups of the Lomovotovo culture, who appeared at the end of the 4th century AD and at the turn of the 5th/6th century in the Kama, Byelaila, Chusovaia regions, around Perm. Later, in the second half of the 7th century, after the collapse of the Onogur-Bulgarian empire of Kovrat, the Volga Bulgars, and with them some tribes of the Sabirs (Hungarians), moved north, roughly to the region of the Oka, Kama and the middle Volga.

We are convinced that the migration of the Julian Hungarians to the north should most likely be dated to the time of these latter movements of people, and should be viewed in close connection with them. The question remains open for now as to whether these Hungarians already called themselves "Magyar", or whether they were perhaps hiding under the name of Sabirs who migrated with the Volga Bulgarians.

In connection with this possibility, we refer to the much-mentioned "Kangár" attack, which Károlyi Czeglédy places with convincing argument around the 7th century, and with which Emperor Constantine mentions the division of the Hungarian tribes, but also their old "Sabartoi" name, in close connection. Gyula László also believes that the ancient Hungarians (then still called Sabirs), who settled in a long strip on the right bank of the Volga, may have been hit by the Kangár attack somewhere around the middle of their settlement areas, and while the tribes living further south moved towards the Caucasus, the northern wing may have remained in place, or perhaps only moved slightly further north. Although Gyula László sees the predecessors of the later conquering Hungarians in this northern wing, we do not consider this to be an irresolvable contradiction, because on the one hand it is very likely that at the end of the 880s, at the time of the great movement, some groups of Hungarians living along the middle Volga could have joined the Álmos tribe.

343
===== PAGE 340 =====

On the other hand, in terms of the essence, Gyula László says something different than what we ourselves argue: the majority of Hungarians did not live in the area of Bashkiria in the period before the 9th century, but much further south.

What Gyula László explains further regarding this mysterious "Kangar" attack is also very remarkable. According to him, the central tribes of the Volga settlement areas, who were directly affected by the attack, migrated west, and these would be the "griffes-Indás" late Avars or early Hungarians who appeared in our country around 670. The question involuntarily arises here as to what is the difference between this ominous "Kangar" attack and the events that ultimately led to the disintegration of the Onogur-Bulgarian empire of Kovrát, which are also unknown? We cannot rule out the possibility that this Kangar attack is closely connected with the fall of the Onogur-Bulgarian empire and the dispersion of its peoples. In this case, however, Emperor Constantine's statement would actually tell the story of the tragedy of Kovrát's sons and the dispersion of their peoples, only not under the name of Onogur-Bulgarian, but of "Sabartoi Asphaloi". We do not have the opportunity to elaborate on this idea now, but it seems certain that, given the numerous Hun-Bulgarian, Hungur-Onogur-Sabir and other relevant connections presented so far, it fits organically into the general picture of the historical circumstances of southeastern Europe in the 7th century AD.

As for Julianusz's Hungarians along the Volga and around Bashkiria, they naturally came in very handy for the believers in our exclusive Finno-Ugric origin. No wonder, since they were geographically quite close to the Ob-Ugrics, and so it was easy to argue that we came from there, as is also proven by the "Ugric origin" of our language.

However, the very conceivable possibility that the Hungarian equivalents of the Vogul and Ossetian languages, which are closer than the general Finno-Ugric parallels, could possibly originate from these Hungarians who had broken away to Bashkiria - an assumption that, given the cultural and historical conditions of the time, is much more likely than the postulation of a "nebulous common Proto-Ugric language" - well, this obvious possibility was not even considered, let alone raised.

That is why Fodor also migrates from the ~~separately-entranced Ugric~~ ^{separately-entranced Ugric} homeland, which he imagines to be on the banks of the Ob-Irtysh-Tobol, to Bashkiria around 550 AD. the origin of the metal plates covering the eyes and mouths in the graves of the Hungarian Conquest period

However, the 62014310 approach most often leads to contradictions in historical research. This happened to Fodor too, when he was still in his own

===== PAGE 341 =====

He cannot argue convincingly even in the detailed question carefully selected by him, which is probably intended as the most compelling evidence. We are thinking of the eye and mouth metal plates, from the places and circumstances of their occurrence he believes it can be concluded that "our ancestors did not leave Magna Hungaria before 700". From the fact that copies of the silver eye and mouth covers found in the graves from the time of the conquest around 900 also appear in the Kama, Bashkiria and Ob regions, and — as Fodor himself writes — "approximately between 700-800", we — unfortunately > can only draw the conclusion that Hungarians may have lived in the area of Bashkiria after 700-800 — and not before.

Incidentally, I would like to note: look how perfectly this data fits into our previously explained line of thought, namely that the Julian Hungarians most likely moved to the central Volga-Káma region at the end of the 600s after the collapse of the Onogur-Bulgarian empire.

However, here again, it is not primarily a question of Fodor's inability to think logically, but of the well-known "Finno-Ugric" practice of proof, when inconvenient data is simply omitted. This time, Fodor provides us with an excellent opportunity to present this "method" in detail with the help of a concrete example.

Specifically, we would like to say something about the metal eye and mouth plates in question, as well as the complete death masks made of metal, and more precisely, their origin.

Fodor deals with these in more detail not only in this book, but also in his article (Our Bulgarian-Turkish loanwords and archaeology) published in the volume "Studies of Hungarian Prehistory". He describes that these plaques occur in a smooth, simple form among the Ob-Ugric peoples, in Bashkiria and Hungary; larger ones, with the eyes and mouth cut out, were found in Bashkiria, along the Kama and in Hungary; finally, complete death masks, also with eye and mouth cut outs, were found only among the Volga Bulgarians and along the Kama. The age of the finds is between 700 and 1000 AD. From these circumstances of the finds, Fodor not only draws the previously described unreasonable conclusion, but also claims in the aforementioned article that the Ob parallels point to "a folk kinship between the Hungarians and the Ob-Ugric peoples."

First of all, if these plaques indicate "folk kinship", then why only in the Ob and why not in the Kama as well? Of course - even unspeakably - because of our special "Ugric" origin, among the western Finno-Ugric peoples of the Kama region, who, as is known, are only our more distant relatives, these plaques no longer "point" to "folk kinship", although they also occur in the same form as in Bashkiria or Hungary (larger, with cutouts; 1. first),

345
===== PAGE 342 =====

Másodszt: since both simple plate-like and elaborate masks occur in Hungary, Fodor's conclusion - here too we can safely say: "of course" - is that this undoubtedly means that when this custom developed among them, they were in contact with both populations."

So while the Ob-Ugric parallel is a sign of "folk kinship", the Kama and Volga-Bulgarian correspondences are merely borrowed in our country. That is, Fodor explains his evidence again afterwards - as he did with the anthropological constitution of the Andronovo "Ugrians" - because later he himself obviously noticed on what shaky foundations his above statement is based, only about the "folk" kinship of the Ob-Ugric - Hungarian "flat-topped" people, while at the same time he is silent about the other correspondences.

So Fodor dug up everything from the arsenal of the Finno-Ugric perspective: "Ugric" folk kinship, Hungarian borrowings from the Volga Bulgars and the Kama Finno-Ugrics alike, but he did not think that this burial custom might have come from us, both here and there, primarily to the Kama Finno-Ugrics and Ob-Ugrics — which is much more likely than Fodor's views. Not least because Fodor himself states in his aforementioned article that when the Volga Bulgars moved north around 700, Sabir and Hungarian groups also appeared with them at the Volga bend.

It is already clear from what has been said so far (including from Fodor's article itself!) that the plates in question, which appeared in the Volga-Kama-Ural-Ob regions in the 8th-9th centuries, were in all likelihood introduced to these areas by the Volga Onogur-Bulgarians and the Sabirs and Hungarians who arrived with them. They really did "stand" with both the Kamians and in relation to both populations the Obians, as is confirmed by several contemporary sources, reporting on their extensive fur trade, which, by the way, is considered a characteristic occupation

— is already mentioned by Jordanes in the 6th century, miraculously accurately in describing the Hun-descendant people whom he sometimes called “Hunugur” (onogur!), sometimes “Sabir”. Certainly, such historical “coincidences” are extremely interesting, especially when they occur in series. But the Sabir traditions of the Ob-Ugric people also prove the same. The Volga Bulgarians who moved north and the Sabirs (Hungarians) who arrived with them soon took control of the fur trade; according to the indications, the Bulgarians in the western, European parts, and the Sabirs in the north-western Siberian areas, i.e. in the area where the Ob-Ugric people settled. In addition to the Vogul-Ostiak traditions concerning the Sabirs, the Tatars of the Tomsk region testify to this. However, Fodor does not even mention these circumstances as a possibility. Again, he has a good reason for his silence. For if he were “Siberian” legends, but the body itself to speak about these questions, he would also “Siberia” is also called (I, Patkanov id, m.). have to look into where this burial ground ultimately comes from.

===== PAGE 343 =====

It is better to avoid such broader horizons in the first place, however, because they could easily provide very embarrassing surprises for researchers of our Finno-Ugric origin with a perspective adapted to the tundra. This is no different in the case of the metal plates covering the eyes and mouth in question and the death masks.

The ancient homeland of these exclusively “Ugric” - Hungarian folk kinship plaques and masks is the Parthian Empire (Dura-Europos) and Sfria (Emesa, Baalbek, Tortosa), where they appear around AD, in both forms (plaques and masks). From there they spread not long after to the Cimmerian Bosphorus region - today's Kerch. In one of the royal tombs of Kerch (the ancient Pantikapaion), a complete death mask dating from the 3rd century AD was found, and in the necropolises of other Bosphorus cities, as well as the former Chersonesos and the capital of the Crimean Scythian state, Neapolis Skythica, gold plaques covering the eyes and mouth were found in several tombs from the 2nd-4th centuries AD. century (Gajdukevich: Das Bosporanische Reich; p. 443, note 215 with the relevant literature).

Moreover, this burial custom can be traced back to even earlier times in the Pontic Scythian cultural circle: a Mongoloid-type gold death mask from the 4th century BC was discovered near Olbia (G. Charrière: Die Kunst der Skythen; Figure 80, caption on page 233). We could ask Fodor whether these Parthian and Scythian tablets also allow us to conclude that they are “ethnically related”? Or do the rules of the game no longer apply south of the 50th degree of latitude?

Perhaps we can help our author in this dilemma. We would suggest that he feel free to include the Parthian and Scythian plates and masks in his next work, and explain them as a manifestation of Vogul culture. All that is needed for this is to place one or two Obi-Ugric cemeteries with such metal plates a scant 800-1000 years earlier. He already has experience in this: let us recall the cultural influences of the East Minarians, which he dated to around 4000 BC, presumably based on linguistic-historical considerations. Continuing this line of thought, it is possible to include Mycenaean gold masks from the middle “pre-Ural” settlements, and the of the 2nd millennium BC in this group, together with their contemporary or slightly earlier Caucasian and Kuban-Pontic parallels. All of these could easily be signs of the cultural influence of the Andronovo “Ugrians” of Fodor, who produced “high-quality and artistically executed” bronzes -- which would roughly prove the continental-scale, world-historically significant cultural role of the Voguls since the 2nd millennium BC.

We have not written these lines with the intention of mocking. We merely wanted to present the point of view -- we will not deny it, a little exaggeratedly -- 347

===== PAGE 344 =====

which is pervasive in Fodor's book, from beginning to end: everywhere he claims to uncover “Uralians”, “Ugrians”, he tries to construct a history for such insignificant fragments of people who never had a folk, ethnic history, and moreover, all this in such ancient times, when the Uralic race had not yet developed anthropologically. Considering our above argument against this background, we believe that, in essence, it does not differ that much from the reasoning of Finno-Ugric historiography. Let us think of Fodor's “Ugrians” of Andronovo, who — even if they did not make all the Andronovo bronzes (Fodor graciously allows this on page 109) — but in the fact that these products reached the Finno-Ugric population of far Eastern Europe after a migration of several thousand kilometers, only the Ugrians could have played a significant role. Their traders brought them along the trade route that had been trodden since ancient times, leading through the Urals...” (page 109). Moreover, according to the assumption of the Soviet researcher Yu. A. Krasnov, Fodor also notes the first sprouts of animal husbandry around 2000 BC, which is proven by the Asian origin of the wheat and barley varieties widespread here, . On this route, the Ugrians not only conveyed the products of advanced metalworking to their Eastern European relatives, but also the knowledge of agriculture and as well as some animal breeds. (page 109) Now, can anyone say after all this that we were just being irresponsible? Returning to the issue of plaques and death masks, we can rightly say that the finds from Parthian Mesopotamia and Syria, as well as the Crimean and Pontic eye and mouth covering plaques and death masks from 1-2 centuries later, and their appearance around 700 AD in the Volga-Kama-Ob region, clearly indicate that this custom appeared in these northern regions with the Onogur-Bulgarians, Sabirs and Hungarians who migrated north from the Pontic-Caucasian region around 700. According to recent research, the Bulgars and Sabirs undoubtedly migrated from the former Parthian Empire and the later Hephthalite-Hun Empire through Northern Iran, Dagestan and the Caucasus, first to the Don-Volga-Pontus region, and then around 700 to the middle Volga, Kama and present-day Bashkiria.

Although we have no direct data about the Hungarians -- calling them “Magyar” -- that they participated in these national movements, several circumstances, however, speak with a probability approaching certainty that we must look for the ancestors of the Hungarians, starting from the 400s AD, in close connection with the Onogur-Bulgarians and the Sabirs, among these peoples. This is confirmed - as we have already mentioned - by our foreign name “Hungar”, and “Ungar”, the news of Emperor Constantine about our old name “Sabartoi-Asphaloi”, but also by Altheim's very thoughtful, reasonable conclusion

348
===== PAGE 345 =====

also that one of the three “Turkic” peoples mentioned by Patriarch Michael the Syrian, who arrived from Transcaucasia to the Volga-Don-Kuban-Pontus region around 585, following the Sabirs and Avars, can be identified with the Hungarians (we would rather say: with one of the groups of the later Hungarian tribal confederation). There is therefore no particular problem, the data are quite clear - indeed much more specific than we could have guessed from this vague period -, the path of spread can be outlined with great probability. Under such circumstances, however, we can speak with very little justification that the occurrences of eye and mouth plates in the Obi-Ugric and Hungarian periods of the Hungarian conquest - arbitrarily taken out of their other contexts - would point to a folk relationship in just these two cases.” Even less so, because the simple and more elaborate metal plates only suggest that their owners belonged to the poorer or wealthier classes. It is therefore evident that among the primitive, poor Obi-Ugrians we find only the cheapest version. The Saltovo culture and the Alans Let us now look at the next chapter, “Levedia” (pp. 172-195). On page 178, we learn from Fodor that the northern, long-headed-Europid ethnic group of the Saltovo (or Saltovo-Mayacki) culture is considered by researchers to be the Alans who fled from the Caucasus to the north, to the Don-Donetsk region, to escape the Arab attacks of the 730s. It was mainly their developed agriculture and goldsmithing that were adopted by the Hungarians who also moved to this region from Magna Hungaria — i.e. from the area of Bashkiria — around 750, he writes. Then he tries to bring our words, which are considered to be Bulgarian-Turkish loanwords referring to developed agriculture, into line with the previous assumption by stating that the Alans must have become Bulgarian in the Bulgarian environment near the Don-Donetsk River, and as a result the Hungarians adopted Bulgarian words from them, not Alan words. It is worth observing Fodor's fascinating logic again. In the 730s, the Alans who fled to the Don had already adopted the Bulgarian language in barely twenty years, by the time the Hungarians arrived in 750 — presumably by organizing crash language courses to make them compatible with Fodor's —, of course, only for the purpose of so-called “Bulgarian-Turkish loanwords” Alan cultural influences. However, this distortion has very deep, theoretical and historical reasons. Let us remember when Fodor had bronzes of “excellent quality and artistic execution” manufactured in the middle of the 2nd millennium BC, somewhere in the Tomsk and Omsk regions (pages 108-109 of his book). Well, now we can take our author at his word: we, the Ugrians, need

349
===== PAGE 346 =====

Is it possible that the descendants of these same Ugrians - about whom you yourself write that they later also constantly lived within the scope of cultures that had developed metalwork (Scythian, Sarmatian region) - would suddenly search so feverishly, even at the cost of illogicalities, for some foreign people from whom these late Ugrians could re-learn the same metalwork that their ancestors had already cultivated at such a high level more than two millennia before? Here we can once again tangibly feel the crude manifestation of the distorted historical perspective of the Finno-Ugric school, which we already mentioned in our comments on Fodor's Andronovo Ugric bronze artists. At the same time, however, we are indebted to Fodor for providing us with such compelling evidence. We could not have expected more to justify our criticism than the fact that the same author, in the same work, disputes the very possibility of the existence of their own metalwork by the Hungarians of the 8th century AD, who 75 pages earlier counted without further ado, as a matter of course, on the “Ugrians” of the mid-2nd millennium BC - whose mere existence could not be proven in those times - with highly developed bronze art of a high technical and artistic standard.

This view has unfortunately become almost ineradicable in our historical science. We can experience step by step that in cases where source data or any other circumstance is found that would be positive and beneficial for the history of our people, “source criticism” immediately occurs, the twisting, twisting, and misinterpretation begins, and it continues until the data in question has been taken apart with the help of sly hair-splitting. We do not want to generalize, but we can say with a clear conscience that this view prevails in the vocal circles of our historical research.

We would like to make a few more comments regarding the Alans for this chapter. As we have seen, Fodor attributes the highly developed agriculture and metalworking of the Saltov-Mayacki culture primarily to these Alans, and explains it with their cultural influences. However, in this case too we would like to apply the principle of “equal measure”. And strictly based on the same methodological and source-critical foundations, just as our historiography, for example, rejects without selection all sources from the same period or close in time that call the conquering Hungarians Avars, Huns, or even Parthians, Scythians. Our scholars say that these names prove nothing, because each of them is a so-called “topos”, i.e. an old, historical role in a certain geographical area.

350

===== PAGE 347 =====

the deliberate transfer of the names of peoples who came to the area to peoples who later appeared in the same areas. Well, if this definition is valid for the Scythians, Huns, and Avars, why wouldn't it be equally applicable to the Lower-Ass people, who appeared in contemporary sources in Eastern Europe, more specifically in the Caspian-Caucasus-Don region, for centuries before the appearance of the Huns. In line with this line of thought, we should examine very carefully any source that continues to speak of the Alans after the Hunnic times. It is well known that the Huns shattered the Alan-Aes tribal alliance and invaded their country. The Alan-Aes people themselves split into at least three, and possibly more, parts. An insignificant fraction of them moved up to the more protected valleys of the Caucasus, while the other two groups — the bulk of the people — migrated from their former places of residence. One joined the Huns and, mixing with them, moved further west, while the other fled from the Huns in the company of the Western Goths and Vandals. The latter escaped all the way to Gaul and Hispania, and one of their divisions even crossed over to Africa with the Vandals.

Given these historical data, it is obvious that after the events outlined above, we can probably only count on insignificant Alan remnants in Eastern Europe. In any case, much fewer than the Huns remained after Attila's death.

Our historians take it for granted that the Huns, after the collapse of their empire, crumbled within a short time and merged with the surrounding and newly arrived peoples; so much so that 100-200 years later their name only lived on as a topos. It is all the more surprising, however, that these same scholars consider it just as natural that the handful of Alan fragments survived all the bloodshed, migrations, the rise and fall of empires fresh and unbroken, and even acted as a leavening agent for several centuries in the development of the culture of the Bulgarians, Khazars, and Hungarians.

We do not wish to dispute the presence of Alanic elements in Eastern Europe - primarily in the Caucasus region - in the second half of the 1st millennium AD. We consider them to be such insignificant factors that we reject the possibility that these fragments played any significant, tangible historical or cultural role in the region in question. In support of this opinion, it is sufficient to refer to the fact that the Ossetians, considered to be the descendants of the Alans, numbered barely 200,000 in the second half of the last century, while the Circassians, for example - despite their continuous wars with the Russians - numbered almost a million (Révai Lexikon). However, we can read very little about the Circassians, who were more than twice as numerous in the former for-351

===== PAGE 348 =====

rasok; they are mentioned only once or twice by the Byzantines, and even then only in connection with insignificant, local events. That is, despite their much larger numbers, they did not play a significant historical role at the time. Yet their level of education was not lower than that of the Ossetians — neither in the recent past nor in ancient times.

After considering these circumstances, we came to the conclusion that in a significant proportion of sources mentioning Alans after the Hunnic era, the name Alan — similarly to the 10001 example — can also be considered only a topos, a memory of the ancient, pre-Hunnic times Alan-Ace tribal alliance around the Caspian-Caucasus region. However, we must also know that the Ossetians — and with them the Alans, who were considered their ancestors — were also inflated from historical mice into elephants by 19th-century Indo-Germanic studies. At the same time, they were seen as having a grand, glorious historical past, naturally befitting the honorable role, the last, unadulterated representatives of "pure Proto-Indo-Germanic" (l. Révai Lexikon; under the heading "Ossét"). For this, the spiced with appropriate Indo-Germanic cultural influences. The signs indicate that the repeated emergence of the Alans in Hungarian prehistory and linguistics is still a fossilized memory of this former Indo-Germanic glory.

We can observe very beautiful, museum-worthy specimens of these fossil remains — as follows — in our so-called Ossetian loanwords. These were first cleaned of the dust of centuries by Bernát Munkácsi.

It will be instructive to take a look at Munkácsi's scientific method, based on his original article (Alanische Sprachdenkmäler im ungarischen Wortschatze; Keleti Szemle, V/3. 1904). In this paper, Munkácsi derives about thirty Hungarian words from Ossetian, i.e. ultimately from the Alanian language. Of these, he notes that they have no equivalent in any other Iranian language except Ossetian.

We can assume after this — of course only with our own untrained reasoning — that at least these nine words have doubts about the direction of the borrowing. Of course they do! He records all nine, without any comment, as loanwords in Hungarian.

However, anyone who does not mind the effort and reads the 1904 issue of Keleti Szemle from beginning to end will realize that it was not only our little chicken who had the idea that if the nine words above are not found in any other Iranian language besides Ossetian, then they could perhaps be Hungarian loanwords in Ossetian. We were pleased to read in the same 1904 issue of Keleti Szemle, which contains literary reviews, that one of our renowned linguists also thought so. Holger

352

===== PAGE 349 =====

This linguist criticizes the article "Armenier und die Nachbarsprachen" by Danish linguist professor Pedersen - in which Pedersen attempted to show Armenian loanwords ýj- in Turkish - with the argument that since many Turkish words considered by Pedersen to be Armenian loanwords only occur in Armenian, but not in other Indo-Germanic languages, the borrowing could only have occurred in the reverse direction.

This outstanding Hungarian linguist was called Bernát Munkácsi. See, it is not a wasted effort to browse through such old folios. Very often, we can observe in them that our linguistics (and our historical science) has not made a similar statement: "Our linguistics has undoubtedly established..." — let's say — that our language is a derivative of the "Uralic" proto- the birth of "solid results", which then only usually occur in the later literature in this way: language. The various chains of hypotheses of our linguistics and historical science, in this case the Alanian cultural influences, are mostly based on such "certain results".

However, there are other unclear questions regarding the Alans. As we have seen, Fodor quite naturally, self-evidently calls them Iranians. To his credit, he is not alone in this opinion. As they say, "in accordance with the current state of science," this is the generally accepted view. Nevertheless, we dare to question this claim. The Iranian origin of the Alans is not as clear-cut as one might think given the unanimous opinion of international research.

To shed more light on this question, we should recall Strabo's statement, mentioned in connection with Haussig's work, that towards the end of the 2nd century BC — around 130-128 — the "Asi" (or "Asiani"), "Pasiani" etc. peoples from the Aral Sea region pushed into Bactria. We have already referred there to the striking similarity of these people's names with the names of the Uz and Pechenegs who appeared later. We have mentioned that in Chinese sources the Turks appear as a branch of the Huns, and the Chinese give their people's names in the phonetic form "a-si- na" (Liu Mau-Tsai: id. m.). We have also seen that in the company of (Haussig: id. m.). The "As" and the "Alan" peoples are always mentioned together in contemporary sources, which is . Another group of people appeared shortly afterwards in the area around Lake Caspian and in the foothills of the Caucasus, the Alans. why Haussig believes that the two peoples formed a tribal alliance. Moreover, we have shown that the name "Ossetian" is nothing more than the name of the Abbeli people, which confirms our suspicion that the names "Asi" and "Pasiani" are the 353 The plural form of the word "as" is formed with the plural suffix "-t", i.e. "SZOK", "úz" and "besenyő" (pacinak) may contain folk names, so pay attention

===== PAGE 350 =====

This is also worthily confirmed by the statement of the 10th century Persian-Arab author Biruni that in his time the Pechenegs lived in the immediate vicinity of Khovarezm, in the company of Alans, who, however, could have been other than the Uz (Oguz), to whom the Pechenegs actually as" (Altheim-Stiehl: Geschichte Mittelasiens im Altertum). We note in relation to this source data that these "as" are not belonged. They could not have been Alans, especially since in the 10th century only insignificant Alan fragments were alive, also hiding in the foothills of the Caucasus.

Closely related to this are the "u-sun" or "wu-sun" people from the area of Turkmenistan, mentioned by Chinese sources in the 2nd century BC. They lived in the immediate vicinity of the "Kang-kü" empire, and the name of their ruler - or rather his title - was, according to the Chinese, "kun-bok" ("kun beg"). So, we can date the presence of all three main tribal groups of the later — in a broader sense — "Uz", or "Oguz-Turk" peoples in the vicinity of Turkmenistan as early as the 2nd century BC: the "Asi" people are in all probability identical with the later Uz, the "Pasiani" with the Pechenegs (whose old name, "Kangar", however, is documented by the name of the "Kang-kü" empire), and the "Wusun" people of the Chinese — as evidenced by the princely name "Kun-bok" — with the Cumans. At the same time, the old name "A-si-na" of the later Turks in Chinese sources can only correspond to the "Asiani" people of the contemporaneous Western, Greco-Roman chieftains. All the more so because — as we have already mentioned — the ancestral homeland of the so-called Altai peoples was more than likely in Turkmenistan.

In our opinion, these data do not allow us to speak of the Alans (as) simply as Iranians, and on the other hand, they make Fodor's assumptions about massive Alan cultural influences in the area of the Saltov-Mayacki culture that developed in the 8th century very unstable. It should also be mentioned that Fodor not only repeatedly speaks of Alan cultural influences in the northern areas of the Saltov-Mayacki culture, but also derives Bulgarian culture itself entirely from the Alans (in his present book and in his article published in Magy. Östör. studiuns). In these lines, we have shown once again how necessary it would be to thoroughly reexamine the various beliefs of Indo-Germanic studies, originating in the 19th century, before we base conclusions on them in our research into prehistory.

The events of the conquest and historiography from a Finno-Ugric perspective However, let us turn the pages further in Fodor's book. We have arrived at the conquest of the country (pp. 231-236). The pages of this chapter are also very characteristic. They illuminate more brightly than the sun the confusion into which our historical science has maneuvered itself. They could not even agree on the date of the conquest, let alone its

354

===== PAGE 351 =====

There have been almost as many different opinions on the question of how, why, and more detailed circumstances as there have been on history. Hóman (Hungarian History): The Bulgarian War took place in 895, and the conquest of the country took place in the autumn of the same year.

Bartha (The 9th-10th century Hungarian tyrasadalom); The Bulgarian War 68 The conquest took place in 896.
Györfly (Conquest, settlement, adventures): The Bulgarian war was in 894, Árpád arrived at Verecké in the spring of 895.
In this work, Fodor almost completely agrees with Györfly's opinion.
Hóman's views were criticized in detail and thoroughly by Padányi in his book "Dentumagyarria" (1. ibidem). However, we must state that Hóman's chronology best corresponds to the real situation that can be deduced from contemporary sources. — We have already had the opportunity to deal with Bartha's claims, so it is not necessary to dwell on them again now.
However, let us look at Györfly and Fodor's ideas about the conquest.
When looking at the general picture of the conquest of Hungary by these two researchers, what is most striking is their barely concealed effort to obtain all kinds of information from the conquering Hungarians.
The possibility of rational planning is denied. Despite Padányi's thorough analyses and the emphatic acceptance of their results by Gyula László, they still want to separate the Bulgarian war from the conquest and, for this purpose, they date Levente's campaign to 894. There is little point in listing Padányi's evidence now, since anyone can easily look it up. On the other hand, in Fodorék's eyes, Padányi is "fantast" anyway — although the question arises here: would Gyula László also be "fantast"? — Let us therefore turn to an "objective, distant, unbiased" foreign chronicler and get acquainted with the description of the Byzantine-Bulgarian war of 894-96 in the work of the most competent researcher, the Greek historian AD Zakyntinos, entitled "Byzantinische Geschichte". He will also give us an answer as to who represents "fantast" views on this issue.

According to Zakyntinos, Simeon declared war on Byzantium in 894. In the first phase of the war, he invaded Thrace and defeated the Byzantine army there. Leo the Wise then asked for help from the Hungarians, recalled the famous general Nikephoros Phocas from the southern front, appointed him commander-in-chief of the Balkan front, and instructed him to organize a new army. After the preparations were completed, they launched a concentric attack against Simeon together with the Hungarians. The Hungarians routed Simeon's army, and he
He initiated peace negotiations. He deliberately delayed the negotiations until he managed to convince the Pechenegs to join him. Then, together with them, he attacked and defeated the Hungarians. After the defeat, he became much more demanding in the peace negotiations, and soon
355
===== PAGE 352 =====

He still broke them and attacked again, in 896 Bulgarophygonnâi won a brilliant victory over the Byzantine troops (id. m. pp. 145-146).
The events of the Bulgarian-Byzantine war of 894-96 are described in the same way by another internationally renowned Byzantine expert, G. Ostrogorsky, in his book "Geschichte des byzantinischen Staates" on pages 208-209 of his work, with the only difference being that he also mentions the repeated victory of the Hungarians over Simeon's armies in two successive battles,
From the two most authoritative Byzantine historical works of recent times, it can therefore be clearly stated that the Bulgarian-Greek war consisted of three phases. In the first -- in 894 -- Simeon defeated the Greeks and achieved significant territorial gains in Thrace. In the second -- in 895 + the combined Hungarian-Greek counterattack took place, and according to the above data, with a probability approaching certainty in the spring of 895. The war was interrupted in the winter even during the Turkish times, let alone in the 9th century. But the conclusion of the Hungarian alliance, the recall of Phokas and the organization of the new army could not have happened overnight. Such an event requires long months.

This was followed by Simeon's request for peace, then his Pecheneg alliance, and finally his victory over Levente's army. This battle could not have been earlier than the summer-autumn period of 895. In this case, too, the conclusion of the alliance and the launch of the war necessarily took a considerable amount of time, because on the one hand, the Pechenegs were not exactly numerous from Bulgaria, and on the other hand, contemporary sources clearly state that . . . They lived "under gardens", the Hungarians once again completely destroyed the Bulgarian army, and Simeon himself was only able to escape from them to the castle of Silistra with difficulty. Simeon therefore had to raise a new army from scratch, gathering the scattered remnants from the entire territory of the country little by little. All this again was not a matter of weeks. Zakyntinos also emphasizes that Simeon dragged out and postponed the peace negotiations until he managed to reach an agreement with the Pechenegs.

And by the summer of 895 — even according to Fodor (i.e. Györfly) — the Hungarian tribes must have been on their way to the Carpathian Basin for a long time, otherwise they would not have been able to occupy their new settlements in the autumn of the year.
All data therefore indicate that the people of the Eteleköz settlements set off towards their new homeland in the spring of 895, following Árpád's army, meaning that the conquest was a pre-planned undertaking: Árpád's main army attacked the Bulgarian provinces of Transylvania and Trans-Tisza simultaneously with Levente's Bulgarian campaign.
It also suggests that Simeon only dared to open the third phase of the war against Byzantium in 896, when he had already secured his back against the Hungarians - apparently by concluding a peace treaty. Although we have no source data on this peace treaty, the evidence speaks for itself.
356
===== PAGE 353 =====

historical facts. From the consequences it can be clearly established that Simeon paid the price of peace: he renounced the 68 Bulgarian provinces of Transylvania and Tiszatul.
Despite the fact that his Pecheneg alliance resulted in a significant increase in power for him.
Even with the greatest goodwill, we cannot understand the need to forcefully deprive the conquering Hungarians and Árpád of even the most elementary military knowledge and leadership skills, when the masterfully planned and executed enterprise of the conquest, as well as the serial victories of the subsequent Western campaigns, which can be verified by a mass of contemporary sources, undoubtedly prove that the conquering Hungarians were far superior to the peoples of Western Europe in military tactics and strategy.

In this regard, we recall the map of the Tatar invasions found in the 2nd volume of Hóman-Szekft's "Hungarian History", which, looking at it, from Batu's (or Subutai's) continental-scale encircling military movement, from the clockwork-like precision, precise, coordinated execution of the march from Poland to the Iron Gate - let's note the dates of the capture of individual cities, as well as the battles of Moh and Liegnitz! - perhaps even Fodor could notice that these "wild barbarians" of the steppe, if nothing else, at least knew the strategy, and to such a high degree that, counting the northern wing of the Horde - we can precisely recognize the plan of Árpád's encircling military movement to occupy the Carpathian Basin. Moreover, it is obvious that Árpád did not need a northern wing because the main enemy, Bulgaria, lay to the southwest — as seen from . . . "The civilized West" was first reached during the time of Napoleon. However, it can also be determined from this map that — Eteleköz — and did not extend further than the Danube-Tisza border. Levente's march to the Transdanubia and Árpád's march to Verecké thus took Simeon's empire, and more closely its provinces in the Carpathian Basin, into a perfect grip, and as it were cut off the latter from the body of the mother country.

All of this was emphasized emphatically by Mihály Ferdinándy in 1941 (We Hungarians), long before Padányi and Gyula László.
We consider it unnecessary to discuss in detail the other evidence that proves the superior, sovereign military leadership abilities of the conquering Hungarians: their repeated winterings in the heart of Western Europe, in the middle of enemy countries, their thorough geographical knowledge of the planned march and operational areas, etc., because such things cannot be achieved by historians with Fodor's angular thinking anyway. Apart from one or two insignificant, forced corrections, which can no longer be ignored with our current knowledge, they are essentially still rushing into the country today and
.. They imagine the conquering Hungarians as "robber herds", which

357
===== PAGE 354 =====
"They have scattered the bloodied land behind the backs of countless powerless peoples," as Rössler so beautifully and eloquently put it a hundred years ago. We can say: they really learned the lesson taught by 19th century Indo-Germanic studies well, and even a hundred years later they still say it,
While we are on this subject, it would be a good idea to take a closer look at the attacks that Schlözer, Rössler and their associates have used on the Hungarian chronicles. For example, Schlözer says of Anonymous: "the crazy chatter of a foolish friend" (Sebestyen L., in defense of Simon Kézai).
Or Rössler about the Hungarian chronicles in general: they must be thrown into the fire" (Gy. Rohonyi: The History of the Conquest).
Why this fierce, hateful voice? It will soon become clear if you continue browsing. Schlözer: "Transylvania was not part of the Hungarian state before Saint Stephen, a land of dubious ownership until the Saxons moved in." Then he writes about the Székelys: "...descendants of the Akuns after 1089, but in any case after the Transylvanian Saxons moved in" (emphasis added).
Rössler on Anonymous: "... fabricated and violently distorted, where he could have helped the Hungarians to glory... To be banished from the hands of historical investigators... It must have seemed unbearable to him to imagine his ancestors as marauding herds rushing into the country after the German chronicles were written down...., etc. (L. above) ... No, hating the idea that they had ever come into their possession through arbitrariness and injustice (emphasis added), he transferred his legal concepts to the time of his ancestors, and tried to shift the injustice committed at least many generations back onto their completely unknown ancestors, the Huns. Now the Hungarians appear to be seeking their rights: they are not conquering, but only reclaiming what they have lost."

The above quotes were written by László Sebestyén. We took it from his discussion paper "In Defense of Simon's Hands".
Certainly, these are all very scientific, and especially extremely objective opinions, in which there is bias, or perhaps pan-Germanic chauvinism is creeping into our country, the Germans supported the territory of Hungary, which is why they were excellently suited to take deep, thriving roots in Hungarian historical research today . . . We cannot detect even the faintest traces of his efforts to assert his "historical rights," (J. Györfly Gy.: Our Chronicles and the Hungarian Ancient History).

At least Schlözer still had enough openness to say without mincing words: Transylvania belongs to the Saxons, because they were the first settlers there, Rössler was much more concise.
Similar to the proverbial thief, who, merging into the crowd pursuing him, cries out most loudly, "seize him!"; Anonymous uses the Germans' supposed right to Pannonia, which originated from the conquests of Charlemagne, § the
358
===== PAGE 355 =====
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throughout the Middle Ages, but also in the 19th and even 20th centuries - let us just remember the Hitler era! ~, he justifies it by saying that
that the Hungarians have nothing to do with the Huns or Avars, so the claim of Charles the Great is older than that of the Hungarians, who only came into possession through arbitrariness and | | injustice"
"This is what the game was about in 19th century Pan-Germanic circles, nothing else. That is why the Hun-Avar-Hungarian relations had to be denied at all costs, and our chronicles and the numerous other contemporary source works that called the Hungarians Avars or Huns were to be declared to be the inventions of scholarly chroniclers. We have seen that Hunfalvy was even

He did not hesitate to expose the Hephthalite-Hun and Avar "Eviláth" is a biblical borrowing, but the real context, the traditions of Kéza - the Avars also came to Europe from the "Eviláth land", from the Hephthalite-Hun empire. Returning to the question of the method of the conquest, we would also mention that, for example, the alliance with the German king Arnulf against the Svatopluk in 892 also clearly indicates that the Árpád had carefully planned the conquest for many years and — as we would say today — diplomatically prepared it. In stark contrast to all these clear data, Fodor — or rather Györfy — wants us to believe that Árpád entered the Carpathian Basin at Verecké in the spring of 895 and began the conquest, while the people remained quietly seated in their camps in Etelek. Simeon, on learning of the absence of Árpád and the main army, formed an alliance with the Pechenegs with phenomenal speed, and without delay they jointly attacked the defenseless people who had remained at home. We really don't know what to think of this setup. In the past, this kind of thing was called a "scientist's theory" concocted at a desk. However, this analogy doesn't quite fit the Fodorék people. For example, instead of a desk, the suggestive effect of the evening news on television is clearly noticeable. Just as today we learn from the announcer that there was a tsunami in Honolulu that day, they imagine Simeon to have immediately learned not only about Árpád's invasion of Verecke, but also about the fact that in 893 the Arabs had defeated the Karluks in Central Asia, who attacked the Uzbeks, who in turn chased the Pechenegs westward, as a result of which the appearance of the latter on the eastern banks of the Dnieper could be expected any day now. He therefore quickly sent his envoys to conclude an alliance with the soon-to-arrive

with Pechenegs. Otherwise, the ideas of Fodor and his colleagues about the conquest collapse on themselves. Quite simply for chronological reasons. If Simeon only sent envoys to the Pechenegs after he had heard about Árpád's campaign in the Carpathian Basin, given the communications conditions of that time, 359 ===== PAGE 356 ===== the earliest he could have known, then the joint Bulgarian-Beshen6 migration could only have occurred much later, at most in the late summer or early autumn of 895, when - even according to Fodor and Györfy4 - the people of the Hungarian tribes were already in the Carpathian Basin, or had almost completed their migration. However, by doing so, they themselves indirectly admit that the people did not remain in Etelek, but followed them at a certain distance. You can't just flee from Ukraine to the Carpathian Basin in a few weeks. So, even starting from the Fodor-Györfy views, and arranging them in a careful and reasonable chronological order, we would have arrived at the same date that we have already clearly determined from the data on the course of the Greco-Bulgarian war: the Pecheneg-Bulgarian attack simply could not have taken place earlier than the summer of 895, early autumn. Although we cannot have much hope that reasonable conclusions will be heard by representatives of Fodor's view of history, we will nevertheless attempt to justify our opinion in detail.

Let's start from Fodor's presentation. 1. According to him, the root cause of the Pecheneg attack was the Arab victory in Turkmenistan in 893 over the Uzbeks. (More precisely, over a related tribal group of the Uzbeks, the Karluks; 1. Hóman-Szekfy: Hungarian History, Volume I) Following Padányi (Dentumagyarja), we ask: is it conceivable that an event in Turkmenistan could spread to the Carpathians in less than two years? = Certainly not. In the spring of 895, Árpád entered the Carpathian Basin with his main army. Simeon learned of this, allied himself with the Pechenegs who had just appeared on the Dnieper, and together with them attacked the settlements of the people who remained in the Etelek. The people fled to the Carpathian Basin after Árpád. However, the losses were not too heavy, because the The troops guarding the accommodation managed to partially cover the escape (Fodor: ism. m. pp. 233—234). Let us look at the individual moments in order. In the spring of 895, Árpád marches into the Carpathian Basin. Simeon learns of this, and obviously after the event and not before. (Or did he have spies at the Hungarian headquarters? -- We can primarily suspect Kurszán, who must have already suspected that Árpád would later oust his family from power.) He also learns -- but let us be lenient with Fodor and assume that he already knew -- that the Pechenegs have appeared at the Dnye* trial. He then sends an embassy to the Pechenegs with the offer that they attack the Hungarians together, because their main army is far away! The Pechenegs either believe this or not.

It is much more obvious that they do not believe it, but first try to verify the truth of the news themselves, 360 ===== PAGE 357 ===== that is, they send out scouts and spies. But let us be understanding again and believe Fodor that the Pechenegs immediately take every word of the envoys of a completely foreign people, never heard of before, as cash. In this case - according to a well-established ancient custom - sharp bargaining begins about the distribution of the booty and many other things. However, they can only begin negotiations if the autonomous, almost independent tribal leaders of the eight Pecheneg tribes living in a loose alliance have been summoned in advance. This also takes some time. Finally they agree. But what can they agree on during such a first envoy mission? Only that they are willing - under such and such conditions - to attack the Hungarians together with the Bulgarians at a given time, for example during a certain month. At this the Bulgarian embassy hurries home and informs Simeon of the preliminary agreement. If Simeon is satisfied with the month given by the Pechenegs, he sends his envoys back with the final treaty. In the meantime, he gathers his army and waits for the embassy to return with the exact date of the attack.

Only after this procedure can the attacking armies move. However, after initial successes, they find themselves facing increasingly strong defenses and after the invasion of the eastern borderlands — let's say, up to the Dniester — they can only advance step by step, literally. We can determine the pace of this advance quite precisely: it could not have been faster than the pace of the oxen caravans of the fleeing people (1. Padányi: id. m.). Otherwise, the families of the Hungarians, together with the majority of their cattle, would not have been able to get into the Carpathian Basin. And yet they did.

The journey from the Dniester through the Carpathians to the eastern part of Hungary — with oxen carts, including grazing — takes a good 2-3 months, and if we take into account the frequent river crossings, it takes much longer.

So, after these considerations, what remains of Fodor's (Györfy's) conceptions of the conquest? How can all these events be condensed into the spring and summer of 895? In our opinion, absolutely not. But let us not forget to mention that the only historically undoubted result of these brilliant Bulgarian-Bechenzh victories was — in Fodor's own words — that: "The Bulgarians lost their Transylvanian and Tisza-Videk territories..." That is, their most economically valuable provinces, the centers of salt and ore mining.

In the margin of the chapter on the conquest, it is necessary to mention that Fodor introduces this chapter with the following words: "There is agreement as to whether The opinion of our historians... still not this was a planned, premeditated conquest or a desperate escape from the disastrous Bulgarian-Pechersenenian attack. The more accurate knowledge of the events makes the truth more and more certain to the supporters of the former position." 361 ===== PAGE 358 =====

And after these introductory lines follows the confusion we described above. This clearly proves once again: we are indeed not wrong when we claim that our historical science has still not been able to free itself from the shackles of outdated 19th-century literature. Once again about the pre-conquest population of the Carpathian Basin On page 237 of his book, Fodor estimates the population of the Carpathian Basin before the conquest at no more than 200,000, while the number of Hungarians, based on data mentioned in Arab sources, is: "their king marches out with 20,000 horsemen." Thus, the unprovable assumption, --, It puts it at 400-500,000. which only serves to support the so-called linguistic historical findings of Finno-Ugric linguistics, reappears, which we have already seen when introducing Bartha's work. The Finno-Ugric perspective desperately needs this hypothesis, because without it they would not be able to explain why the "Finno-Ugric" Hungarian language has remained in the Carpathian Basin to this day, and why it did not become Slavized, like Bulgarian, in a short time. have This fact can only be understood if the Finno-Ugric-speaking conquerors are considered to be at least 70-80% majority compared to the local population speaking other languages. Otherwise, our language could not preserved its structure, which is fundamentally different from the languages of the surrounding peoples, intact. Their situation is only aggravated by the historical fact that with the conquering Hungarians, a considerable number of groups, certainly not Hungarian-speaking, arrived in our country: the Kabars and various other tribes that joined them; — let us remember, several of our tribal names demonstrably originate from Turkic languages.

We have already presented the evidence for the mass survival of the Avars after the conquest (Gyula László, Pál Lipták, F. Altheim, etc.). Now we will supplement our argument at that time with two further considerations.

First: the total population of the naturally blessed, fertile Carpathian Basin at the end of the 9th century could not have been only 200,000 people, when at the same time, according to research, at least 8-10 million people lived in other, mostly barren regions of Europe with much less favorable climates; and this is only in Central and Western Europe, not counting the territories of today's Russia. This figure — based on an area of approximately 4.5 million km² — means a minimum of 1.8 people per km². At the same time, according to Fodor, the population density in the Carpathian Basin would have been well below 1.0, i.e. it would not have reached even half of the European average. — It can therefore be considered much more realistic if we start from the average European population density and estimate the pre-conquest population of the Carpathian Basin — very cautiously — at 300,350,000, 362 ===== PAGE 359 =====

Secondly: we consider it very exaggerated to conclude that out of the twenty thousand horsemen displayed, there were 400-500,000 Hungarians who conquered the country. It is very unlikely that in the case of the Hungarians, who were mostly animal breeders, 8 if not 18 in full, but approximately comparable in structure and social organization to other so-called nomadic horsemen, only every 20-25 people would have been armed. Studies conducted among the nomadic horsemen of Central Asia in the 19th-20th centuries showed that among them we can count on an average of 5 people per yurt, i.e. per small family, and in the vast majority of cases each yurt displayed one armed person. Only in very poor families did it happen that two small families jointly equipped a warrior. Even if we take this latter circumstance as a basis, we do not arrive at more than — in the case of 20,000 warriors — a total population of about 200,000. However, taking into account the partly settled, agricultural lifestyle of the conquering Hungarians - which can hardly be disputed today - we can estimate the total number of the conquerors at a maximum of 300-350,000.

Based on these considerations, we could therefore expect two populations with roughly similar numbers in relation to the conquering Hungarians and the population of the Carpathian Basin.

If this ratio proves to be correct, it could shed more light on several obscure historical questions of the era of the Hungarian Conquest and the period immediately following it. For example, the hitherto incomprehensible contradiction that characterizes the Hungarian-Peshenyo relationship in the times before and after the conquest would be resolved. While, according to the sources, the Pechenegs repeatedly took offensive action before the conquest — even if not with such irresistible force as our historiography believes — after the conquest, the Pecheneg harassment ceased without a transition, so to speak, at one stroke, and indeed, after that, we can only read in the well heads about one Pecheneg group after another submitting to the Hungarian princes and requesting permission to settle in Magyarország, even at the price of assuming the border guard service that was undoubtedly very burdensome and entailed great blood sacrifices. — This phenomenon cannot be explained by the protective wall of the Carpathians alone, because, for example, the mountains did not prevent the Uz and Kumans from repeatedly raiding our homeland,

It would also be easy to understand the anthropological conclusion that the Avar masses found here played such a large role in the ethnogenesis of the Hungarians that the skulls of the Árpád period appear, anthropologically speaking, to be an uninterrupted continuation of the late Avar period (Liptak). We would like to discuss this demographic consideration, especially as an essential support for the double conquest of Hungary.

363
===== PAGE 360 =====

The exclusive Finno-Ugric trend and the double conquest” Since we are already on the subject of the double conquest, let us see what arguments Fodor brings up against this assumption. Given his view of history as revealed in his statements so far, we can only consider it natural that he cannot accept Gyula László’s theory. He summarizes his main “difficulties” in five points (p. 239): “1. The Avars who arrived in our country around 670 did not develop in the areas where — according to our current knowledge — Finno-Ugric peoples had ever lived, and consequently, they could hardly have been Finno-Ugric, i.e. Hungarian-speaking. 2. It is also unlikely that a significant number of Hungarian ethnic groups joined the Avars pushing westward from Asia somewhere in Eastern Europe, since at the time of their migration across the European steppe, the Hungarians — as we saw above — were still living in Magna Hungaria, further north of the steppe highway. 3. Our archaeological findings definitely speak against the assumption that the Avars arrived in significant numbers at the time of the Hungarian conquest. The sparsely populated Avar population merged into the Hungarians in a very short time, and not vice versa. Archaeological and anthropological studies clearly prove that in 895 A large number of new populations arrived in the Carpathian Basin, with their burial customs rooted in religious beliefs and a unique treasure trove of artifacts.

their art and anthropological constitution were also different from those of the Avars. The basis of the later national development of the Hungarians was formed by this population that moved in in 895. 4. From this latter circumstance and from many other data, we can conclude that the vast majority of Árpád’s conquerors were Hungarian-speaking, and only the annexed Kabars formed a significant Turkish-speaking group. 5. Finally, not a single contemporary written source mentions that Hungarians lived in our (?) country before 895, and the ethnonym “Magyar” does not appear in earlier place names either.” (The question mark after the incorrect use of the article “, a” is ours.)

These arguments once again only expose the blatant one-sidedness and hopeless irrationality of Fodor and the Finno-Ugric school’s historical perspective and scientific methods, without being able to refute anything in substance from the data supporting the double conquest. It can be shown from all five points that they either simply do not correspond to the facts or do not belong to the subject, or they slam open doors; they refute things that Gyula László does not claim.

Fodor therefore uses three different arguments at the same time, all of which are contrary to the elementary requirements of scientific methodology, i.e., unscientific, against the thesis he does not like:

364
===== PAGE 361 =====

- 1. by stating untruths,
- 2. with side conversation,
- 3. by distorting the proposition to be refuted.

But let us take a closer look at Fodor’s “main difficulties”. In the first point, he argues with the never-proven hypothesis of a historical view based on an exclusive Finno-Ugric origin, which is made even more improbable and unstable by the numerous data supporting the assumption of a double conquest than it already was. Here, therefore, one view is opposed to another, namely an idea built exclusively on unprovable, 19th-century linguistic conclusions, with a complex view based on concrete, more recent archaeological finds and historical data. In such a case, it could only be refuted in a meaningful way if Fodor were to present thorough, compelling arguments to prove the Finno-Ugric thesis, much more convincing than Gyula László’s for a double conquest. However, he does not write anything about such things. For him, the mere existence of the Finno-Ugric lad theory is sufficient to reject other ideas. This procedure can be called anything but scientific. No hypothesis in itself refutes another hypothesis.

The second point also rejects the possibility of a double conquest, based on a hypothesis. The essential difference, however, is that this assumption (namely that the Hungarians lived in Bashkiria around 670) is even more unstable than the previous one; it cannot be considered a generally accepted opinion even within the Finno-Ugric school of thought (see our notes on the chapter “Magna Hungaria”). Fodor, therefore, by treating his rather isolated position as a fact, goes far beyond the concept of hearsay, and in this case already falls into the slush of stating improbabilities.

In the third point, all three of the above-mentioned “into” “methodical unmethodicality” is mixed freely. It is a side note that “in 895 a large number of new populations arrived in the Carpathian Basin”, because this does not affect the essence of László Gyula’s thesis in the least. The statement that the archaeological legacy and anthropological structure of the conquerors differed from those of the Avars means a distortion of the theory of the double conquest. László Gyula never stated anything to the contrary, and in fact, he concluded from the difference in the two archaeological groups of finds and the skull structure that the large masses of Avar-Hungarians found here were there, since the late Avar sites were not occupied by the conquerors, and on the other hand, the late Avar cranial characteristics later became dominant in the undoubtedly Hungarian population of the early Árpád period. In other words, in addition to distorting László Gyula’s theses, this counterargument also strongly approaches the limits of stating untruths,

again tries to refute Gyula László’s misrepresentation: 365
... the vast majority of Árpád’s conquerors today- In the fourth point, Fodor

===== PAGE 362 =====

“It was a Hungarian language.” — he writes. Laszlo never claimed that the majority of the conquerors of 895 were not Hungarian speakers. However, in the fifth point, Fodor states an untruth beyond all doubt: “...not a single contemporary written source says that Hungarians lived in our (?) country before 895...” We know not only one, but several contemporary sources, which mention the folk names “Ungar”, “Hungar”, “Ugor” in the Pannonian region before 895. Moreover, a Dunántúl place name that is well understood in Hungarian and still exists today also occurs in the well heads before 895. Here is a merely improvised, “ad hoc” compilation:

- Approximately , the “Conversio Baguariorum et Carantanorum (Monumenta Germaniae Historica, XI, Band, 12.) already mentions a bishop and comes named “Hungarus” in the time of Charlemagne and his immediate successors, based on a charter from 888.
2. A charter of Louis the German from 860 mentions a mountain called “marcha uengeriorum” and the Avars 3. The “, uenger” (Gyula László: The name “Strada” “double conquest”). military route leading through Slavonia and Friuli to Lombardy (Answer “Ungarorum” is mentioned in a charter from 888 as a name that had long been well-known to the people (Sasinek: to the answer of Sándor Márki; Centuries, 1905).
4. In a charter from 762-770, the property of the Sexti monastery (Friaul) is “and is seen as the way of the Hungarians” (Sasinek: id. m.).
5. The “Ugrians” destroy the city of Aemonia in Friuli in 590 (Monum. Germ. Hist. SSRR Longob. et Ital. Hannover, 1878).
6. Charlemagne returned home from his Avar campaign through “Sarwar” in 791.
7. The same “Sarwar” also occurs in 805: Theodor Avar kán Böhmer: Regesta “Between Sarwar” and “Heimburg” he sought a new settlement from Charlemagne. (The data no. 6 and 7 are 1. Dr. JF cronologico-diplomatica Karolum, Frankfurt am Main, 1833; pp. 17 and 24).

Why this obvious nervousness and attempt to refute, even with such primitive means as Fodor’s arguments? Because the dual citizenship thesis cannot be accommodated even then. into the idea of our exclusive Finno-Ugric origin 6[+, even if Gyula László tries to reconcile it with it. This is best known to those who themselves are fully aware that the 19th century dogma of our Finno-Ugric origin would not survive even the slightest correction without the entire Hungarian, ancient history built on it collapsing. That is why they avoid a sober, thorough, and genuinely scientific discussion of the theory of the double conquest, and place themselves in the sterile

366
===== PAGE 363 =====

to the position of denial, merely puffing out outdated 19th century linguistic phrases as counterarguments. However, from Fodor’s utterly unmethodical argument, it also becomes clear that István Szili is certainly right when he notes in his essay “On the Deficiencies of Finno-Ugric Prehistory” (p. 63): “It seems that no matter how much evidence is gathered for the validity of this theory (i.e. the double conquest), it will never be enough for some researchers. Naturally, no one considers it necessary to prove the opposite.”

The tree of life motif “... “Iranian” origins and the Pan-Indo-Germanic perspective of our historical science We should also stop at page 256, where Fodor instructs us as follows about the origin of the motifs of the decorative discs from the time of the conquest of Bashalm: it is not difficult to recognize In the depiction the tree of life growing from the back of a horse made of plant elements. This method of representation can ultimately be traced back to an ancient Iranian prototype: the representation of an animal (lion, etc.) standing in front of the tree of life.”

Unfortunately, Fodor was again completely wrong. International archaeology and art history trace this “mode of representation” back to an ancient Sumerian prototype. This motif appears en masse and continuously among the Sumerians from the end of the 4th millennium BC. We can safely say that it was one of the leading motifs of ancient Sumerian glyptics and relief art. In every archaeological or art historical work dealing with Mesopotamia, we can find a series of Sumerian depictions of animals standing at or climbing the tree of life. The Iranian Persians adopted this motif from Mesopotamia, through multiple transmissions: Hurrian-Urartian-Med. And this was a good two and a half millennia after its first appearance, in the 6th century BC.

We may ask: what theoretical and methodological considerations lead Fodor to state something that is clearly contrary to the facts in a question that can be easily verified by any “dilettante”? What historical basis is there for tracing everything back to Persian, or in a broader sense, “Iranian”, or perhaps “Indo-Iranian”, even at the cost of tangible untruths? Why should we remain silent about the fact that has been well-known for a hundred years, that it was certainly not the “Indo-Iranians”?

the first to "they came down from the tree", but we find high civilizations in East Asia 2500 years before them? Not to mention the fact that the competent demonstrate Indo-Germanic studies in the period 367

===== PAGE 364 =====

in: the term "Indo-Germanic" is purely a linguistic, grammatical concept, which has no ethnic content whatsoever.* We must point out most emphatically that this view of history has been out of date for almost a century; it directly contradicts all the archaeological and historical facts that have been known since then. Therefore, if someone writes a work on prehistory in 1975 without taking into account either the newly accepted chronology of the Indo-Germanic languages or these pre-Asian cultures, we simply cannot take the archaeological or pre-historical conclusions and assumptions of that author seriously. Such a researcher is either hopelessly behind the development of science or is extremely one-sided. However, since we cannot assume that Fodor's knowledge does not even reach the level of an average high school student, the second possibility remains.

Here, however, the question involuntarily arises: why is it so one-sided? Why does it distort or omit generally known things, even taking the risk of ridicule? The answer here is the same as we have been able to establish without exception in similar cases so far: the thesis of our exclusive Finno-Ugric origin requires that we take Iranians into account already in the 4th-3rd millennium BC, because Finno-Ugric studies, based on so-called "linguistic historical facts", has undoubtedly established "— as they say - that our words considered to be "Ősiráni" loanwords entered the "Finno-Ugric" basic language in the 4th-3rd millennium BC".

For this reason, due to the 19th century linguistic idea, Harmatta must also reckon with Indic peoples in Mesopotamia as early as the end of the 3rd millennium BC, and with Indo-Iranians in Central Asia as early as the 3rd millennium BC and before (Article I in Hungarian Studies on Archaeology; p. 169). In the same place, he states without any commentary, simply as a fact, that: "the separation of Indo-Iranian tribes from other groups of Indo-Europeans occurred before the development of agriculture, approximately around 5000 BC" (ibid., p. 169).

We have seen that such assumptions have been rejected by Indo-Germanic studies in the last 50 years, with the exception of the isolated, ahistorical trend that has already "Indo-Germanized" the Ainu, strictly on the basis of "sound laws." This latter circumstance, incidentally, also shows how "undoubted" the linguistic history and language relationship statements of the linguistic school working with regular sound changes are in reality.

Let us think a little about whether it is worth dragging out the fiction of "our Finno-Ugric origins" by uncritically voicing all these ahistorical, long-disproven, 19th-century ideas - without evidence, merely referring to the mass of specialized literature that has accumulated in the meantime.

* (See more details in the next book.) 368

===== PAGE 365 =====

We believe that the discrediting of Hungarian historical science will perhaps be too high a price to pay for clinging to an outdated hypothesis with all our might. There can hardly be any other end to this approach. We have long since reached the point where, for example, Altheim, in the same work, calls Professor Harmatta twice, not only feeble-minded, but also other Hungarian researchers who — as he writes — "called" Attila's chiefs Goths and also explain the Latin word "reges" as Gothic, reiks.

Certainly, something like this is quite close to being able to trigger the concept of "distorted theories" or "confused ideas," which, according to Fodor, are hardly anything more than good-natured cheerfulness. "As we have seen, Altheim does not think otherwise.

But such a thing does not arise in us. The trend "good-natured cheerfulness". Moreover, we do not even feel any gloating over the fact that the boomerang has backfired, hitting precisely those who speak out loudest in the sanctioned by the above has increasingly about "distorted theories" and unscientificity. We find it very sad that our historical science is academically broken away from the contemporary line of international historical research. Indo-Germanocentric outbursts like the ones above have mostly disappeared from foreign scientific literature and only now and then do they experience their pale second flowering in sensationalist journalism, in the field of so-called historical "bestsellers". Or in the cartoons of children's television programs: in the stories of "Astérix", the Gallo-Celtic hero, and "Wicki", the cunning Viking child.

Additions to the ancient Ugric custom of boat burial

On pages 258-259, Fodor explains in detail that the boat-shaped headstones found in the upper reaches of the Tisza River "preserve the ancient Ugric custom of burial in boats." He supports this by saying that a boat-shaped grave pit was also observed in the Szabadkígyós cemetery from the time of the conquest (András Pálóczi-Horváth).

It is worth noting, however, that the Vikings or Varangians, with whom the Hungarians had very close relations for at least a century before the conquest, also buried themselves in boats in the second half of the 1st millennium AD, and even much earlier. This burial custom was common in the pagan North Germanic world of the 1st millennium, as is clearly demonstrated by the numerous relevant finds. The wealthier, the princes, and the great men received entire boats as a means of transport to the afterlife: Sutton Hoo in England, Oseberg in Norway, Vendel, Valsgarde in Sweden, etc. (Propyläen Kunstgeschichte; Supplementband IV. Kunst der Völkerwanderungszeit, 1979).

369

===== PAGE 366 =====

We must therefore once again condemn Fodor for his unacceptable one-sidedness when, while remaining completely silent about this very important circumstance, he tries to make his readers believe that in the period in question, boat burials did not occur anywhere else except among the Ugrians. In connection with these boat-shaped headstones from the Upper Tisza region, we would like to remind you of Nándor Fettich's already mentioned statement that, according to the strong North Germanic influence on the skulls from the Hencida cemetery from the time of the conquest, groups mixed with Vikings from the conquering Hungarians also arrived in our country. And Hencida, on the Berettyó River, is not very far from the Upper Tisza.

This anthropologically clearly demonstrable fact obviously suggests that the boat-shaped headstones in question are much more likely to preserve the tradition of Viking burials in boats than that of the Ugrians.

Or perhaps the entire northern Germanic people had adopted this custom from the Voguls? Such a thing would only be believable to those who, like Fodor, examine everything through a "Ugric" lens. But even if this were the case, it would only prove that this burial rite reached the Carpathian Basin from the Viking-mixed Hungarians.

Given these circumstances, we can no longer be surprised why Fodor so tactfully glosses over the fact that Vikings were buried in boats. After mentioning this, he could hardly claim - as if it were self-evident - that the chieftains of the Upper Tisza region "preserve the ancient Ugric custom of burial in boats."

Fodor's statement is therefore nothing more than a conscious misdirection of the reader, directing the thinking of the general public exclusively in a Finno-Ugric direction. Let us remember our author's promise: "We do not try to conceal the difficulties anywhere, or rather, to present our more or less probable hypotheses in the final analysis."

To dress it up in the guise of solutions (Fodor: ism.m. p. 28) Well, perhaps the above statement, made at the cost of consciously not mentioning the fact of Viking boat burial, is not really a concealment of difficulties and a dressing up of more or less probable assumptions in the guise of final solutions?

Summary

This concludes our introduction. In many cases, the analysis of Fodor's book has undoubtedly shown that the work not only does not meet the requirements of scientific method at all, but also shows that its author considers the generally educated Hungarian readership - for whom he intended the book - to be so historically uninformed that he has no idea how to explain it.

===== PAGE 367 =====

Yes, in many cases they don't even shy away from completely crude distortions, thinking: they don't know what they're talking about anyway. We emphatically reject this condescending arrogance. Significant masses of our people are very interested in the questions of our origin, and many of them are already familiar enough with our prehistoric problems that they can adequately evaluate historical works like Fodor's book. Our enlightened, critical readership can no longer simply be explained on the basis of authority. In order to believe something, it requires reliable historical sources, careful consideration of archaeological finds, reasonable conclusions, and logical arguments, not just monotonous references to earlier authors, or the naive silence of a considerable part of the relevant knowledge available to us, verifiable even by the interested "dilettante", in favor of pre-formulated fossil assumptions of 19th century origin.

This obvious lack of method in the exclusive Finno-Ugric linguistic and historical approach is in many cases further exacerbated by the shocking lack of training of individual researchers. It also makes up for its shortcomings, such as Fodor's "snapped" and "fisherman-hunter" Neolithic era and other statements of similar weight. It is primarily in these circumstances that we can look for the deeper reasons for the widespread feeling of lack, which then — in many cases, in the form of autodidacts without the appropriate training — really gives rise to "historical miracle bugs." However, the question cannot be settled by the superficial remark that the psychological background of these "wonderbugs" is merely an over-exuberant national pride or knowledge - although such motives undoubtedly occur sometimes. But most often it is simply a question of the fact that the linguistic and historical ideas of our school of exclusive Finno-Ugric origin - very often clearly recognizable even to non-professionals - are improbable and illogical, and above all profoundly one-sided. It would therefore be very useful if we also considered the important aspect of why a reader interested in Hungarian history and with an appropriate general education noticed this, of course with other, more reasonable explanations. |

seeks a shock — and finds it. However, since his special knowledge and research opportunities are more or less limited, we cannot be surprised that these explanations are often inadequate. At least not in their entirety — which, however, does not exclude the possibility that in individual details they often see connections that are very worth considering. This is mostly the case in the case of various, not exclusively Finno-Ugric, prehistoric ideas. And the fact that sometimes quite fantastic views come to light is only a logical consequence of the obvious analogy that if

371

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===== PAGE 368 =====

"they" = i.e. the experts - are allowed to argue with unproven, and often even common sense, assumptions, then he - the layman - can also come up with prehistoric theories. Vice versa.

We therefore see the key to the whole phenomenon, the most important psychological motive, in the fact that research with an exclusively Finno-Ugric perspective, despite more than a hundred years of painstaking work, is still unable to outline a linguistic and prehistorical concept that is even somewhat logical, coherent, and free of glaring contradictions.

We know very well that this is natural to a certain extent, since in this area — since we are talking about very ancient times — our knowledge cannot be exhaustive. But it is precisely here that the Finno-Ugric school falls into the same mistake that it condemns so much in others: by using incomplete data, unverifiable assumptions, by one-sidedly using known sources, while at the same time ignoring other, equally known, wellheads or other circumstances that contradict the previously formulated hypothesis, it tries to maintain its outdated ideas, based on our 19th-century scientific knowledge, in a categorical, intolerant tone, almost unchanged, despite the data of the vast archaeological and historical discoveries that have occurred in the meantime, revealing completely different perspectives. Fodor's book is one of the most striking proofs of how much this is so. Looking at Fodor's work from this perspective, we must undoubtedly give András Róna-Tas justice: "On the Famous Path of Verecke" is truly one of the most outstanding works of the latter period of Hungarian prehistory research with an exclusively Finno-Ugric perspective" - even in terms of its shocking one-sidedness and lack of method, with which it undoubtedly surpasses most publications on similar subjects by the Finno-Ugric school.

372
===== PAGE 369 =====

road
Chapter 4
NOTES ON THE METHODOLOGICAL PRACTICE OF OUR PREHISTORIC RESEARCH

Theoretical requirements for evaluating prehistoric hypotheses
In the previous chapters, when presenting the works of Bartha and Fodor, we had to state repeatedly that the Finno-Ugric trend of our historical research does not apply complex, comprehensive, or well-founded and objective methods in its investigations. We could see that they start almost exclusively from the linguistic and linguistic historical ideas of the 19th century in the research of our prehistory, while the massive amount of recent archaeological data or contradictory historical sources are practically not taken into account.
Under such circumstances, it would be essential to clarify the theoretical foundations. Since we still have almost no clear insight into the depths of Hungarian prehistory, for the time being we should be wary of all kinds of apodictic statements based solely on assumptions, such as things like "our linguistics has undoubtedly established" etc. We must finally soberly realize that linguistics has "undoubtedly" established nothing in the world about these ancient times. Everything that is being talked about in this field is merely unproven assumptions, deduced from the also unproven "sound laws" and "regular sound changes", which, moreover, are not even unequivocally acknowledged by their mother, Indo-Germanic studies.

In close connection with this, we should also set limits to the unpredictably proliferating chains of hypotheses, the starting point of which is most often just an assumption itself.
There is no doubt that working hypotheses are necessary, but we should restrain our imagination much more than we have done so far in carrying the conclusions drawn or that can be drawn from them too far. This principled requirement naturally applies to the adherents of the Finno-Ugric trend as well as to those representing other ideas.
researchers. For theoretical and methodological reasons, the practice that has prevailed among the representatives of the currently generally accepted Finno-Ugric hypothesis is fundamentally incorrect and should be rejected: they allow even the most absurd conclusions and call them "scientific", while at the same time requiring the opposing camp to work only with facts that can be proven beyond all doubt. As we have mentioned, in prehistoric research we almost never encounter such provable facts. Most of the time, the only question is which assumption is more reasonable,

. [which more likely corresponds to the former circumstances, El.
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===== PAGE 370 =====
It is therefore inexorably necessary to introduce "equal measure" when evaluating various theories of prehistory or language development.
Here, the problem of prehistory ideas that have increased remarkably in recent decades, and that do not have an exclusively Finno-Ugric perspective, is also connected to these methodological and theoretical questions. Among these — we do not dispute — there are certainly many rejects, but there are also quite a few assumptions that seem to be much more compatible with the latest archaeological and prehistory findings than the thesis of the exclusively Finno-Ugric origin of the Hungarian language and people. The latter are systematically elaborated by experts - or even autodidacts. Such concepts should not simply be labeled as "dilettantes" — we mean workers who base their assumptions and theories on material (archaeological) or concrete linguistic (written) monuments, well-springs "miraculous", even if we find only one or two pieces of data worth considering in them. We are not in the fortunate position of being able to give up on following even a trace. We should conscientiously examine such traces to see if they might contain a grain of truth.

The Finno-Ugric school has so far branded pre-historical or linguistic ideas that are not exclusively Finno-Ugric as unscientific, rejecting them a priori without any closer examination, most often simply with the argument that they "contradict our Finno-Ugric origin". The investigation of the connections of our language beyond the Finno-Ugric relationship is also rejected from the outset, saying that such a thing is linguistically impossible. However, the Finno-Ugric origin of the Hungarian language - we emphasize: its origin, not its relationship! -, as well as our linguistic history and pre-history built on it, are themselves mere hypotheses, not facts. This fundamental difference is evident from what has been said so far, obviously and irrefutably. However, no assumption can be doubted simply on the grounds that it contradicts some other hypothesis.

Therefore, in cases where new theses arise that are incompatible with the currently prevailing Finno-Ugric theory, we cannot emphasize this enough - only all that can be counted
After careful consideration and discussion of the circumstances, we should take a position, but even then not in the form of irrefutable statements, but only by recording the degree of probability of the assumptions in question.
Let no one claim that only those who have a degree from an educational institution can engage in historical research or linguistics. This argument is also one of the most common objections to the Finno-Ugric school. They try to frame the issue as if only a degree makes someone capable of research. We would like to remind you in this regard that we can find it right at the cradle of Finno-Ugric theory.

374
===== PAGE 371 =====

most uninitiated linguists and historians. For example, Sajnovics was an astronomer, Hunfalvy and Reguly were lawyers. But otherwise, in the history of almost every science, the so-called dilettantes played a very prominent, and sometimes even decisive, role. It is not university qualifications that decide the scientific or unscientific nature of a hypothesis, but only its evidence, archaeological, source or other supportability. Let us think, for example, of the case of Schliemann in archaeology. Before he found Troy, everyone laughed at him.

If someone in our time wants to conduct prehistory or linguistic history research with the claim of scientificity, he will inevitably have to come face to face — sooner or later — with the pre-Asian archaeological discoveries of the last century. It is simply impossible to progress in this field today by excluding them. As a fundamental principle requirement, we could say as a watershed of systematic scientificity, the only possible way out appears with imperative necessity: all kinds of
Future research should start from these immeasurable insights, and at the same time we should thoroughly review our previous assumptions, historical and linguistic development ideas, and retain only those that prove to be compatible with the lessons of ancient Proto-Asian civilizations.

We would call such a workflow methodical and scientific.
Hungarian prehistory research and archaeological discoveries in the Middle East
However, with an approach such as that expressed in Antal Bartha's article "What is not according to reason is ugly" (Tört. Szemle, January 1978), we cannot hope to eliminate the one-sidedness of our linguistic and historical studies that has been practiced for a hundred years in the foreseeable future.
The article is preceded by an article by Gyula László, published in Tört. Szemle 1976, issues 1-2, in which he points out that the theory of Sumerian origin is gaining ground among significant masses of our public opinion. He suggests an open debate on this theory, and suggests that it would not hurt if the upcoming ten-volume Hungarian historical synthesis also dealt with these widely spread doctrines in a meaningful way, instead of ignoring them.
These benevolent advices, which — quite rightly — take into account that views different from the Finno-Ugric trend (not only the Sumerian theory!) have spread among the general public largely due to the lack of substantive refutations and honest discussion, were addressed directly to Bartha, the editor of the prehistoric chapters of the aforementioned historical synthesis; and after two years of labor, Bartha's answer was born.

375
===== PAGE 372 =====

After the rather long silence, we might expect that Bartha had looked into the question in the meantime, at least glanced here and there at the relevant literature, and tried to find out as much as he could about the current results of Middle Eastern research, so that he could then prove the untenability of the Sumerian theory with scientific arguments in his answer.

Instead, we find that Bartha's knowledge of pre-Asian archaeological and prehistoric discoveries is shockingly incomplete and, above all, extremely confusing.
"In recent times (emphasis added) there have been reports of prehistoric discoveries that have captured the imagination, particularly from the Middle East," he writes. Well, this is the "latest time" has been going on for a good 120-130 years. He then continues: "The processing of these discoveries and the achievement of somewhat solid results will keep many disciplines busy for years, even decades." According to Bartha, we are still far from being in a position to draw even somewhat certain conclusions from the findings of this research.
"And if the warning to be careful is not emphasized enough in the preliminary popularization," he notes, "dilettantism gains wings and chases the mirage of Hungarian prehistory in the Middle East. Even though the prehistory of the countries there has no direct connection with Hungarian prehistory," Vocatum est.
We would just like to ask, how does Bartha, who three lines above claimed that we will have to wait decades for "somehow solid results", know this so surely? He certainly considers this method of refutation to be very scientific. However, we are much more inclined to believe that such arguments are not even remotely plausible. He then explains that in bringing the fiction of Sumerian origins to life, these Hungarian circles "according to my understanding" are true.
galvanized it to life,

the new prehistoric discoveries, without a doubt, played a role, The truly new moment, that of reactionary

By doing so, he tries to frame the issue as if the above-mentioned "new" pre-Asian discoveries were used by reactionary emigrants to support their ideas, so that they were merely current political and ideological views, in which prehistoric problems were also secondarily involved. From this we can also see that

why did Bartha have to talk about "newer" prehistoric discoveries: this was the only way he could connect the issue with the emigration after 1945, and shift it to an ideological plane, thinking that by doing so he would also be able to discredit its scientific aspect.

We need not dwell on this subject any longer, because it is well known that these excavations in the Middle East — as we have mentioned — have been going on continuously for more than a hundred years, and the Sumerians were not discovered until after 1945. Bartha therefore consciously distorts the real situation when he claims that no firm conclusions can be drawn from the pre-376

===== PAGE 373 =====

On the contrary, from these concrete discoveries in archaeological and written linguistic monuments we can obtain a much, much more certain picture of prehistory and language development than the hypothetical Bartha's other main argument, the proof of the untenability of the Uralic "Ss" tongue.

Sumerian theory, Géza Komoróczy's excellent book "Sumer and Hungarian?" and his article published in the 1976/10 issue of "Valóság" (The Mystery of the Sumerian Language as a Prehistoric Problem), in which - according to Bartha - Komoróczy demonstrated the unfoundedness of the Sumerian theory.

We intend to deal with Komoróczy's works in more detail in the linguistics volume. We note here, however, that Komoróczy, in his excellent book and article, to which Bartha refers, does not prove anything, but merely constantly makes excuses. For example, he assumes that the Sumerian language was an island language, he believes that it has no connection with the Uralic and Altaic languages, he notes that the failure of the efforts to identify them seems symptomatic, etc. In particular, his article published in "Valóság" is almost one giant question mark.

If Bartha Komoróczy believes that these two works can be interpreted as demonstrating the groundlessness of the Sumerian theory, then this only proves the astonishing lack of method in Bartha's scientific thinking, nothing else.

For example, the failure of the efforts to identify the cognates seems symptomatic, and we should not be surprised at all if we know that until now the possible relationships of the Sumerian language have been examined only by the method of sound laws and regular sound changes, and attempts have been made to compare its words with the languages of other language families. According to these dogmas of sound development, mass phonetic correspondences or high-level similarities cannot be reconciled over a period of 4-5000 years, because there is no regular sound change. For example, the cognates of the Sumerian BAL (axe) with the German yyy, (axe); the Hungarian BAL-ta, PALL-os; the Turkish BAL-ta (axe); the Akkadian PIL-akku (axe); the Greek PEI-ekys (battleaxe), etc., are phonetically impossible.

As for the currently popular thesis that the Sumerian language is unrelated and an island language, we have already pointed out several times the unfoundedness of this view and its cultural and linguistic historical absurdity.*

The ideology of Indo-Germanic cultural priority

However, if Bartha is always looking for current political and ideological motives in prehistory research and linguistics, then in the post-1945

* (See more details in the next book.) 377

===== PAGE 374 =====

Instead of bothering with the views of the Hungarian emigration, we could offer him a research area promising much more significant results. In particular, if he would like to know the real, non-scientific, deep psychological roots of the efforts aimed at trivializing and isolating the Sumerian language, he should just turn back two or three pages in the 1976/10 issue of "Valóság" - apart from the remarkable reflection related to the Komoróczy article he mentioned, he can read the clearly and unambiguously formulated opinion on - where Györgyné Hary, in her essay "Additions to the History of a Language Debate", and several other this issue by one of the pioneering international authorities in Sumerology, A.H. Sayce. Because of its importance, we quote Sayce's statement (translated by Györgyné Hary).

European scientists have long been lulled into the comfortable belief that the white race, first and foremost, and Europeans, secondarily, are actually superior to the rest of humanity, and that the development and origin of civilization were rightly attributed to them. The discovery of the common kinship of the Indo-European languages only strengthened this belief. The view was developed that in Sanskrit they had found, if not the primary language, at least something close to it, and an idyllic picture was painted of an Aryan community which already possessed in its Asiatic home the elements of its later culture. Outside them stood the barbarians, the short-faced, slant-eyed, yellow, brown and black races, whose intelligence was not much higher than that of animals. And if they had any culture at all, it was from the white race, and corrupted in the process of receiving it.

They simply saw it as a paradox that culture could have arisen outside the white race.

The decipherers of the Assyrian cuneiform found themselves confronted with this paradox. And with something else: The civilization of the Tigris and Euphrates had arisen among a people who spoke an agglutinative language, and were therefore neither Aryan nor Semitic. Consequently, the Semitic Babylonians and Assyrians derived their culture, including their cuneiform writing, from this older civilization. It seemed impossible that this revolutionary doctrine could be true, and so the Semitic philologists naturally rejected it. For public opinion had for centuries held that the language of Paradise was Hebrew, and so the Semitic scholars of the nineteenth century were unconsciously influenced by the old idea that the Semitic Adam was the first civilized man on earth. It was difficult to put aside the education and prejudices of youth, and the fact that cuneiform writing, deciphered by only a few specialists, was a mystery to the average linguist also played a role. A handful of researchers confused the students of the extinct Semitic languages with new and dangerous principles." (Sayce; The Sumerians; 1908),

378

===== PAGE 375 =====

We highly recommend these plain lines to everyone's attention, especially because we have previously followed and demonstrated the pervasive presence of this same, blatant, Eurocentric view of history point by point, not only in Indo-Germanic scholarship, since its beginnings, but also in our special research into Hungarian prehistory and language development, since the Hunfalvy joined the Indo-Germanocentric view. To justify this opinion, we can also mention the provocative question of Halévy's controversial article in the "Journal Asiatique" of 1874, with which he introduced his tireless struggle of more than forty years to deny the existence of the Sumerian language and people: "Is the presence of a Turanian people on the soil of Babylon permissible?" (Zsigmond Varga's communication). This view is also expressed quite clearly and unambiguously - indeed, in the tone of political demagoguery - in an article by E. Babelon (Annales de philosophie chrétienne, 1882), where he justifies Halévy with the argument that a Turanian "Five Thousand Years in the Future" (in his book).

empire could not have existed between the Euphrates and the Tigris only because the Turanian peoples were always barbarians and came as God's scourge to destroy the work of other races (Zs. Varga's communication, 1882), in his work).

However, these tricks were overthrown by the facts of archaeology. In the 80s and 90s of the last century, excavations reached the layers of the Mesopotamian cities dating back to the 3rd millennium BC, where the Akkadian-Semitic texts suddenly disappeared, and only clay tablets written in the Sumerian language were found. It was thus proven beyond a doubt that the Sumerian language really existed, and before the appearance of the Semites. That is, the earliest human language was the Sumerian language.

The Sumerian people created a culture in the true sense of the word. The background to the efforts to isolate the Sumerian language

It is no coincidence that it was precisely during this period, around the turn of the century, when Halévy's sterile denial had already become clearly absurd, that the theory of the Sumerian language being unrelated and an insular language was born. In other words, if the thesis of direct cultural priority was not sustainable, at least they tried to save the handle of the broken axe by declaring that the great cultural achievement of the Sumerians was theirs alone, but the survival of this civilization was indirectly due only to the Akkadians, Babylonians, and later the Indo-Germanic Hittites and Iranians, who took it over and preserved it for posterity. To support this twisted argument, it was of course essential to postpone the Sumerian language and people without descendants, to declare them an insignificant insular language and people,

379

===== PAGE 376 =====

In these historical and philosophical assumptions we can look for the real roots of the otherwise incomprehensible phenomenon that both Indo-Germanic and Finno-Ugric studies still fear to touch the Sumerian language as the devil fears holy water. In Chapter 5 of the next book we will give a brief account of the unforeseeable consequences that would follow for the ideas of language development that have been ingrained for almost a century and a half if the Sumerian language were to be compared thoroughly, objectively, and without any phonological prejudices with the languages of the West Eurasian language families. We will show there that not only are there hundreds of serial We are not faced with dictionary similarities, but also with massive, organic parallels in word formation, synonyms, homonyms, linguistic associations, and metaphorical meanings between Sumerian, Semitic, and Indo-Germanic languages. These close connections mathematically rule out the possibility of coincidence.

We would be very curious to know whether Bartha does Do you think it is "intellectually true" that Indo-Germanic and Finno-Ugric philology, despite all these facts, still does not take not take the Sumerian language into account in his linguistic historical studies, but continues to insist on the completely unprovable dogma of "regular sound development", which is most obviously refuted by the massive phonetic correspondences of the 4-5000-year-old Sumerian language monuments with the words of modern languages. In any case, we believe that such behavior is "not in accordance with reason", and therefore ugly.

The prehistoric pursuits of the Pan-Indo-Germanic perspective

But it is not only in connection with the Sumerian question that we encounter these prepotent views of cultural priority. Let us look, for example, at J. Marguart's infamous book (Osteuropäische und ostasiatische Streifzüge), which was used as a bible in Hungarian historical research circles for decades, and there too the flowers of this skewed view appear in pure culture. Right in the preface, he condemns Otto the Great with sad despair for not having exterminated the Hungarians as thoroughly after the Battle of Augsburg - in the name of European culture! - as Charlemagne did with the Avars: kal. We will later learn that the mass murder of the Slavic tribes along the Elbe-Oder is just such a praiseworthy European-Germanic. kultimissid, there were 4 bars, completely illogically, but at the same time he weeps with crocodile tears for the Slovaks, who have been "groaning under the Hungarian yoke for 900 years". He forgets that the "poor" Slovaks not only live; but have multiplied considerably in a thousand years, in contrast to the Slavs of the Elbe-Oder Marquart Vélés region, who, however, it should be noted that these are a deeply Germanic, cultural mission. Consequently, we only know them from hearsay. But this is only natural, after all.

380

===== PAGE 377 =====

According to him, everything is permitted to the Germanic "Herrenvolk", and the wild barbarian Hungarians should be happy to live - thanks to Otto the Great's incomprehensible arrogance - let alone oppress Slovaks, thereby shamelessly removing them from the sphere of influence of the blessed Germanic cultural mission.

It is also very characteristic how indignantly Marguarth rejects the statement of the contemporary eyewitness, Ibrahim ibn Yagub, that the Pechenegs, Khazars and Rus peoples were strongly mixed with the Slavs. His argument is that the superior, cultured Slavs could not have mixed with such barbaric hordes as the Pechenegs or the Khazars. He seems to have forgotten the Germanic cultural mission along the Elbe-Order, or that the Germans were even more superior and cultured than the Slavs.

This superiority is also attested to by the information of Harun ben Yahya, for example, when he writes that even in his time — around 890 — the Lombards lived in tents and dilapidated huts made of planks, and apart from their capital, Pavia — where the population was Romano-Italian — they did not have a single city, let alone villages.

Needless to say, Marguarth simply describes the report of this contemporary eyewitness as a mistake, although he must admit that Yahja describes the Lombard landscape and Pavia very accurately, so he must have been there.

From this episode we can also measure the level of the much-mentioned great Western culture in the age of the conquest, from which we supposedly took over almost everything. We get a particularly vivid picture of the real situation if we compare Harun ben Jahja's data with the Hungarian conditions at that time. The Lombards did not build a single city or village in the heart of Italy during their more than three hundred years of stay there, while in our country, in the time of Saint Stephen, a hundred years after the conquest, we already know of several cities, not to mention the stone and brick buildings of the conquest-era village of Felyi excavated by Gyula László.

But in this regard, we also remind you of those alleged "Lombard masters" who, according to Hungarian art historians, played such a prominent role in the construction of our Romanesque churches that a student at the College of Fine Arts will inevitably fail if he does not trace almost every engraving on one of our Romanian column capitals back to a Lombard or French master.

Speaking of the excavations in Felyi, we should also mention that while €vi 20,000 forints are available for the excavation of this village from the time of the conquest - the eminent significance of which in Hungarian historical research does not need to be emphasized - the annual budget for a larger Roman-era excavation reaches half a million forints or even more (Gyula László's statement in his article "Letters of a Professor of Archaeology"; História, 1979. 3. issue),

| 381 a

===== PAGE 378 =====

This shockingly disproportionate financial support once again clearly demonstrates the frantic efforts of the authoritative circles of our historical science to continue to maintain the fiction of the "barbarian Hungarians" that they have been promoting for a hundred years. They know very well that as soon as the high level of culture of the conquering Hungarians is proven beyond all doubt, supported by archaeological finds, they too will have to thoroughly revise their one-sided views built on Indo-Germanic cultural dominance, including the true origin of thousands of our words that they have declared to be foreign words. That is why the excavation of the one-millionth provincial Roman column in our country — what an epoch-making discovery! — costs about the same number of forints, and the village of Felyi from the time of the conquest, which refutes the ideas of primitive Hungarians from the Finno-Ugric perspective, is not even one twentieth of that.

In any case, this is also a very typical "scientific" method: to stifle research that promises results contrary to certain hypotheses and disturbs the circles of the trend in question by administrative means, by withdrawing financial support, or at least to make it extremely difficult and delayed. There can be little doubt as to how "reasonable" such a thing is. Can we hope that Bartha does not think so either?

Wouldn't it be Would it be more "realistic" and more expedient if Bartha extended his campaign against the ideologisation of historical science and "pathological" historical approaches to the phenomena outlined above? All the more so because within Indo-Germanic historical research and linguistics itself, the revision of these exaggerated cultural priority theories had long since begun - since 1945.

Thus, for example, we can read in Altheim-Stiehl's already mentioned joint work (Geschichte Mittelasiens in Altertum) that Greekness and Hellenism were only a passing episode in the history of the Middle East and Central Asia, and that the previous one-sided, Greco-centric approach must be thoroughly revised. The independent development and culture of Central Asia are much larger in scope than the previous distorted view of history taught, they write.

It would not have hurt Bartha to have looked into this book, either, because he could have learned, for example, to look for the roots of the Sasanian style not only in "Hellenistic-Iranian" and "Bactrian-Greek" influences (Bartha: A 9th-10th century Hungarian society; p. 126).

We would also like to especially recommend to our researchers who are studying our Iranian connections and word parallels the statement of Altheim-Stiehl that although the word "vardana" means a fenced place ("umwalite Siedlung") in the Iranian language, there is no mention of cities in either the Avesta or the Rigveda. Therefore, we should carefully examine again whether "vár", "várad", "vár" are truly Iranian loanwords.

382

===== PAGE 379 =====

Our words "váda" (ý. 6. Sumerian "bar" = „rings umschlossen, Ringmauer, Schutz“; to surround, wall, protection).

Lám, Indo-Germanic science itself is already scolding the wild followers of its own 19th century cultural priority theory. What need is there for us, Hungarians, to break our spears in support of these outdated views under such circumstances? Let us scold too, and then those "reactionary" emigrations that Bartha so much resents will soon disappear on their own. These phenomena ~~pathological aprioristic notions, are also reactions~~ to the theory of Indo-Germanic cultural priority, to its obvious distortions of history, and even to its more than "reactionary views" but not in the sense that Bartha does: such once falsifications. They do nothing but retort, for better or worse, and oppose various Indo-Germanic "Herrenschicht" (ruling stratum) theories to "Turanian" or even Hungarian "Herrenschichts". If the primary, Indo-Germanic "Herrenschicht" finally gets off its high horse, then the other secondary, "just-for-nothing Herrenschicht" will burst without a trace, like a soap bubble.

We believe that it would have been instructive for Bartha if, before declaring his crusade against similar views emerging in certain groups of Hungarians abroad, he had given some thought, for example — remembering Sayce's words quoted above — to what kind of ideological and historical foundations he himself, in his essay "Society and Economy in Hungarian Prehistory", set out to describe the immigrants he had thoroughly and objectively from the south to the Volga-Ural region at the beginning of the 2nd millennium BC, when he believed that the colonists with a developed bronze culture were "Ösiránians". If considered the archaeological connections to be presented in our next volume, as well as the current position of modern Indo-Germanic studies on the development of the Indo-Germanic languages, we would not believe that he would still be keen to talk about mirage chasers in the Middle East, because he would have to realize that other people seem to be chasing mirages, or rather, ancient Iranians - not in the Middle East, but in the Eurasian steppe. The primarily competent Indo-Germanic linguistics and history have shown for a good 30-40 years that the term "Indo-Germanic" does not even denote a linguistic category, but is merely a linguistic structural and grammatical concept, and we cannot look for any ethnic content in it at all.

We cannot emphasize enough: an unbiased, objective, and comprehensive reassessment is needed, so to speak, in the entire field of history and anthropology, if we do not want to continue to stagnate in the lurch of outdated 19th-century views.

383

===== PAGE 380 =====

Ignoring the linguistic monuments of Middle Eastern cultures is methodologically unacceptable.

The discoveries of archaeology in the Middle East have clearly shown - not only recently, but for at least 70-80 years - that the earlier theories of prehistory and language development were fundamentally flawed. We cannot avoid drawing the necessary consequences sooner or later. The need for revision is becoming more and more urgent in direct proportion to the proliferation of archaeological finds, and has been for the past 60-70 years.

It is simply intolerable from a methodological perspective that the research of history and language that calls itself scientific does not take into account the material, intellectual and linguistic relics of the most ancient cultures of humanity even after more than half a century, but instead tries to isolate these civilizations with the help of wordy and unreasonable hair-splitting, and although it acknowledges their wide-ranging, massive cultural effects and continental-scale ethnic conquests in principle, it nevertheless struggles with all its might against the possibility of any kind of connection between the languages of these cultures and the later West Eurasian language families, apart from a few forced or involuntary, "allowed" so-called international migratory words, but even in these cases, it mostly ignores the ultimate Sumerian source.

The only possible explanation for this behavior can be found in Sayce's lines quoted above. Centuries of reflection, the ingrained belief in the superiority and cultural and historical primacy of the European races, still have an impact.

An integral part of this approach, which seeks to isolate the pre-Asian civilizations, is that not only Sumerian, but also other ancient languages known from excavations, Elamite, Hatti, Lull, Gutian, Hurrian and Urartu — which, miraculously, all belong to the type of inflected languages — were declared, with phenomenal speed, to be unrelated, isolated island languages, at the moment it became clear that they could not be related to either the Indo-Germanic or the Semitic languages.

It doesn't occur to anyone how grotesque this position is. To declare the languages of the first center of civilization on our planet, but primarily the Sumerian language, which dominated the entire Proto-Asian region for almost two millennia and then continued to be used as a cultic, scientific and literary language for another millennium and a half, to be an island language, is simply a cultural-historical absurdity. It is even more illogical than if someone were to claim that Greek or Latin were island languages at the time. Whereas in Greco-Roman times, the peoples conquered by the Hellenes and Latins or strongly influenced by them culturally already had their own relations with the

===== PAGE 381 =====

They had highly developed cultures and long-established languages, while in the 4th-3rd millennium BC, during the pre-Asian dominance of the Sumerian language, the surrounding ethnic groups were still at the beginning of their development, both culturally and linguistically. Therefore, the difference in cultural level, the so-called cultural ladder, between them and the Sumerians was much higher than in ancient times between the Greeks, Romans, and the so-called among "barbarians".

Given this cultural background, we cannot accept, even with the greatest goodwill, the unreasonable views that the Sumerian language was an island language, which are in direct contradiction to all the archaeological discoveries of the last hundred years.

The migration of the Ural homeland from the reach of the Near East civilizations

There are, however, certain indications that in the stronghold of the exclusive Finno-Ugric trend, they are not in reality as firmly convinced that the Sumerian language is an island language as their confident statements might suggest. However, we do not wish to express this suspicion, because we are aware that such a thing can never be scientifically proven.

In any case, it is very striking that the Finno-Ugric and Uralic ancestral homeland, which had settled in the Oka, Kama and Bjelaja regions for many decades, started moving again in the early sixties and did not stop until the northern Ural-Also-Ob region (I. Fodor: Verecke's famous path..., and P. Hajdú: Finnugor népek és nyelvek, 1962). That is, the presumed area of the development of the Finno-Ugric (Uralic) peoples was placed between 55 degrees north latitude and 60-65 degrees north latitude, in the region of the Arctic Circle. During the presentation of Fodor's book, we already dealt with this new ancestral homeland hypothesis, and we showed that its alleged archaeological evidence comes from much later times than the so-called The supposed development of the Finno- "Ural" or even the Ugric "proto-language". We have also seen that the other main support, the so-called biogeographic data, does not hold up either, but they were spliced together through a completely twisted and distorted interpretation of the results of pollen studies, and on the other hand, on the assumption of the alleged "forgetting" of words (tree names) that are consistently missing from certain Finno-Ugric languages (I. above).

The unbiased observer must involuntarily ask why there was a need for this absolutely unreasonable hypothesis, which cannot be proven with any concrete data, when there are much more probable circumstances in favor of the Finno-Ugric homeland in the Oka-Káma region -- if they want to postulate a unified "Pan-Finno-Ugric" or "Primordial" people at all costs, we cannot get rid of the well-founded suspicion that Finno-Ugric-385

===== PAGE 382 =====

In our time, there arose -- precisely in the early sixties -- such an irresistible desire to learn about the tundras and the Arctic Ocean because in the 1950s - mainly as a result of Gordon Childe's epoch-making work and his complex evidentiary procedure supported by convincing arguments, with which he incorporated the archaeological find complexes known up to that time into a continental-scale, organically connected, unified prehistoric picture (G. Childe: Vorgeschichte der europäischen Kultur; and Stufen der Kultur) - international archaeology and prehistoric research recognized the decisive role of ancient pre-Asian civilizations in the cultural development of Western Eurasia (I. for example Schachermeyr: Die vorderasiatische Kulturtrift). Moreover, it became obvious that in addition to the massive cultural influences, we also have to take into account the mass colonial activities of various pre-Asian ethnic groups in these regions, primarily in the southern regions.

Before these realizations, we could safely remain, along with the other Finno-Ugric peoples, in the Kama region, because it was convenient and quite convincing to refer to the 3-4000 kilometer distance separating these areas from Mesopotamia if someone came up with striking Sumerian parallels of the Hungarian language; just as until the 1960s, geographical distance was indeed one of the main arguments for rejecting possible ancient Sumerian-Hungarian (Finno-Ugric) connections. However, after archaeological research from the Aral Sea through the Southern Urals and the Middle Volga to Southern Russia consistently and unquestionably demonstrated direct Proto-Asian influences, their ancestral homeland around the Oka-Kama suddenly became very uncomfortable for our Finno-Ugric ancestors, where excavations also proved the presence of Caucasian-Transcaucasian colonial settlers who had immigrated en masse in several waves.

And lo and behold, miraculously, it was precisely in these years, when the Finno-Ugric knot was also beginning to tighten — in 1962 — that Péter Hajdú discovered, based on the improbable linguistic-geographical conclusions mentioned above, that the Uralic homeland was not so much on the Kama, but rather on the lower reaches of the Ob, in the tundras of Western Siberia.

And here we have it again: in 1975, one of the leading representatives of the orthodox Finno-Ugric core (István Fodor) enthusiastically picks up and continues to roll Péter Hajdú's 1962 unprovable idea before him, diligently supported and praised to the skies for his efforts in this direction by the president of the Szeged Primitive History Working Group, András Róna-Tas (Róna-Tas: A nyelvrokonás; p. 422). At the time, we showed that Fodor, in his efforts to prove this Hajdú-like hypothesis, did not hesitate to place the Early Middle Ages civilization more than 1000 years earlier than its actual age, and to speak of Early Middle Ages cultural influences in the area between the Ob and the Ural Mountains at the beginning of the 4th millennium BC.

386
===== PAGE 383 =====

It seems that this Finno-Ugric "Drang nach Norden" was the only consequence that our Finno-Ugric-oriented historiography has drawn from the archaeological and prehistoric discoveries of Pre-Asiatic times,

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391
==== PAGE 388 =====
BOOK III
"DISCOVER ONLY TONGUE!"
Methods and results of Finno-Ugric linguistics
Motto: Hit, not your mother! Proverb.
Just dissect it, linguist! So what's the harm in losing the victim? Does this belong to you?... One thing is certain: not your mother! (János Arany: On the Orthologists II.)
==== PAGE 389 =====
INTRODUCTORY PLACE

After compiling and drafting the material for our current volume, we became acquainted with Gyula László's epoch-making work, "Our Ancient History" (1981), in which -- perhaps surprisingly at first glance -- the reader can nevertheless find a language development and paleoethnogenetic approach that is very closely parallel to the ideas we have expressed in this volume.

However, upon closer inspection of these striking similarities - and even sometimes coincidences - between Gyula László's explanations and our own ideas, anyone can easily recognize when reading our text that without the previous guidance of Professor László, primarily the in-depth study of his fundamental work entitled "The Earliest Stages of Our Prehistory", without the continuation of the numerous new, fruitful ideas raised in it, and without following the archaeological and linguistic traces disclosed there, we would certainly never have reached the stage of developing the prehistorical approach outlined in the previous volume and the paleolinguistic approach that follows below.

Therefore, we would like to draw the attention of our esteemed readers to the not insignificant circumstance that -- as evidenced by the lines of thought contained in this volume -- the conclusions we have drawn through the multifaceted examination and comparison of the historical, cultural-historical, anthropological, demographic, linguistic and other data currently available to us are very much in line with the more recent research results of Gyula László, although we only learned about them retrospectively.
From these agreements, we dare to conclude that, at least to a certain extent -- as far as our limited possibilities allowed -- we have succeeded in fulfilling the fundamental methodological principle of modern research, which requires a complex approach to controversial issues from multiple angles, consideration of all known relevant data, objective comparison of individual hypotheses, and careful consideration of their reality value.

We are releasing this volume without consciously changing anything in the text of the original manuscript, and we do not refer anywhere to the new work of Gyula László mentioned, although we could have done so in many cases, because Gyula László's findings consistently and effectively support our views, and by using them we could have rounded out our theory. We have abandoned comparisons because this makes much clearer the fact that objective, complex research, using all known data and taking into account the fundamental principles of logical thinking, is interdependent.

==== PAGE 390 =====
We therefore ask our readers to take this work in hand, preferably together with Gyula László's latest book entitled "Our Östört&netünk", and to pay close attention to the agreements, the identical or very similar conclusions and

results.
396
==== PAGE 391 =====
Chapter 1
THE THEORY OF THE FINNO-UGRIAN ORIGIN OF THE HUNGARIAN LANGUAGE AND ETHNICITY IN THE MIRROR OF MODERN INTERNATIONAL LINGUISTIC AND ETHNIC RESEARCH

Chronology of the Urdo-Finno-Ugric language family
The large number of dictionary similarities and parallels found in Hungarian and the various languages that were called Uralic (Finno-Ugric) in the 19th century, as well as the common elements of grammar and word formation, undoubtedly testify that Hungarian and the aforementioned languages were once very closely related to each other. It cannot be disputed that a clearly recognizable linguistic relationship can be demonstrated between them. We do not want to doubt the fact of linguistic relationship itself - this is emphasized at the outset, in order to avoid misunderstandings - in the slightest degree,

However, we cannot agree with the tacit understanding generally prevailing in our linguistics, which almost automatically equates the Finno-Ugric relationship and origin of the Hungarian language; namely, in such a way that the Hungarian language and ethnicity are derived from the so-called "Uralic", then "Finno-Ugric", and finally "Ugric" basic languages and ancient peoples, which peoples, separated from each other over the course of millennia and increasingly separated linguistically, would have formed the Finno-Ugric languages spoken today.

This scheme, which at first glance is indeed appealing, cannot be proven by anything. It must simply be believed, like the dogmas of the churches. We will show below that it has no solid foundation that would withstand systematic scientific criticism.
This hypothesis was put forward by Schölzer at the end of the 18th century, and then in the middle of the 19th century, fully fitting into the language family tree theory of the Indo-Germanic linguistics of that time, it was cast into a definitive, scientific axiom-like form by the so-called Göttingen school (followers of Schölzer) abroad, and in Hungary mainly by Pál Hunfalvy and -- also from Göttingen -- József Budenz. The working method of Finno-Ugric language comparison is therefore based on exactly the same two "fundamental theorems" that we have already discussed in Book I, in relation to Indo-Germanic studies. These fundamental theorems, as formulated by O. Schrader, are as follows

evenings: 1x The relationship of the Indo-Germanic languages can only be explained (!) by assuming a common Proto-language.
397
==== PAGE 392 =====
2. The assumption of this Indo-Germanic language necessarily proves (!) the existence of an Indo-Germanic ancestral people (1. Book I, p. 66, or O. Schrader: Sprachvergleichung und Urgeschichte, 155. Old.).

At the time, we pointed out emphatically that as "fundamental" as these propositions are, they are also fundamentally flawed:
1. The relationship between two or more languages cannot be explained "only" by assuming a common ancestral language.
2. No assumption can prove -- not even generally, let alone "necessarily" -- anything.
Below we will see that modern Indo-Germanic studies no longer adheres to these "fundamental principles".
Let's now take a closer look at how this idea of the Hungarian language evolved.

When Hunfalvy and Budenz began a wide-ranging, detailed, systematic comparison of Hungarian and Finno-Vogul languages, it soon became apparent that the number of Hungarian correspondences and parallels in the individual Finnish and Vogul-Ostiatk languages was very different. But the situation was similar with the correspondences between the Finnish languages.
From this phenomenon, they concluded that the individual Finno-Vogul peoples and ethnic groups (this would be the correct name, since the inclusion of the ethnonym "Ugric" among the Finnish languages is completely unjustified and arbitrary terminology, which has no scientific justification; 1. y. book, pp. 270-272) separated in stages from a supposedly unified ancestral people, and that their languages gradually separated from some kind of common ancestral language.

Thus, again closely linked to the scheme of the Indo-Germanic language family of the time -- among whose individual languages there were conditions very similar to those outlined above in terms of similarities -- the hypothesis of a unified, "Uralic" ancestral people, common to the "Finno-Ugric" and the Samoyeds, was born. This split is usually dated to AD.
These are the "proto-Urals" then -- according to the assumption -- would have split, first in the narrower sense
It is placed around 4000 BC, but rather somewhat earlier, in the second half of the 5th millennium. After the separation of the Samoyeds, it is assumed that the so-called "Finno-Ugric" ancestral people lived together or closely next to each other for a long time, and the dialects of the individual groups were more or less identical. This hypothetical linguistic state is called the "Finno-Ugric" basic language,

A "Finno-Ugric" unity, according to linguistics, has become increasingly loose over time as a result of the expansion of certain tribes and smaller or larger migrations, and approximately,
Around 2000 BC, the complete separation of the eastern tribes, the so-called "Ugrians", from the "Finno-Permian" group, which
2398
==== PAGE 393 =====

the latter itself was later divided into "Baltic-Finnic", "Volga-Finnic" and "Permian" subgroups.
The separated "Ugric" ancestral people - according to the scheme - would have formed the so-called "Ugric" unity for a long time, about 1000 years, again with a common "Ugric" basic language. Our linguistics places the disintegration of this "Ugric" unity at 1000 BC, or perhaps somewhat later, in the first half of the 1st millennium BC, when the so-called "pre-Hungarians" would have separated from the other "Ugrics", i.e. the Voguls and the Ostyaks, who are collectively called "Obi-Ugrians".
These are indeed quite impressive, but also quite astonishing chronological figures, especially if we recall for comparison one or two parallel, concrete and irrefutable linguistic historical data. Germanic studies place the time of the separation of the Germanic languages at the 4th century AD, while according to Romance studies, the Neo-Latin languages became independent between the 4th and 8th centuries AD. We admit that the difference between the parallel words and language structure of Hungarian and Vogul is greater than, say, between the word correspondences and grammatical elements of German and English, but not so much that this would make it permissible to double the time of their separation, i.e. to extend the period of linguistic development in different directions from 1500 to 3000 years. László Szabédi correctly notes in this regard in his book "The Ancient History of the Hungarian Language": "Why do Finno-Ugricists want to make us believe that Hungarian and Vogul separated from each other three and a half thousand or even three thousand years ago? The identical

The phonetic form of the words does not really support this assumption. The phonetic difference between the Hungarian name, húsz, jég, víz, him and the Vogul ndm, chus, jank, vit, chum is no greater than that between the French nom, yingt, glace, onde, on and the Italian nome, venti, ghiaccio, onda, uomo. If at the turn of the 8th and 9th centuries the French and the Italian were still quite similar,

considered one language, it is not entirely clear why Hungarian and Vogul could not have been considered one language or two dialects of the same language at the same time." (id. m. p. 31)

We cannot deal with Szabédi's assumption in more detail now. But anyway, the point of this time is not whether Hungarian and Yogul separated from each other only in the second half of the 1st millennium AD or earlier -- if indeed they ever formed a common ancestral language, which is very unlikely. The main emphasis at present is on how Finno-Ugric studies arrived at these fantastic chronological assumptions in the case of a language family whose oldest linguistic monuments are barely a thousand years old? Well, there is a good reason for this. It is known that in the Finno-Ugric and Indo-Germanic languages there are a number of words with the same meaning and parallel phonemes, which occur in almost all Finno-Ugric languages, and at the same time in most of the Indo-Germanic languages. Classical Indo-Germanic studies were still concerned about these words in the middle of the 19th century.

===== PAGE 394 =====

He showed that they belong to the basic layer of Indo-Germanic languages, and that their phonemes could only have been transferred to Finno-Ugric during the so-called Early Indo-Germanic language community, and that — due to their general distribution in the latter languages — the borrowing must have occurred during the time of the common Finno-Ugric basic language. Thus, they came to the conclusion that the Finno-Ugric language was roughly the same age as the common Indo-Germanic proto-language, and we cannot date its split much later than the time of the breakup of the Indo-Germanic language. That is, the chronological ideas of Finno-Ugric studies about the branching of the Ural-Finno-Ugric language family and the "regular" sound changes of the Finno-Ugric languages observed over time — in the complete absence of historical data and linguistic monuments — are based exclusively on the so-called "phonology" of these loanwords allegedly adopted from the Indo-Germanic languages into the Finno-Ugric language, or more precisely, on the hypothetical Indo-Germanic sound forms that 19th-century Indo-Germanic linguistics deduced using the "methods" that we presented in detail in Chapters 2 and 3 of our Book I.

In Hungarian, this means — and we must point out this circumstance, especially in order to inform our non-linguistic readers, in great detail — that the clarification of the chronology, sound development, and historical connections of the Finno-Ugric languages — precisely in the sense of the methods of orthodox Finno-Ugric studies — is impossible without a thorough examination and elucidation of the development, chronology, sound history, and historical connections of the Indo-Germanic languages. After all, the credibility of the entire chronological construction set up to explain the former unity and then separation of the Finno-Ugric languages — including the chronological views held on the temporal succession of the assumed regular sound developments in the Finno-Ugric languages, which are inseparable from all of this — ultimately depends on whether there ever was a common Indo-Germanic proto-language at all, and if so, when this basic language existed. In short, the chronology of the Uralic-Finno-Ugric language family, and indeed the "Uralic", "Finno-Ugric" and "Ugric" basic languages themselves, stands and falls on the reality value of the inferred phonetic forms of the above-mentioned Indo-Germanic base language words, and on the validity or invalidity of the 19th-century Indo-Germanic family tree theory, and on the existence or non-existence of the common Indo-Germanic proto-language. In other words, Finno-Ugric linguistics is tied to the contemporary state of Indo-Germanic studies by its own methodological fundamental theses with inextricable threads.

From all this it follows that it is a methodological necessity for Finno-Ugric studies to be responsive to any changes in Indo-Germanic linguistic and pre-historical research, to any new discoveries or to more thoroughly developed methodological approaches.

400
===== PAGE 395 =====

react most sensitively to these guidelines: immediately compare your traditional ideas about language development and prehistory with them, and make the appropriate draw conclusions and make the necessary corrections within their own field of expertise. In the following, we will see what modern Indo-Germanic linguistic and prehistory research thinks about the traditional, 19th-century common proto-language and language family tree theory, the manner and chronology of the development of Indo-Germanic languages, and the inference of proto-language root words. At the same time, we will also show to what extent the new Finno-Ugric studies adheres to the methodological principles it has laid down itself, and to what extent it takes into account the epoch-making changes that have occurred in Indo-Germanic studies in recent decades.

Some methodological considerations
After establishing the basic thesis of the language family, the next step was to deduce the already assumed "Ugric" proto-language from the Vogul and Ossetian parallels of the Hungarian language, by systematizing the sound differences found in words in such a way that they correspond to the "Finno-Ugric" proto-language assumed for even earlier times. After reviewing and comparing the corresponding sound forms of the various Finno-Vogul languages, they made the sound that occurred in the most idioms — counting them by language — the proto-language basic sound.

In other words, they made a fundamental methodological mistake right from the start. With a method based on the federalist principle of "one language, one vote", they could not come to any other conclusion than that Hungarian is a derivative of these alleged "Ugric", "Finno-Ugric" and "Uralic" roots. However, the federal principle of equal weight of provincial votes, no matter how well it works in social organizations, cannot be applied in linguistics. It is methodologically unacceptable that, since, for example, the word head (Kopf) begins with the sound "p" in all so-called Finno-Ugric languages except Hungarian, we should consider this "p" to be the original basic sound. Moreover, it would be unacceptable even if we only knew about Hungarian and various other Finno-Ugric languages that part of their vocabulary and many grammatical elements are related, but we had no more detailed information about their linguistic heritage and the historical and cultural conditions of the peoples speaking these languages. Why? - Simply because the very fact that Hungarians constitute 2/3 of the majority of all so-called Finno-Ugric peoples does not allow the conclusion that the language of this approximately 15 million-strong ethnic group, which, moreover, - almost uniquely in Europe - practically has dialects:

401
===== PAGE 396 =====

It forms a unified block, with a population of approximately 8 million, divided into 16 independent languages and more than 50 dialects, most of which show very strong differences. other Finno-Ugric peoples would derive from the alleged "basic language". Under such ethnic and linguistic circumstances -- if we want to search for a common "basic language" at all costs, we can only research this starting from the numerically dominant and linguistically uniform Hungarian language. In other words, the methodological confusion, or in Hungarian terms, the conscious obfuscation, begins with the terminology, when they talk about the "Finno-Ugric" language family, even though the only correct technical term would be "Hungarian-Finnish". In addition to this, however, there are many other, equally compelling arguments against the "Finno-Ugric" derivation of the Hungarian language. In addition to emphasizing the Finno-Ugric origin of our language, they do not usually talk about the fact that the Hungarian language monuments far precede the oldest monuments of all other Finno-Ugric languages. The first scattered words of our language appear as early as the 10th century (the tribal names and personal names of Constantine, born in Biborbanszületett, and the word "Etelköz"), while the monuments of the Finnish language appear only in the 16th century, and we have data on other smaller Finno-Ugric languages only since the 18th-19th centuries. In addition to this distribution of linguistic relics, the self-confidence with which Finno-Ugric linguistics declares that — to stick with the previous example — the ancestors of the Hungarian word "fej", which has been dated with the initial "f" sound for almost a thousand years, are those Finno-Ugric word forms with the initial "p" sound, which we can only trace from the 16th-18th centuries. What proves that there had to be a "p" > "f" sound transition in Hungarian — as they claim — and what excludes the possibility that it happened the other way around in other Finno-Ugric languages? This objection is completely justified and well-founded, even within the framework of the theory of sound laws and regular sound changes: all methodically working "sound law" linguistics establishes the "regular" sound changes believed to be temporal based on the age of linguistic monuments, and considers the sound form of the oldest occurrence as the basic sound. We emphasize: this procedure is a fundamental one that necessarily follows from the essence of the theory of regular sound changes viewed as a temporal process.

methodological requirement. However, it seems that by an inscrutable twist of fate, this otherwise generally accepted and applied methodological principle does not apply to the Hungarian language — because it seems that only by turning it off can Hungarian be derived from a "Ugric" or "Finno-Ugric" base language. Therefore - as they say - we must assume that the Voguls, Ossetians and other primitive Finno-Ugric peoples, precisely because of their primitive way of life and undeveloped social relations, were much more faithful to the

===== PAGE 397 =====

preserved the ancient sounds, like the Hungarians, exposed to many foreign influences and repeatedly thrown back in the storms of the Torténclem, In the margin of this alleged thesis of linguistic conservatism, we will only note that Bernát Munkácsi, who stayed with the Voguls in 1883-89, could not do much with Reguly's vocabulary list from 45 years earlier, because the Vogul language had changed beyond comprehension in the meantime (L. Munkácsi's travel report in the first volume of "Ethnographia" in 1890).

If we look at the historical and cultural data, we get a picture similar to the previous one. The historical role and cultural level of the Hungarians are separated by an unbridgeable gap from the social development and cultural level of most Finno-Ugric peoples; even if we compare the conditions of the Vogul-Ostjaks in the 19th century with the state-building ability, social relations, and culture of the conquering Hungarians 1,000 years ago.

Even the Finns, who are far superior in culture and numbers among the other Finno-Ugric peoples, were not able to form an independent state until recently. Our closest linguistic relatives, the Obi-Ugric people, not only lived in Stone Age conditions even 150 years ago, but — ironically — they are also completely different from the Hungarian ethnic group in terms of anthropology.

This inescapable contradiction, between the thesis of our Finno-Ugric origin and the historical reality, the objective facts, our linguistics and prehistory researchers try to explain by simply stating, in the words of József Deér (Pagan Hungarians, Christian Hungarians), that "we are forced to assume" (sic) that the Obi-Ugric people had a much higher culture in the past than in the 18th-19th centuries. However, they prefer not to talk about the anthropological difference - which is the most serious reason for excluding a common "Ugric" origin.

And we are "forced" to state that such arguments have very little to do with systematic scientific proof. The facts mentioned so far are not usually included in the confident mention of the Finno-Ugric origin of our language. It is appropriate to tactfully pass over them, yet even if considered separately, they would be very important for a truly scientific, objective and unbiased examination of the origin and relationships of the Finno-Ugric languages. However, taken together, and considered soberly and rationally, they mean nothing less than that the formation and development of the Finno-Ugric languages could not have taken place within the framework taught by Finno-Ugric studies.

Based on the demographic, linguistic, historical, cultural-historical and anthropological facts outlined above, it can be undoubtedly stated that our linguistics made a fatal mistake, a fundamental, theoretical and methodological error when it did not fully understand the far-reaching consequences of the Hungarian language in all respects.

===== PAGE 398 =====

He attempted to reconstruct the hypothetical proto-language - or rather proto-language stages - not from the Hungarian language, but primarily from the languages of various primitive Finno-Ugric minority peoples.

Considering the overwhelming numerical majority of the Hungarian language and ethnicity, the half-millennium priority of our linguistic monuments and the uniquely outstanding cultural level of our people, a language comparison that considers itself scientific, methodical, and even claims to be infallible should not have neglected the Hungarian language to the extent that it did. In the given situation, it is only by starting from the Hungarian language that a truly methodical language comparison can be carried out and the supposed ancestral language approached. Just as, for example, Indo-Germanic studies did not build its theory of language development on today's Gypsy and Afghan, but on the Old Indus, Old Persian, Old Greek and Latin languages, which have the oldest linguistic monuments and ancient cultures and were spoken by large populations.

Finno-Ugric studies, on the other hand, based its hypothetical language family tree primarily on the languages of the culturally lowest, but numerically insignificant, fragmentary peoples. Languages whose oldest relics are mostly barely 2-300 years old. Moreover, these fragments of people were discovered in a more or less advanced state of Russification in the second half of the 19th century, when systematic comparative linguistic studies and the collection of material began on a larger scale (Károly Pápai I: A vogulok és osztákköl).

These languages could therefore in no way have served as a basis for inferring hypothetical proto-languages — much less for drawing far-reaching yý-historical and ethnogenetic conclusions. We emphatically recall the testimony of Bernát Munkácsí, one of the most outstanding representatives of the Finno-Ugric linguistic and pre-historical school, who — as we have already mentioned above — did not achieve anything with Reguly's Vogul vocabulary during his 1888-89 research trip to Western Siberia, because barely 45 years later the pronunciation of the words was completely different. What probative value do the phonetic forms of Vogul words from a hundred years ago have in such circumstances in inferring the phonetics of the "Ugric" basic language, which is believed to be 3,000 years old?

The complete desolation of these languages and peoples is also shown by the fact that the 6,000 Voguls use four dialects so different from each other that they barely understand each other. Ossetian, spoken by 20,000 people, is divided into 7-8 dialects, which are also so different from each other that they are incomprehensible. The same is confirmed by Kandra Kabos' statement (Magyar Mythológia) that in 1888 Munkácsí had already heard Macken tell the Vogul creation myth, as Reguly had done 45 years earlier (1, Book II, p. 330),

404
===== PAGE 399 =====

But we must also emphasize once again the fact of paramount importance that the Ob-Ugrians are anthropologically a Paleosibirid-Mongoloid-Primeval people, and therefore anthropologically have nothing to do with the Hungarians or the actual Finnish peoples (Lipták 1977). Géza Nagy pointed this out emphatically at the end of the 19th century (Honfoglaló yseink, 1894). This undoubted fact, taken in itself, is more than enough to reject at least the thesis of a "Ugric" ancestral language: under such circumstances, it is biologically absurd to speak of a "Ugric" ancestral people and basic language that would have included the ancestors of the Hungarians. This anthropological finding compellingly suggests that we must believe those Finno-Ugric linguists who have long claimed that the Voguls and Ostyaks — similarly to the Lapps — did not originally speak Finno-Ugric, but only adopted their present-day language from their Finno-Ugric neighbors at a rather late date (1. e.g. E. Setälä: A finno-ugor népek őstörténete; G. Ránk: Völker und Kulturen Nordeurasiens).

Despite the data listed so far, some of which are very thought-provoking and some of which are downright exclusive, the authoritative representatives of our linguistics claim in a tone that does not tolerate contradiction that the Hungarian language derives from the ~~Finno-Ugric basic language~~ ~~specifically~~ ~~those~~ ~~who~~ ~~doubt~~ ~~the~~ ~~Finno-Ugric~~ ~~or~~ ~~"Ugric"~~ ~~origin~~ ~~of~~ ~~our~~ ~~language~~ ~~(we~~ ~~emphasize:~~ ~~not~~ ~~its~~ ~~relationship,~~ ~~but~~ ~~its~~ ~~origin!)~~ ~~and~~ ~~do~~ ~~not~~ ~~accept~~ ~~it~~ ~~as~~ ~~a~~ ~~scientific~~ ~~axiom~~ ~~are~~ ~~labeled~~ ~~as~~ ~~running~~ ~~madmen,~~ ~~so~~ ~~to~~ ~~speak.~~ However, in contrast to this position, and following from what has been said so far, we can rightly ask the question that Gábor Bálint formulated more than 80 years ago: "...what could be thought of someone who would want to develop the Greek language from the Albanian language, and Sanskrit from the Afghan language, and put it on a scientific basis, since they are related, and in numerals even better than Hungarian is in relation to the Eszszak languages?" (quoted from Kandra. Magy. Mythol. p. 467).

Indeed, what would Indo-Germanic linguistics say about the claim that the Indo-Germanic languages spoken by hundreds of millions of people originate from Albanian, Afghan or Gypsy, because Sanskrit, Old Persian, Ancient Greek and other ancient linguistic monuments are much older, and the cultures of these peoples were — already 2000 years ago — far above the level of education of the 19th century Gypsies and Afghans, but the latter — precisely due to their backwardness and small numbers — have preserved the most ancient word forms to this day.

Well, this is exactly what Finno-Ugric studies say about the Vogul or Ossetian languages, about the small peoples — in comparison to Hungarian. We remind you: about languages that we know for certain that they changed to the point of incomprehensibility in less than 50 years in the 19th century. We ourselves can neither follow nor accept this unreasonable argument, but we stand equally bewildered by that confident assertion.

===== PAGE 400 =====

before the conviction that anyone who rejects a completely irrational line of thought would be a dilettante or a fantasist. On the contrary, we must point out again that the real ethnic, paleolinguistic, historical and cultural-historical nature of the kinship relations between the Hungarian and Finno-Ugric languages, the former proto-linguistic relations, the processes of language development and culture — based on the weighty arguments presented above — could be examined primarily from Hungarian, as the most developed, oldest cultured language and the language spoken by the most people, in the hope of the greatest success. Let us briefly summarize our objections so far.

1. Finno-Ugric linguistics overlooks the eminently important fact that the number of Hungarian speakers constitutes 2/3 of the Finno-Ugric peoples, and when deducing the basic words of the supposed ancestral languages, it hardly takes the overwhelmingly dominant Hungarian language into account: the language of the 6,000 Voguls, divided into four strongly different dialects, weighs much more heavily in the Latvian language than that of the 15 million Hungarians.
2. Our linguistics ignores the fundamental, generally accepted methodological requirement of the language research method based on regular sound developments observed over time, that the sound forms of the assumed common Cossack state must always be reconstructed starting from the sound forms of the earliest datable linguistic monuments, and despite this, it considers the sound forms of the words of Finno-Ugric languages, which appeared a good half to three quarters of a millennium later than the Hungarian linguistic monuments, to be authoritative in determining the phonetics of the so-called "Ugric" and "Finno-Ugric" basic language words.
3. They do not take into account the huge cultural difference between the Hungarians and most Finno-Ugric peoples, but rather assume from the outset that practically all Hungarian words that are not found in other Finno-Ugric languages are loanwords. As a result, they paint a completely false picture of the cultural relations and language of the Hungarians before the conquest, since all of this is primarily measured by the 19th century Voguls and Ossetians. Despite the fact that archaeology can present numerous undoubted evidence to prove the developed social relations and culture of the conquering Hungarians; that the cultural state of the conquering Hungarians cannot even be compared to that of the 19th century Ob-Ugrians.
4. Finally, in a methodologically completely incomprehensible and even unacceptable way, they do not draw any consequences from the anthropological fact that our "closest linguistic relatives", the Ob Ugric people, belong to an anthropological species that is completely different from the Hungarians, so in their case we cannot speak of either ethnic kinship or genetic linguistic kinship.

===== PAGE 401 =====

The development of the international language domain and Ostorthenet research in recent decades
We have consciously tried to fit our thoughts so far into the linguistic and prehistorical perspective of the second half of the 19th century, into the relevant knowledge of the time, because we wanted to show that the basic thesis of Finno-Ugric studies about the Finno-Ugric origin of the Hungarian language, in the form in which it was taught and is still taught today, can be called outdated, untenable, and even downright unscientific, even by the 19th century standards and methodological requirements of linguistic and prehistorical research.

Since the beginning of the 20th century, however, the situation of orthodox Finno-Ugric studies has become increasingly bleak. The increasing number of ancient cuneiform texts discovered in excavations in the Middle East, now numbering more than half a million, has increasingly demanded a thorough revision and reassessment of the previous theories of language families and ideas of language development in European linguistics.

In the previous volumes we have repeatedly indicated that Indo-Germanic studies, albeit hesitantly and reluctantly, had already begun this inevitable revision. They realized that the theory of Indo-Germanic cultural priority could no longer be maintained. Moreover, a significant proportion of authoritative Indo-Germanic linguists and ancient historians rejected the thesis of a common Indo-Germanic basic language and a unified ancestral people. In the world of pre-Asian linguistic monuments, they saw that the Indo-Germanic languages developed relatively late: sometime during the 2nd millennium BC, and their consolidation lasted in part until the middle of the 1st millennium AD. For example, the Indo-Germanization of Italy was only completed in the 1st century BC, but even then only "officially", within the framework of the Latin administrative language of the Roman Empire; A considerable part of the people still spoke non-Indo-Germanic languages (Ligurian, Etruscan, Raetian, etc.) for a long time (1. F. Altheim: Geschichte der lateinischen Sprache; M. Pallottino: Die Etrusker). This is still evident in the Italian dialects today: a Tuscan or Ligurian hardly understands a Neapolitan or Sicilian.

But even those researchers who do not reject the possibility of the former existence of a more or less unified Indo-Germanic basic language place its age only at the middle of the 3rd millennium BC, or at most in the first half (1. Reallexicon der Assyriologie V. Band, under the heading "Indogermanen").
It also became obvious that in the case of the Indo-Germanic languages, we cannot speak of genetic language kinship or a linguistic family tree, but in all probability only of very complicated, multi-level ethnogenetic and language leveling processes, in the sense of arcal languages and linguistic alliances (Book 1, pp. 94-101).

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Since these questions are of paramount importance for the further development of our linguistics, and since we can read very little about the insights of Indo-Germanic studies in this direction in the Hungarian linguistic and pre-historical literature of recent decades, we have already seen that Hungarian research seems to be mostly based on the so-called Indo-Germanic studies.

joined the "ahistorical" school (Book 1, p. 77-81) — we cannot avoid giving our non-linguistic readers, who are completely uninformed by Finno-Ugric linguistics, a somewhat more detailed account of the trend in international Indo-Germanic linguistic and pre-historical research, which has long been concerned not only with, for example, "Indo-Germanizing" the Japanese Ainu with the help of Grimm's phonetic laws (Book 1, p. 81), or with "reconstructing" practically unknown languages without linguistic monuments into Iranian languages (IM Mayrhofer: Die Rekonstruktion des Mesdischen; cf. Book II, p. 236), or with "Indo-Germanic, Herrenschrift" - search for them throughout Eurasia. Therefore, we will now expand the brief references already presented in Chapter 2 of our Book I with further literary data. N. Š. Trubetzkoy already rejected the theses of a common ancestral language, ancestral people and ancestral homeland in 1936 (Gedanken zum Indogermanenproblem) and stated that the name "Indogermän" says nothing about ethnological issues, but is merely a linguistic concept, or even less than that, because it only designates grammatical and structural features of the language, but it cannot be extended to vocabulary, since the vocabulary of the individual Indogermanic languages differs so much from each other that we hardly know basic words that occur in all Indogermanic languages.

F. Altheim (History of the Latin Language) similarly states: "Indo-Germanism, in its origin and essence, is a linguistic concept." (id. m. p. 1) In Hungarian: "Indo-Germanism, in its origin and essence, is merely a linguistic concept." Majdigrir: "In general, linguistic and dialectal diversity is everywhere at the beginning of history. Only certain historical events helped individual linguistic or dialectal groups to achieve a position of preeminence that allowed them to overcome that diversity in favor of uniformity... Thus, Attic or Koine in the Greek world, and Latin as the language of the Roman Empire, rose to the dominance they exercised for centuries. Purely linguistic constructs such as the alleged units of Proto-Greek and Proto-Italian have not proven tenable."

Only one construct has stubbornly persisted: that of an Indo-European proto-language... The analogies show that the assumption of such a proto-language is not impossible. But Knowledge-408

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Science is not concerned with possibilities, but with what can be proven. The prerequisite for the two phenomena mentioned was an administrative and literary language that existed alongside the vernacular, correcting, nurturing, and sustaining it until the latter wholly or partially replaced it; these were events of secular significance: Alexander the Great and Hellenism, the Roman Empire with its unified form of government and culture. To assume a similar event for the emergence of an Indo-European proto-language is to presuppose what one wishes to have proven." (id. m. 2. o.)

In Hungarian: ", In general, at the beginning of history we find linguistic and dialectal diversity everywhere. Only during certain historical events did certain linguistic or dialectal groups become dominant, and diversity was eliminated in favor of uniformity... Thus the Attic language or Koine in the Greek world, Latin as the language of the Roman Empire, rose to the dominance that was then exercised for centuries. Merely linguistic constructs, such as the alleged unity of ancient Greek and the languages of East Italy, proved untenable.

Only one such construction holds out tenaciously: that of the Indo-Germanic proto-language... Analogies show that the assumption of such a proto-language does not necessarily mean impossibility. However, science should not deal with possibilities, but with things that can be proven (emphasis added). The prerequisite for the two phenomena mentioned was a state and literary language that existed parallel to the vulgar language, corrected it, nourished it, and carried it until it completely or partially replaced it; — these were events of world significance: Alexander the Great and Hellenism, the Roman Empire with its unified state form and culture. To assume a similar event in the development of the Indo-Germanic proto-language is to start from what we want to prove."

They ask: what events of world historical significance can Finno-Ugric studies reveal — around 6000 BC! — in the Kama, Ural region, and even more: in the Lower Ob tundra of Hajdú, as a result of which the unified "Uralic" proto-language would have developed? Archaeology has not yet been able to find, let alone historical events, or even the presence of humans there from these ancient times; and not even around the Kama-Ural, let alone the tundras of North Siberia.

The Paleolithic culture of Sigrir (in the middle Urals) was previously placed by some around 6000-5000 BC, but this date was based only on speculation, and in fact only the Finno-Ugric period, which is assumed to be around 6000, was confirmed by Östör-409 was an archaeological record of the "Uralic" ancient people, because there are no certain dating possibilities (1. Gyula László:

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The most recent Soviet archaeological research unanimously believes that the Ural region was uninhabited before 4000 (1. Lász- 16 Gy.: On the research of the Uralic homeland).

Altheim continues his above line of thought in this way! "There is therefore no necessity to accept a single Proto-Indo-European language. Consequently, there is also no reason to recognize a common homeland, a common race and culture... The only thing that can be demonstrated is that a number of languages, which originally did not even have to be largely similar, created linguistic commonalities through mutual, but by no means uniform, contact." (id. m. 4. old.)

In Hungarian: "There is therefore no need to assume a single Indo-Germanic common language. This also eliminates the need to postulate a common homeland, a single anthropological species and culture... All that remains to be proven is that a certain number of languages, which originally did not even have to be remotely similar, developed common linguistic phenomena through mutual, but by no means 62616168 contacts."

From this it follows compellingly that Schrader's "fundamental theses" can no longer be maintained: a.) the Indo-Germanic languages are not related. can only be explained by assuming a common ancestral language; b.) therefore the hypothesis of a single ancestral people is not necessary either. Then Altheim asks the fundamental question: "By what right can Hittite be described as an Indo-European language? Everyone knows that the discovery of Hrozný's proto-language has been crowned with a thorny wreath by the proponents of a proto-language. Indo-European features are undoubtedly present. But in inflection and especially in vocabulary, elements appear that no art can interpret as Indo-European. Moreover, Hittite would have to be considered the oldest Indo-European language handed down to us. According to the usual view, one would have to assume that it is particularly close to the reconstructed proto-language, not only chronologically, but also in its appearance, at least in its entirety. None of this is true. On the contrary, one must conclude that Hittite is among the languages that have retained the least of it. The Proto-Indo-European theory has been invoked here... Instead of simply stating that Hittite was a language that was comparatively only minimally Indo-Europeanized." (id. m. 5. old.)

In Hungarian: "By what right can we call Hetty an Indo-Germanic language? Everyone knows that Hrozný's discovery has woven a very thorny wreath for the proponents of the original language." Indo-Germanic characteristics are doubtful "410

===== PAGE 405 ===== are found in the language. But in the inflection and vocabulary there are phenomena which cannot be explained by any artifice as Indo-Germanic features. What is more: Hittite would be the oldest Indo-Germanic language handed down to us. According to the general understanding, we should expect that it is particularly close not only in time but also in form - at least when viewed in general - to the inferred proto-language. However, nothing of this is true. On the contrary: we must state that Hittite belongs to those languages which have preserved the least of it (i.e. of the inferred proto-language; - our note). They have dragged the Proto-Indo-Germanic theory forward here... Instead of simply stating: Hittite was a language which was Indo-Germanized to a relatively small extent."

Finally, we quote another very important statement from Altheim, which can be considered one of the basic pillars of modern international linguistic research, and which should be especially taken to heart and constantly kept in mind in Hungarian-Finno-Ugric linguistic history studies, since Finno-Ugric studies, compared to Indo-Germanic studies, have only very few linguistic monuments, and these also date back a good 1500-2000 years to the oldest monuments of the various Indo-Germanic languages. "Linguistic development, like all historical events, is thus divided into epochs. Distinguishing these epochs is the first task of linguistic history. This means that linguistic phenomena and their changes must be dated as precisely as possible. It is essential to avoid working with phenomena that are not fixed in time. Otherwise, we will revert to the previously practiced method of assigning commonalities between two or more Indo-European languages to the linguistic origins of the language when a historical date could not be established." (id. m. 9. old.)

In Hungarian: "Accordingly, language development, like everything in history, is divided into periods. The primary task of research into the history of language is to separate these periods from each other. This means that we must determine linguistic phenomena and their changes with the greatest possible precision in time. We must eliminate the practice of working with linguistic phenomena that are not defined in time (emphasis added). Otherwise, we will once again end up with the usual method, according to which all the similarities between two or more Indo-Germanic languages that could not be linked to time were unthinkingly referred to linguistic antiquity." We have quoted this work by Altheim, published in 1951, in particular detail, so that our readers can form a clear picture of the approach and methodology that has been gaining more and more ground among sober researchers of Indo-Germanic studies since 1945. For if Hungarian linguists | 411 €

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We read prehistoric works, and from them we learn almost nothing about these newer insights and methods of Indo-Germanic studies — not even from the latest publications. Even today, we are still walking around with the 19th century language family tree, with its precisely boxed Uralic: "Finno-Ugric" and "Ugric" basic languages, neatly packing the words with the same or parallel sound-% into the appropriate, pre-prepared boxes, of course without having the faintest idea about the real age of the words in question, their possible changes in meaning, or whether they were internal (within the language family) loanwords - in the absence of appropriate ancient linguistic monuments = we could have the faintest idea. We have seen how difficult, often insoluble, the task of determining the chronological origin of individual words is, even in the case of the numerous, relatively old Indo-Germanic languages. How much more cautiously should Finno-Ugric comparative linguistics proceed if it were truly to strive for systematic scholarship! However, we can find very few traces of such caution and prudence in our linguistics, and these too are mostly from outside, non-linguistic circles.

They come from: archaeologists, who, for example, do not understand how our word "szablya" could be a Slavic loanword, when this sword shape never occurs in Slavic finds, while weakly curved single-edged swords, i.e. "szablyá", are found in large numbers in Hungarian graves from the time of the conquest. Well, these finds, as well as the depiction of a sabre in the Pictorial Chronicle made in the middle of the 14th century, are much more compelling evidence for the Hungarian origin of the word "szablya" than the fact that — excluding the questionable occurrence in 1391 — the word can only be dated from the 15th century. However, for our linguists, who, for example, refer the Magyar-Vogul horse-keeping words to the alleged "Ugric" proto-language of the 2nd millennium BC without thinking, in the case of "szablya" only the age of the written linguistic monuments decides the question of the origin of the word (1. TESZ under the heading "szablya"). We ask: where is the "Ugric" basic linguistic monument, based on which the Vogul words related to horse-keeping can be dated to the 2nd millennium BC? Let us weigh equal

in moderation!

To put it simply, we could say that the guiding principle and methodological idea of Hungarian linguistic and prehistoric research is generally that whatever is Samoyedic, Vogul, Ossetian, Komi, Mari or Mordvin is certainly ancient — be it a word, grammatical element, folk belief, legend, myth or ethnographic object. And the more backward the illex The more Finno-Ugric the people, the more ancient their linguistic forms and traditions are considered.

This shockingly one-sided "methodological" thesis then unleashes all sorts of absurd, absurd unmethodology: "one can juggle the different Vogul, Ossetian, Mordvin, etc. languages without inhibition" 412

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blind, declaring them as elderly as the prefabricated Finnish

"Ugric, sound development" demands it. In this way, each word-form then migrates neatly to its proper place and period, and out of nothing without linguistic monuments, the 3-4000-year-old "Ugric", the 4-6000-year-old "Uralic" basic language emerges. In sharp contrast to the fundamental theoretical and methodological question of the Ugric and the so-called Uralic, we must eliminate the procedure of working with temporally undefined linguistic phenomena."

Let us return to the presentation of the latest findings of international linguistic and prehistoric research.

In his essay "Gedanken zum Indogermanenproblem", which we have already mentioned (Volume II, pp. 97-98), G. Solta goes even further than Altheim. He not only flatly denies the fiction of the Indogermanic common language and the original people, but also shows with a thorough methodological analysis that the reconstruction of the so-called Indogermanic basic language with the help of the linguistic relics available to us is theoretically impossible, even if such a language had actually existed.

R. Pittioni, the renowned Viennese archaeologist and paleo-historian, also states in the latest edition of the Propyläen Weltgeschichte (1976) Volume I (Der urgeschichtliche Horizont der historischen Zeit) that we have no archaeological evidence for the former existence of a unified Indo-Germanic primitive people, as a result of which the thesis of a common ancestral language is only a hypothesis hanging in the air. At the beginning of the European Bronze Age, in the middle of the 2nd millennium BC, we can already speak of ethnic blocks beginning to crystallize to a certain extent, but the linguistic projections of linguistics lack any concrete basis. If we can say anything at all, it is that the so-called funnel-shaped ware culture (Trichterbecherkultur) belonging to the 3rd millennium BC probably formed the racial foundations of the later European Indo-Germans. But this is also just a cautious hypothesis, and cannot even be proven, Pittioni emphasizes.

Even J. Pokorny, the co-editor of Walde-Pokorny's standard work "Vergleichendes Wörterbuch der indogermanischen Sprachen", finally came to the realization that we can neither speak of an Indo-Germanic primitive people nor of a common primitive language.

In his study entitled "The Carriers of the Culture of the Neolithic and the Indo-Germanic Question" (1949), he concludes: "The signs therefore indicate that the Indo-Germanic language arose from a fusion of the most diverse elements. The results of comparative ethnology also testify to this." (1. Pokorny: id, m. Scherer: Die Urheimat der Indogermanen, p. 311 - hereinafter: Scherer volume)

"The Indo-European linguistic unit" (1954) című értekezésében pe-dig így ír: „Indo-European is therefore a purely linguistic concept. Its

413

===== PAGE 408 =====

We call the carriers Indo-Europeans, which in itself leaves open the question of their culture and race, since language can be easily transmitted." (1. id, m. Scherer-kötet, 375. old.)

In Hungarian: "The designation Indo-Germanic is therefore a purely linguistic concept. We call speakers of such a language Indo-Germanic, which in itself leaves open the question of their culture and anthropological makeup, since the language is easily adopted."

We must give Pokorny the greatest credit for these lines, because they show that, worthy of a true scholar, even in his advanced age, he had the fortitude to declare openly and with an elevated spirit, after devoting almost his entire life's hard work to reconstructing the Indo-Germanic proto-language: his efforts, his entire life's work, were ultimately in vain, because he was chasing unprovable phantoms. His two statements above are actually nothing more than an honorable admission that his main scientific work, the Indo-Germanic etymological dictionary edited together with Walde, was indeed a work built on sand, and lacked solid methodological, archaeological, and anthropological foundations.

However, such scientists are rarer than white ravens, and therefore, if we think objectively, we must realize that we cannot expect the older generation to abandon the results of their previous work, the meaning of their entire scientific life. For most older researchers, such a thing would lead to a psychological collapse. However, we can rightly demand from the younger generations, from the scientific generation, not only to continue to vary the hypotheses of their teachers, but also to pay attention to new discoveries emerging in international science, and to take these into account when developing their linguistic and prehistoric approaches.

We have already touched upon A. Nehring's opinion (Die Problematik der Indogermanenforschung, 1954) on these issues (Book 1, I, p. 100). The essence of his view is, in short, that the formation of the Indo-Germanic languages and peoples is most likely a fusion of the languages of a "Uralic" or Inner Asian and a Caucasian or Mediterranean population, which fusion could have occurred at the earliest at the end of the European Neolithic, i.e. around the turn of the 3rd/2nd millennium BC, somewhere in the vicinity of Lake Caspian (see Scherer volume, p. 399). This explains well the numerous Finno-Ugric and Caucasian parallels of the Indo-Germanic languages. However, according to Nehring, in the case of the also very significant Semitic-Indo-Germanic correspondences, we cannot speak of direct geographical contacts, and therefore we must probably look for the source of the linguistic parallels connecting Indo-Germanic with Semitic in a third, unknown source - but in such a way that this question: from the complex, Finno-Ugric-Indo-Germanic correspondences should not be dismissed without a preliminary, thorough examination. we must rule out: it is easily possible that these too

414

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===== PAGE 409 =====

they come from the same unknown language - or languages (Scherer volume 1, p. 403)

Basic and "third, unknown language" is not only the common source of Semitic Indo-Germanic parallels 166100, but also, apparently, of the Finno-Ugric-Indo-Germanic linguistic

We can seek the ultimate origin of this agreement in this unknown ancient language.

In connection with this recognition of Nehring, we refer to Chapter 5 of our book I, where we showed that this "third, unknown language" was, according to all the ancient linguistic monuments currently available to us, the Sumerian language, or one of its very closely related dialects, within the framework of the Proto-Asiatic linguistic koine of the Early Metal Age.

Finally, Nehring shows that in the etymologies of Indo-Germanic words, for theoretical and methodological reasons, we cannot be satisfied with the fact that the words in question can be interpreted from Indo-Germanic languages. It has been proven that many Greek and Latin words, although they have a clear Indo-Germanic meaning, are ancient loanwords (Scherer volume 1, p. 408).

So, as we see, Nehring says the same thing that we ourselves claim: in the development of the Indo-Germanic languages, in addition to the so-called "Finno-Ugric", Caucasian and Semitic languages, there was a

An "unknown" language also played a very important role, which in turn was related not only to Indo-Germanic, but also probably to Finno-Ugric and

It also influenced Semitic languages. The only difference between Nehring's view of language development and our own thesis is that we are not talking about some kind of language, but about the given era, i.e. the 3rd/2nd millennium BC. Considering the cultural and linguistic state of the geographical region in question, i.e. the Káspi-Caucasian region and its immediate southern neighbourhood, around the turn of the 2nd millennium BC — when recent research (including Nehring) places the beginnings of the development of the Indo-Germanic languages — we postulate with a very high degree of probability that this language could, by all indications, have been only one (or possibly several) dialects of the ancient Proto-Asiatic linguistic koine, which (or which) must, however, have been very close to Sumerian, if not directly identical to it, according to the evidence of the massive and organic Sumerian-Semitic-Indo-Germanic linguistic similarities and regular correspondences presented in Chapter 5 of Book I.

The hundreds of Sumerian-Hungarian (Finno-Ugric) word similarities that will be demonstrated later, as well as the grammatical and structural correspondences, testify to the same thing.

Nehring does not consider the previously generally followed procedure, in which the Indo-Germanic origin of a given word was attributed to Koveik, based solely on the interpretability of the word from Indo-Germanic, to be methodologically permissible. However, we find numerous examples of this lack of methodology in Indo-Germanic linguistics, even after Nehring's emphatic warning in the 18th century.

415

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For example, in his treatise "Indogermanisch und Alteuropäisch" (1957), H. Krahe uses only the proof that these water words can all be easily interpreted from Indo-Germanic languages in his alleged Indo-Germanic origin of European water names. However, he also states: "The language family tree theory, with all its inferences, is long gone..." (Scherer volume 1, p. 429).

Let us look at some of these "Proto-Indo-Germanic" water names in Krahe's work. 1.1. The obvious reason for the phenomenon of the so-called "á"-diffusion in European water names was already shown in

Book I (p. 99): "a", "aa", "a'a" in Sumerian means "Wasser, Dunst", i.e. "water, mist". |

2. Albiz = "Fluss" (river); cf. Sumerian "al," = "glänzen" (shine) and 3. Arga = "weiss" (Greek "bíz" z , tropfen" (drip); but cf. the Hungarian "víz" "water" as well.

argos, Latin argentum); cf. Sumerian "ar, ara" "glänzen, leuchten, bleichen" (shine, shine, whiten) and "ga", "Milch" (milk).

4. Vara (ýyyý var) = Wasser" (water); cf. Sumerian "buru," = 5. Sara (óind sara) "Feldbrunnen, Bach" (field well, stream); cf. Hungarian biri, búri (ditch).

= "Fluss, Bach" (river, stream); cf. Sumerian sur" = "regnen" (it rains) and , sur," = "laufen, rennen" (run, rush); but cf. also Hungarian csurog, , csörögeteg, csermely, sűrög, surran.

The Sumerian correspondences are undoubted, but at the same time we could also observe that these "Ancient Indo-Germanic" water names show surprising parallels in Hungarian as well. So, obviously, those researchers who consider the roots of the above water names to be Ligurian, Tyrrhenian, Etruscan or other ancient Mediterranean languages, pre-Indo-Germanic, are on the right track, and against whom Krahe can only argue in support of his assumption that "...M1- could not, for example, explain the name "Alba" from its original "white" meaning as Indo-Germanic, which would belong to the Old Norse elfr 4- "albiz "river" (= Albi "Eibe")?" (Scherer volume 1, pp. 445-446). However, as we have seen, the word "albiz" can also be clearly etymologized from Sumerian, with the meaning "helles Wasser, glänzendes Wasser" i.e., bright water, sparkling water".

Wouldn't it be much more important in such circumstances to examine in what way the Sumerian albus and "albus" reached Europe, and on the other hand, "water-clear, " "biz" words, on the one hand in the form of names of water clear, white" meaning, into the Indo-Germanic languages, and on the other hand, "albus" repeating: "albus" in the form "wasserklar", i.e. Why couldn't it be Indo-Germanic?" -- Today, of course, both are Indo-Germanic, but were they the question.

416
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But these parallels also highlight that the various pre-Indo-Germanic ancient European languages, which the Greeks and Romans called Pelasgian, Lelege, Ligurian, Iberian, Tyrrhenian, etc., and whose original, real names are unknown, cannot be examined in isolation, in isolation from all other cultural-historical and archaeological data, but only in close connection with the massive Neolithic and Early Metal Age pre-Asian migration to Europe, and consequently with the ancient pre-Asian linguistic koine, primarily its well-known representative, the Sumerian language. The above surprising Sumerian correspondences undoubtedly testify that the languages of the European Neolithic and Early Metal Age had very strong connections with the Proto-Asian linguistic koine, which is not at all surprising, and is even expected, if we consider that the Neolithic European agricultural cultures not only received their developmental impulses from Proto-Asia, but were also largely ethnically shaped — at least in the southern areas — by Proto-Asian colonists (Book 1, Chapter 1).

The close linguistic connections therefore fit effortlessly, smoothly and, one might say, necessarily into the cultural history and paleoethnological picture of the past revealed and understood by archaeology.

However, recent linguistic and prehistorical research has led to further significant results. It is gradually becoming clearer that the basic linguistic and prehistorical theses of the 19th century are almost entirely unprovable, improbable, and often even based on false methodological principles, and therefore untenable. Linguistic paleontology These established methods include the so-called "linguistic paleontology", which still plays a primary and dominant role in our linguistic and paleontological sciences, to the point that it still constitutes the alpha and omega of research, so to speak. All linguistic, cultural and ancestral research studies start from this method, which is considered infallible.

Until the middle of our century, this was no different in international linguistics and prehistory. Linguistic paleontology was applied with a firm conviction in the infallibility of the method, which consists in the fact that by comparing the basic words of various hypothetical "common languages" deduced with the help of sound laws, the former state and homeland of the ancient peoples of the respective language families, thought to be more or less homogeneous, can be learned. Yet the brilliant P. Kretschmer had already spoken out vehemently against the paleontology of the Greeks in 1896 (Introduction to the History of the Greeks).

417 Ye ae
===== PAGE 412 =====
Sprache). He showed that the only difference between "cognate" words and loanwords is that in the case of loanwords we know the direction of borrowing, while in the case of words we do not. We cannot recognize "ancestral" loanwords that entered individual languages before their historical sound changes, because they have already participated in the sound changes characteristic of the receiving language, and thus they appear to be genuine, original words. — Incidentally, Kretschmer was also very skeptical about the thesis of the Indo-Germanic proto-language, although he did not yet go so far as to openly reject it. Let us observe his further line of thought.

The common Indo-Germanic words therefore only prove, he writes, that there were cultural contacts between the ancient Indo-Germanic peoples. Although Kretschmer only concluded from this that linguistic paleontology works with unmethodical means, the ultimate consequence of his thoughts undoubtedly also points to the fact that the postulate of a common ancestral language also rests on rather shaky foundations.

Kretschmer's evidence was ignored for almost half a century, as is the old, well-established custom. However, in the last 3-4 decades, a number of very remarkable critical positions, supported by thorough arguments and in-depth linguistic, cultural and archaeological studies, have appeared in the international literature, which, for theoretical and methodological reasons, reject the applicability of the method of linguistic paleontology in the research of ancient cultures and the presumed ancestral homelands of language families. Trubetzkoy broke the ice on this issue as well. In his aforementioned dissertation, after establishing that the term Indo-Germanic was a purely linguistic concept, he immediately drew the necessary conclusion: since the term Indo-Germanic can in no way be understood to mean groups of the same ethnic origin, linguistic research into the places of residence and culture of the supposed ancient people will never lead to any results.

We have already mentioned the findings of Solta 1952), Pokorny (1954), Nehring (1954) and Pittioni (1976) pointing in this direction (l. above and p. 101). In close connection with the rejection of the thesis of a common ancestral language and a unified ancestral people, they all express strong doubts about the correctness of the method of linguistic paleontology.

Linguistic paleontology is also rejected, and even declared unscientific — also in addition to categorically denying the existence of an Indo-Germanic ancestor and a common basic language — by the following renowned researchers, among others: E. Pulgram (1958), H. Kronasser (1961) and P. Bosch-Gimpera (1961). Let us take a closer look at their arguments. E. Pulgram (Indoeuropäisch und „Indoeuropäer“) demonstrates with impressive logic point by point that the Indo-Germanic Common Language is nothing
418
===== PAGE 413 =====

In no case can we consider it as a real reality and cannot claim its unity. Whoever claims such a thing must assume that "...the 6sindo European language is some kind of original creation, prehistory and preliminary head without any trace, and as if it had sprung from the ground, or a mystical folk tale. "He was conceived in the womb, or God created him. I myself cannot believe in such a thing." (id. m. Scherer volume, p. 459)
We can note that Finno-Ugric studies really believes in such things. Let us think of the firmly stated and painfully precise separation of the "Uralic", "Finno-Ugric" and "Ugric" basic languages, the corresponding ancestral peoples imagined as a real reality, and the ancestral homelands belonging to them, geographically delimited and viewed as specific territories, which, however, - unfortunately - have not yet been found. - We strongly suspect that we will not succeed with the tools of a-linguistic paleontology.
Pulgram goes on to state that no one can claim that the words of the inferred proto-language ever really existed or that they sounded as linguistics records them. Many of these proto-language formulas are simply unpronounceable, jumbled letters. In addition, if a word occurs in three or even five Indo-Germanic languages, it is not certain that the words in question existed in the proto-language, because all three or all five languages may just as well be early loanwords. Moreover, the possibility of ancient borrowing cannot be ruled out even if parallels of the word in question are found in all known Indo-Germanic languages. Incidentally, we note that such commonly occurring words are rarely found; I. Trubetzkoy ism. m. I. our book, p. 94 and this book, p. 408.)

The same applies to the smaller Indo-Germanic language groups. Not a single word commonly found in the Romance languages is of Celtic or other origin and does not exist in Latin. Therefore, if we did not have Latin linguistic monuments, and we were to reconstruct hypothetical Latin word forms from such common Romance words, they would be completely wrong. Ezekből a megmondásokból azonban az is következik, hogy a külön- féle korú nyelvekből kikövetkeztetett, csillagos" ýsindogermán alapnyel- vi szavak nem csupán teljes mértékben hipotétikusak, hanem ráadásul még különböző idýkből is származnak. "Therefore, the attempt to write something in Proto-Do-European - and it has actually been done - must lead to a confusion of language forms that differ considerably from each other in the degree of their authenticity and the time of their origin...It is a fundamental error to equate common Indo-European words with non-Indo-European words and to draw conclusions about that Original people or the original homeland of the "To build on the knowledge of Indo-Europeans, but that is exactly what has been done, indeed it seems to be the preferred research method." (Scherer-kötet, 462-463.old.)

419
===== PAGE 414 =====

In Hungarian: "For this reason, attempts to write down some text in the East Indo-European language - and indeed, this has been attempted - must necessarily lead to such confusions of linguistic forms: which differ significantly from each other in the degree of their real value and in the age of their origin... We make a fundamental mistake if we simply consider 862085 1 roepan words as Proto-Indo-European and draw conclusions from them about the ancient people or the homeland of the Proto-Indo-Europeans. However, this is precisely what they do, and it seems to be the most preferred method of research."

Pulgram then explains that so-called linguistic paleontology, for the reasons above, is a fundamentally flawed method. It cannot even be used to find the homeland, let alone to infer ethnic, social, or cultural conditions. Despite this, a mass of publications have appeared in recent decades, all of which have described the life, customs and culture of the "Indo-Europeans" using the method of linguistic paleontology. "And if it is not astonishing enough that such works are written at all, one must nevertheless be surprised at the uncritical and admiring credulity with which many researchers accept them." (Scherer volume, p. 466) In Hungarian: "And if it is not astonishing enough that such works are written at all, one can only be surprised at the uncritical and admiring credulity with which many researchers accept them."

Pulgram then takes up the various Indo-Germanic racial and migration theories. He shows that all of these are unprovable and highly improbable brain-teasers, often ridiculously primitive (Scherer volume I, pp. 466-468). He states that the proto-Indo-Germans cannot be linked to a certain archaeological culture (ibid., p. 469). , Equally naive and incorrect is the view that languages, starting from a common trunk, develop and branch out organically, like the shoots of a tree. Languages are not living organisms; they cannot sprout from a seed, be born, but they cannot grow old or die either." (ibid., p. 473)

11. Kronasser (Vorgeschichte und Indogermanistik) also firmly rejects the methods of linguistic paleontology. He emphasizes that Kretschmer's classic theorem (1. pp. 417-418) remains unchanged, and that even today we cannot reliably separate ancient loanwords from ancient cognates. But most of the time it is also impossible to determine what the objective meaning of words that occur together in individual Indo-Germanic languages was in ancient times. Furthermore, we are groping in complete darkness as to how old these common words are. In the Germanic languages, for example, we have very good parallels with the 420
Run" (foot), . "Hand"; "Fin-

===== PAGE 415 =====

get" (finger), "Zehe" (toe), "See" (lake, sea), "Mauer" (wall), "boxen" (to box), "Telegramm", "Direktor" etc., but the time of their establishment extends from the East Germanic period (Fuss) almost to the present day (Telegramm, Direktor). Of course, these — being words from such well-known languages as the Germanic languages — can be clearly distinguished in time. "Wie will man aber das um unbekannte Zeiträume verschiedene Alter von Gleichungen erkennen, die tief in schriftlose und vorgeschichtliche Zeit reichen?" (id. m. Scherer-vol., p. 491) In Hungarian: "How can we, however, recognize the real age of such parallel words, which are separated from each other by unknown intervals, and which go back deep into the time before writing and history?"

The proponents of so-called linguistic paleontology have thrown all these methodological considerations aside, Kronasser continues. The result is, of course, what we should expect from this:

..... linguistic paleontology has not been able to produce any reliable results to this day." (ibid., p. 494). Moreover, as many researchers as there are, there are almost as many kinds of — often directly contradictory — , results" were produced: "With Kretschmer's complete rejection, the completely different results of linguistic paleontology (individually and in general) are in affirmative agreement." (ibid., p. 494) In Hungarian: "Kretschmer's completely negative position is fully justified by the completely different results of linguistic paleontology (individually and in general)."

Igen tanulságosak Kronasser összefoglaló végkövetkeztései a nyelv- veszeti paleontológiáról: "Thus, I unfortunately come to the conclusion that the lauded collaboration between linguistics and prehistory within the framework of linguistic paleontology has so far only produced a number of very beautiful, but also very different, soap bubbles, from which one can choose according to taste. In my opinion, anyone who agrees with the current method is being blinded..." (u. ott, 496. old.)

In Hungarian: "Igy unfortunately I come to the conclusion that the much-praised cooperation between linguistics and prehistory within the framework of linguistic paleontology has so far only resulted in a certain number of soap bubbles, although very attractive, but at the same time equally different from each other, from which everyone can choose according to their own taste. Whoever accepts the method used so far is, in my opinion, allowing himself to be blinded. ?

"Since linguistic paleontology has repeatedly emerged for well over 100 years with supposedly refined methods and new results, I felt it necessary to discuss in detail its fragility and the lack of even a reasonably secure foundation." (u. ott, 487. old.) 421

===== PAGE 416 =====

In Hungarian: "Since linguistic paleontology has been emerging again and again for well over 100 years, supposedly with increasingly sophisticated methods and new results, I considered it necessary to discuss its fragility and lack of even a somewhat certain foundation in an exhaustive manner." P. Bosch-Gimpera also flatly denies the existence of a single Indo-Germanic, basic language and rejects the methods of linguistic paleontology. In the concluding chapter (Schlussfolgerungen) of his book "Die Indoeuropäer" (Les indo-européens), he states as a fundamental methodological prerequisite for further research that in examining the development of the Indo-Germanic peoples we must get rid of such outdated old views as, for example, the theses of a single ancestral people, a common common language, or the theories of various linguistic subgroups that arose through the alleged division of the Indo-Germanic languages, which, according to the idea of the language family tree theory, would have separated from the ancient common stem in stages over time. We must be content with outlining a rather broad framework. with, in which for a very long time we can only perceive complex folk formations, a motley mixture of different ethnic elements, and then in the next stage, to some extent, more concrete foci of crystallization, from which — after further transitional periods + finally emerged the peoples that we can now understand historically (id. m. Scherer volume, p. 510).

It seems only possible to assume with a certain degree of probability that some kind of loose pre-people formation began already in the Neolithic Age, but this can only be applied to the ancestors of the Proto-Asian, Hamitic, Semitic, Mediterranean, Uralic and, within this, Finno-Ugric groups. For some of these, namely the Proto-Asians, Hamitic and Semitic, development seems to have been faster. The European Neolithic groups developed more slowly, especially in those areas where the Indo-Germanic peoples later appeared. We can only speak of Indo-Germanic languages and peoples- yyj in the times after the turn of the 3rd/2nd millennium BC. (ibid., pp. 511-512).

Bosch-Gimpera's final conclusion in these paleolinguistic and common language formation questions is particularly noteworthy for us! "It appears that one must assume the formation of a language complex whose relationships extended in various directions and whose further development would lead to new language complexes. The languages of the Mesolithic thus represented a large general substrate from which certain elements were preserved in later language complexes, both in the Indo-European and in others. After the Indo-European language emerged, Finno-Ugric persisted as its neighbor in Europe."

422 ===== PAGE 417 =====

which at the same time formed the basic layer from which, after a certain period of time, the Rhaetian language would emerge; in the west, other substrates survived, among which the Basque language later stood out." (u. ott, 512. old.)

In Hungarian: , We must obviously start from the formation of a linguistic complex that had connections in different directions, and whose further development led to the formation of new linguistic communities. The languages of the Mesolithic thus formed a large general substrate, certain elements of which were preserved in later linguistic complexes, both in Indo-European and in other language communities. After the Indo-European language developed, Finno-Ugric remained its neighbor in Europe, which also formed the basic layer from which the Raetian language developed after a certain time. In the West, other basic linguistic layers survived, from which the Basque language later emerged."

As we can see, Bosch-Gimpera's paleolinguistic approach is very close to our position — derived from archaeological finds — according to which the earliest pre-Asian ethnic and linguistic complex was formed in the Western Eurasian region of interest to us; namely, at the end of the European Mesolithic, at the dawn of the Neolithic here, which period, however, corresponds to the Late Neolithic period in Pre-Asia, the beginning of the Copper Age there, i.e. almost exactly to the Hassuna, Samarra, Tell Halaf cultures and the H-II. stages of the Eridu-Ubaid civilization, roughly the 6th-5th millennium BC.

In Chapter 1 of Book I, we showed that these pre-Asian archaeological cultures of the Chalcolithic period already had connections extending far into the regions, from Asia Minor and Southeastern Europe to Western Iran and Turkmenistan. Consequently, this primary 65-language complex that developed in Pre-Asianism, which we call the Neolithic-Early Metal Age 610-6025101 linguistic koine, did indeed have connections in different directions" - as Bosch-Gimpera also writes. But it also fits exactly into the cultural history picture of the time that the "further development of this primitive language complex led to the formation of new linguistic communities"; Shortly after this period, in the immediate vicinity of the primary cultural area of the Pre-Asian Neolithic-Celtic Age, the first seeds of crystallization of the Hamitic, Semitic and Mediterranean language groups appear: the predynastic Egyptian cultural centers — with numerous Pre-Asian parallels in their findings — as well as the Cretan-Aegean, Balkan and Carpathian Basin Neolithic cultures on the coasts of the eastern half of the Mediterranean and in the adjacent areas, also with massive Pre-Asian cultural influences and with Pro-Asian land-based settlers immigrating en masse in several waves. We must look for them in the same period — further east, in the Caspian Sea region, around the Caucasus and Turkmenistan 2

423 ===== PAGE 418 =====

also the paleolinguistic processes preceding the development of the Uralic languages, i.e. those dialects of the Proto-Asiatic language koine that would later play a dominant role in the emergence of the very first foci of the later Hungarian-Finnish and Altaic languages. We will return to these questions - which concern us very closely - repeatedly; Now, however, let us examine more closely the more distant connections and the paleolinguistic background of Bosch-Gimpera's statement, which is surprising at first glance, that the so-called Raetian language developed from a European "Finno-Ugric" base layer. Of course, when Bosch-Gimpera calls it "Finno-Ugric", we should not think of the Finno-Ugric language family in the modern sense, i.e. something like the fact that the areas of settlement of the northern Finno-Ugric peoples once extended as far as Switzerland and Friuli. If we were to understand this reference in this way, we would arrive at the same place as Hungarian prehistory research based on the hypotheses of Finno-Ugric linguistics; That is, just as, for example, István Fodor found Voguls in the ethnicity of the Andronovo culture (Book I, II, pp. 315-321), we should also look for such "pan-Vogul" linguistic influences in Raetic. Such a thing would be absurd, needless to say, but there is no need for it, because we find the much simpler, so to speak obvious key to the question in a completely different direction.

It is worth noting that the inhabitants of the Roman province of Raetia once spoke the language that we now call Raetian. The original language itself is virtually unknown. All that is certain is that it preceded the Celts and that it later came under strong Celtic influence. Then, after the Roman conquest, as a result of the dominant influence of the Latin language, a mixed language gradually developed, which we now call Raeto-Romance or Ladin. From the sparse remains of the original Raetian language, it has been established that it initially belonged to the so-called Raeto-Tyrrhenian language group, which included Etruscan in addition to several ancient Eastern Mediterranean languages. This is the so-called "Ribezzo-type Tyrrhenian language unit", by which we mean the pre-Hellenic and pre-Latin, i.e. pre-Indo-Germanic, languages of Asia Minor, Greece, and Italy, and which, according to Kretschmer, also includes the Pelasgian language (cf. M. Pallotino: Die Etrusker).

By placing the question in this larger co-linguistic framework, we have already gone one step further. It is well known that the ancient connections of the Etruscan language clearly lead to Asia Minor, primarily to the Hurrian language, the striking parallels of which in Latin are explained by J. Friedrich (Einführung ins Urartäische) directly in terms of the strong Etruscan influence on Latin (Book I, 92. p.). On the other hand, in Chapter 1 of Book I, we also pointed out that the settlers with a developed bronze culture who appeared en masse in the Volga-Ural-Ob region in the first half of the 2nd millennium BC, coming from the south, had a developed bronze culture in the Caucasus and Transcaucasia.

424 ===== PAGE 419 =====

dekeröl, i.e., they came from the most ancient nest of the Hurrian peoples. In these colonists of Transcaucasian origin we must see the pre-Asian ethnicity (or ethnicities) whose language (or related dialects) by all indications played a decisive role in the consolidation of the languages of the pre-Finno-Ugric groups that were just emerging at that time,

Thus, the Raetian-"Finno-Ugric" linguistic connections mentioned by Bosch-Gimpera also become clear: they are based on the same Hurrian-like linguistic superstratification of Proto-Asiatic origin, which entered the Raetian language through Etruscan, and the Finno-Ugric languages through the language of the Volga-Ural-Ob region colonists of Transcaucasian origin. The development of the Altaic languages also fits into this paleolinguistic picture, insofar as the first foci of crystallization of these languages can be viewed in close connection with the peoples of pre-Asian origin who flowed north and northeast from Turkmenistan — also around the turn of the 3rd/2nd millennium BC and after — and whose first wave was decisive. played a role in the formation of the Afanasyevo culture in the Altai-Yenisei region, and their later groups — together with the Volga peoples who had meanwhile expanded to the east —

With colonists of Transcaucasian origin between the Urals and the Ob, they created the Andronovo civilization, which covered vast territories and had a unified cultural and anthropological profile, in the West Asian region.

This mass migration, which started simultaneously from two regions of the peripheral areas of the Proto-Asian primary cultural circle — Transcaucasia and Southern Turkmenistan — and repeated in several waves over half a millennium, brought a whole series of ethnic groups of Proto-Asian origin with related languages to the Eurasian steppe and forest-steppe zones, and we are convinced that the language, or languages, and dialects of these southern masses played a dominant role in the unification of the originally different languages of the language families now called Finno-Ugric and Altaic.

We must say a few words about Bosch-Gimpera's paleolinguistic approach described above. We have seen that he places the development of the Uralic language family, more specifically the Ural-Finno-Ugric language family, before the development of the Indo-Germanic languages. At this point, we must correct Bosch-Gimpera's views. However, we must emphasize that in this question he is not working in his own field of expertise — Indo-Germanic studies — but is forced to adopt the opinion of the competent Finno-Ugric linguistics, which, as is well known, places the beginnings of the development of the Uralic language family in the Mesolithic.

According to Péter Hajdú, for example, "...about 8000 years is the time depth to which we can look back, where the outlines of the Uralic basic language, albeit vaguely and imperfectly, are drawn out." (Hajdú: Preuráli nyelvi szópakatok; Magy. űstört. tan, 157, p.) And say this about a language family whose oldest linguistic monuments are barely a thousand years old, but these are only found in Hungarian, whose Finno-Ugric or Uralic origin is more than

425
===== PAGE 420 =====

is doubtful. But this is not even that important. Much more important is that it continues to profess, without any changes, the 19th-century Uralic-Finno-Ugric language family chronology, which at that time was determined exclusively on the basis of the phonetic forms of loanwords supposedly adopted from Indo-Germanic languages into the Finno-Ugric base language, i.e. on the basis of the phonetic forms of words derived from the supposed ancient Indo-Germanic and Indo-Iranian or Iranian base languages. Well, these ancient base languages have since been revealed to be not that ancient, but at most only date from the 2nd millennium BC, and modern Indo-Germanic studies have even established that the Indo-Germanic languages did not develop from some kind of common base language, but through complex processes of language leveling. Not to mention the fact that, according to recent research, the reconstruction of the phonetic forms of the words of the ancient language is methodologically impossible. We have presented all these new insights with ample quotations on the previous pages.

It seems that Professor Hajdú unfortunately only aims his paleolinguistic telescope at the Arctic Circle, and does not even vaguely see other areas of our Earth. Not even in the perspective of 8,000, or even 4,000 years. Moreover, it seems that even the relevant foreign literature of the last 3-4 decades remains outside the field of vision of this instrument that looks back 8,000 years.

In other words, Bosch-Gimpera only places the development of the Ural-Finno-Ugric languages before the Indo-Germanic ones because he unsuspectingly adopts the findings of Finno-Ugric studies, and — not being his field of expertise — he is unable, and does not consider it necessary, to verify their actual reality. He cannot know that Finno-Ugric linguistics has essentially not developed at all for a hundred years, and our linguists still hold to the language family tree theory that he categorically rejected, but linguistic paleontology, which has also been recognized as unmethodical and unscientific in the meantime, still represents the net continuation of science in our country, and moreover in its most ossified form. He believes in good faith that, in parallel with international Indo-Germanic studies, Finno-Ugric linguistics has also adopted the newly developed, much more well-founded than previous methods of language development and prehistory research, and By careful, objective application of these, he came to the conclusion that in the span of 8000 years, the outlines of the Uralic basic language are already emerging. He cannot assume that all these new methodological principles are swept aside by Finno-Ugric studies with a gesture of superiority, not only in the Uralic or Finno-Ugric, but also in the Indo-Germanic context, and that our linguistics accepts the theses of that ahistorical school of modern Indo-Germanic studies, which has already "Indo-Germanized" the Ainu, and whose research methods and "results" are now practically ignored by sensible foreign scholars, as is also clearly evident from the above quotations.

426
===== PAGE 421 =====
However, Hajdú still only repeats what he learned 35-40 years ago: he searches for the supposed similarities of a few tree names with the "beech tree" theories that were "Ural" ancestral homeland (cf. fashionable in Indo-Germanic studies a good 40-50 years ago.
As a necessary consequence of this ahistorical approach, in our linguistic and prehistory research, the right hand does not know what the left is doing. A very good example of this is János Harmatta's dissertation in the volume "Hungarian Prehistoric Studies" (Iranians and Finno-Ugric peoples, Iranians and Hungarians), where the "Finno-Ugric"-minded Harmatta has already forgotten on page 169 what the Iranologist Harmatta said on page 168.

168. old.: In Indo-European linguistics, the developmental model of the language family tree theory is proving increasingly unusable. The previously proposed linguistic branches Of these, only the existence of Baltic-Slavic and Indo-Iranian is currently under debate. It is clear, however, that even in these cases the concept of linguistic unity can only be considered to some extent as a linguistic abstraction going backwards in time, but if we think in terms of ethnic, historical and linguistic geography, we must consider the family tree approach to be incorrect in these cases as well.

These are sober, considered lines: we can see that they correspond in every way to the new linguistic picture that we ourselves have described above. What is striking after this is the complete inconsistency and lack of method with which Harmatta apodictically states on the following page (169): "The separation of the Indo-Iranian tribes from the other groups of Indo-Europeans... occurred approximately i.e. 5000."

There are only two possible cases here: Harmatta either did not understand anything from the new international literature he read — which, however, we cannot assume he did — or he consciously failed to draw the necessary conclusions from the new insights of Indo-Germanic studies, because the from Indo-Germanic languages, which moreover hardly had a common base language, much less a language family tree, it would be quite difficult to transfer the words "Ösiráni" into the hypothetical Finno-Ugric base language in the 4th-3rd millennium BC, the discussion of which alleged borrowings and their division into imaginary chronological groups is the subject of Harmatta's article in question. In other words: since Finno-Ugric linguistics, based on the language family chronology of 19th century Indo-Germanic studies, still places the age of the Finno-Ugric base language between approx. 4000 and 2000 BC, from Harmatta's point of view, the Indo-Iranians must have separated from the Indo-Germanics around 5000 BC, even if Indo-Germanic linguistics itself has long since stopped teaching this, which — to be complete — Harmatta himself explained a page earlier,

427
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===== PAGE 422 =====

Yet, in our humble opinion, it would be precisely Professor Harmatta's task - as a renowned Iranologist, he has the greatest competence and the most opportunities for this - to emphatically draw the attention of Finno-Ugric linguistics to the enormous changes in perspective that have occurred in Indo-Germanic studies in recent decades, to the drastic reduction in the chronology of the Indo-Germanic languages, and to point out that under the changed circumstances, the traditional chronology of the Ural-Finno-Ugric language family can no longer be maintained.

However, it seems that Harmatta has — unfortunately — joined the "ahistorical" trend of Indo-Germanic studies: at least this is evidenced by the fact that — in addition to the above incomprehensible inconsistency — he has recently been trying to identify "Indo-Germanic" loanwords in the Sumerian language, using methods that are similar in every way to the Indo-Germanization of Ainu (1). Harmatta: Notes on the evolutionary history of domestic mammals of Central and Eastern Europe, 1971). Returning to the paleolinguistic issues discussed above, we take the position that the language community called Uralic and Finno-Ugric did not develop before the Indo-Germanic language group, but rather that the originally different languages of both language families, after the consolidation of the Hamito-Semitic and Mediterranean languages, became equalized over a considerable period of time through the superstratification of one or another dialect of the Proto-Asiatic linguistic koine, in direct proportion to their greater distance from the primary Proto-Asiatic proto-linguistic nucleus, i.e. in close parallel with the archaeologically clearly demonstrable fact that the Proto-Asiatic cultural and ethnic expansion penetrated in waves and periodically further north and further north, and in a northeasterly and northwesterly direction. In other words, it first reached the areas of the Hamito-Semites and the Mediterraneans, and from there it spread further to the ancestors of the Indo-Germans, Finno-Ugric, and Altaic peoples.

This picture of global language development viewed in relation to East Asia also fits very well with the well-known linguistic and prehistoric fact that among the Indo-Germanic languages and peoples, those whose pre-ethnicities were closest to the Proto-Asiatic cultural and proto-linguistic core crystallized first — in the 2nd millennium BC —: the Nesi (Hittite), Luwian, and Palae groups in Asia Minor, the ancient Greeks in the peripheral regions of the Cretan-Aegean-Helladic cultural circle of Proto-Asia Minor origin, and the Iranians and Indians in the adjoining areas of the West Iranian-Turkmenist civilization, also originating in East Asia, somewhere between the Caspian Sea and Afghanistan (a more precise definition is not yet possible with our current knowledge). It should also be noted that the order of appearance of the oldest linguistic monuments of the aforementioned Indo-Germanic languages also indirectly strongly supports this view.

428
===== PAGE 423 =====

Methods of the Orthodox Direction of Indo-Germanic Studies
We do not want to fall into the inherent error of exclusively Finno-Ugric linguistic and pre-historical research, into one-sidedness. Let us therefore also learn about the views of some authoritative representatives of the opposing camp on these paleolinguistic and pre-historical questions. Let us look at the arguments of the school that he calls the ahistorical direction (under the heading "Indogermanen"; Vol. "Reallexikon der Assyriologie" V). The authors already mentioned also belong to this group: E. Meyer (1. Book I, p. 96) and partly H. Krahe 6. Book I, p. 99) and this book, pp. 416-417).

W. Brandenstein (Die Indogermanenfrage, 1962) tries to refute the position and linguistic phylogenetic theory of Trubetzkoy and his followers, which rejects the common Indo-Germanic common language, by saying that the Indo-Germanic roots show affinities in many cases: let us just think of Pokorny's "thick" Indo-Germanic etymological dictionary. "And (the languages) hardly share roots," he notes (1st ed. Scherer volume, p. 527).

That is, he simply argues with unproven statements, the truth of which we have already learned in the previous sections. Kretschmer, Trubetzkoy, Altheim, Solta, Pulgram, Kronasser, Bosch-Gimpera, not with empty statements, but through thorough methodological analysis, have convincingly proven that the separation of ancient loanwords from inherited words is methodologically impossible. And it is almost bordering on the ridiculous that he refers to the "thick" etymological dictionary of the very same Pokorny, who recognized 13 years (1949) before the publication of Brandenstein's present study (1962): "...Indo-Germanic arose from the fusion of the most diverse elements." (1. p. 413)

However, for the representatives of the ahistorical school, all these new investigations are not authoritative, they simply "establish" that "roots are hardly borrowed" by languages, the Proto-Indo-Germanic roots -- taking Brandenstein's. "thick" phantom words previously grouped together in Indo-Germanic etymological dictionaries are therefore all genuine, original Indo-Germanic words. Well, these originally own examples! -- look like this in reality:

> sei"; in Aryan languages "werfen, schleudern" (throw, hurl), the rest and in Indo-Germanic it is "säen" (sow; i.e. grain).

Cf. Sumerian "si," = "werfen" (throw, throw); , su," = "ausschütten" (scatter, pour out); , sur" = "ausschütten"(the wedge-shaped sign is the same as the Ek-shaped sign of >Suy); but cf. also the Hungarian , szór" (zerschmeissen, also with the verb ausschütten.

a aidh"; "burn (me).

Cf. Sumerian "ud, ed" = "Sonne, Licht" (Sun, light) vid," = "hell, glänzend" (bright, shiny). Note 429

===== PAGE 424 =====

also that this inferred root "aidh occurs only in Greek and is absent from other Indo-Germanic "brennen", and in Latin it is only found in a figurative sense: "aedes" (Temple), the languages. The truly common Indo-Germanic words "Feuer, brennen", *egnis and "pur, also have Sumerian parallels: "ug" = "Licht" (light) ; , bar" = "brennen, Feuer" (sky, fire). Brandenstein then explains at length that although Trubetzkoy's thesis, according to which the Indo-Germanic language alliance developed in the area between the Caucasian and Finno-Ugric languages, is well supported by the parallels between Finno-Ugric and Indo-Germanic languages, which Collinder considers so significant that he assumes a historical relationship, is contradicted by the statement of 1130687 that the horse was first domesticated in the area of the Tripoli culture. The still unified Proto-Indo-Germanic peoples must have lived near this culture, he writes, because according to the word "ekuos (16), the horse was known even in the era of linguistic unity. However, the argument is fundamentally flawed. It is in no way understandable why Hanéar's above statement should contradict Trubetzkoy's thesis.

After all, Trubetzkoy also claims that the Indo-Germanic languages developed somewhere in the region between the Caspian Sea and the North Sea, in a wide strip of contact with the Finno-Ugric language area in the north, and the Caucasian and Mediterranean language area in the south (L. Trubetzkoy, ed. Scherer volume, 220-221, pp.). After this, it seems that Brandenstein does not even know the geographical location of the Tripoli culture, otherwise he could not argue in the above way. The Tripoli culture, as is known, extended east, northeast and north, starting from the northeastern part of present-day Romania, all the way to the areas beyond the Dnieper, to the Kiev area, Podolia and Galicia. Its population was precisely that group of Mediterranean origin that migrated from the Balkans, which -- together with the Caucasians -- according to Trubetzkoy's idea contributed from the south to the development of the Indo-Germanic languages. In the north, however, we find the ancestors of the Finno-Ugric peoples, whether we consider the Oka-Káma-Bjelaja region as their homeland according to the traditional view, or whether Ladislaus

We accept the much more thoroughly supported theory of Gyula (1961) about the much more southern ancestral dwellings of the Proto-Finno-Ugric peoples. So where is the contradiction here? Apparently only in Brandenstein's argument. Not to mention that the common Indo-Germanic word tekuos, on the one hand, according to Kretschmer's still valid thesis (l. above, pp. 417-418), can also be an ancient loanword, and on the other hand, since when does a commonly occurring animal name prove that the Indo-Germanics could only have known the animal in question in its domesticated form?

We have a lot of archaeological data that shows that in prehistoric times, Eurasian wild horses were hunted and eaten,

430

===== PAGE 425 =====

I Moreover, in addition to all these objections, it must also be said that it is very doubtful whether the domestication of the horse really occurred for the first time in the Tripoli culture. The depiction of a horse carved on bone from Susa, dating from about 2800 BC, which Hancar himself mentions in his work in question (Das Pferd in prähistorischer und früher | historischer Zeit), speaks strongly against this, however, he unfortunately does not recognize the paramount importance of the find.

On the other hand, the Sumerian chariot depictions and chariot finds from the first half of the 3rd millennium BC, which, according to the general opinion of researchers, were drawn by wild donkeys (onagers), are definitely considered by M. V. Oppenheim -- who was also an internationally recognized horse expert (hippologist) -- to be horse-drawn chariots. He emphatically points out that even on the ceramics he discovered from Tell Halaf dating from the 5th-4th millennium BC, we find horse depictions, as well as drawings of wheeled chariots, which he also believes were drawn by horses (l. Oppenheim: Der Tell Halaf; cf. also Müller-Karpe: Handbuch der Vorgeschichte II. B. Jungsteinzeit, p. 59). Oppenheim also emphasizes that he would like to see a soldier who entrusts his life in battle to stubborn, unpredictably reacting donkeys. Donkeys cannot be used to pull tanks, he states (see Oppenheim: Der Tell Halaf).

Oppenheim's opinion is supported not only by the discovery of the Susa horse-breeding statue of around 2800 BC, but also by the fact that, due to the decayed state of the bone remains, paleozoologists have in several cases still been unable to decide whether the equine bones found in the Near East chariot finds come from onagers or horses (l. Müller-Karpe: id. m. III. B. Kupferzeit, p. 697). Accordingly, the relevant manuals also speak cautiously of "equine bones" in most cases.

Marija Gimbutas also works in the spirit of 19th-century Indo-Germanic linguistics and prehistory research. She is one of the most prominent proponents of the theses of a unified Indo-Germanic ancestral people and a common ancestral language, as well as the continental-scale, almost Eurasian-wide cultural-creating role of the various Indo-Germanic ruling strata (Herrenschicht) that allegedly broke away from this ancestral people and migrated far in all directions in recent decades. With her "Pänkurgän" theory, she tries to regain the wind in the sails of traditional Indo-Germanic cultural priority ideas - which had already become rather weak in the second half of our century.

Gimbutas attempts to demonstrate the Indo-Germanic people from the perspective of archaeology, in his essay "Die Indoeuropäer; Archäologische Probleme" (1963); he claims that the finds of the ochre grave (pit grave or kurgan) culture of Eastern Europe from the 3rd millennium BC are the remains of a more or less unified Indo-Germanic people, -- It should be noted at the outset

431

===== PAGE 426 =====

However, we must point out that Gimbutas projects this 3rd millennium archaeological culture back to the 4th millennium, and even extends it to West Asia, although we have no concrete archaeological data for this. - Then this culture 2.

In its millennial successors -- in Eastern Europe the catacomb tomb (Kammer-grab) and beam-frame (Balkengräber) cultures, and in Western Asia Andronovó (ca. 1700-1200 BC) and Tazabagjab -- he believes that he discovered the

The ancient Indo-Germanic people had already split into several separate blocks, each with its own ethnic and language groups (Indo-Iranian, Greco-Latin, Balto-Slavic, Celto-Germanic).

Incidentally, we can see from this how far Hungarian Finno-Ugric studies and Hungarian Iranology have fallen behind the international scientific standard: even the orthodox Indo-Germanist Gimbutas only reckons with separate Indo-Iranian peoples and languages in the 2nd millennium BC, while Harmatta already speaks of an independent Indo-Iranian branch around 5000 BC, and not long later he speaks directly of the "Osiráni" language (Book I, p. 427 and below, p. 552).

Even from this brief outline, anyone who is not completely uninformed about Late Neolithic and Early Metal Age archaeological cultures can see that Gimbutas -- obviously for the sake of his thesis -- consciously falsifies the data of the Late Neolithic and Early Bronze Age archaeological chronology of Eastern Europe and Western Asia.

First: in the 4th millennium BC there was no "kurgan" culture" in Eastern Europe, but a primitive Mesolithic or Subneolithic, in some places with just beginning animal husbandry (1. Jettmar et alii: Geschichte Mittelasiens; Müller-Karpe: Handb. d. Vorgesch. II, B. Jungsteinzeit; Narr: Handbuch der Urgeschichte II. B. Late Stone Age and Copper Age).

Secondly: in the steppes of West Asia in the 4th millennium -- but largely even in the 3rd -- only sparse Mesolithic, pre-Earth-like fish-hunting groups were nomadic (1. Hanéar: id. m. 1956; Jettmar et alii: id. m.; Propyläen Kunstgeschichte XII. B. Frühe Stufen der Kunst),

Thirdly: the lowest horizon of the Ochre-Siro culture, the 3rd millennium BC pit-grave culture (Altgräberkultur), had a long-headed Paleo-European population with a receding forehead and a strong brow ridge, i.e. with a slight Neanderthal influence. Only towards the end of the 3rd millennium BC does a shorter-headed "plump Europid" ethnic group appear from the south, simultaneously with the population of the Afanasevo culture -- also from the south -- of the same anthropological type. At this time, however, a distance of several thousand km still separates the anthropologically similar ethnic groups of the late pit-grave culture of southeastern Europe and the Afanasevo culture of the Altai-Yenisei region (l. Grjasnow: Südsibirien, as well as our book I, pp. 30, 49-50). Only in the first half of the 2nd millennium, with the continuous mass migration from the south,

432

===== PAGE 427 =====

As a result of immigration, anthropological uniformity develops in these areas, with the Andronovo culture uniformly dominated by the "plump Europid" population (l. Hanéar: id. m. 1956; Jettmar yý alii: id. m.; Grjasnow: Südsibirien). At the same time, the population of the 2nd millennium horizon of the ochre grave culture, the so-called catacomb grave culture, is also mostly short-headed (l. László Gy.: id. m.1961).

Anthropologically, there is therefore no global homogeneity -- even in the 3rd millennium -- that Gimbutas' theory would require, even if it applied to the 4th millennium.

Moreover, even ethnic continuity is lacking, because, as we have seen, practically the entire population was replaced between the beginning of the 3rd millennium and the middle of the 2nd millennium.

So, in these areas, if we are to look for pre-Indo-Germanic people at all costs, we can only find them in the 3rd millennium BC, in the form of the long-headed people of the early pit grave culture.

We could find it among the Paleo-Europids and the Kelteminar-like fishing-hunting groups living in the Mesolithic cultural stage, nomadic in Western Asia; and in the 2nd millennium BC, possibly in the Tazabagjab group located on the southern edge of the Andronovo cultural circle, which was in all likelihood a further developed successor of the Kelteminar culture of the 3rd millennium.

After these general objections, let us examine Gimbutas' ideas in more detail.

In the introductory pages of the study, Gimbutas discusses the Indo-Germanic ideas of Bosch-Gimpera's book "Les indoeuropéens" on the development of the Indo-Germanic people and language, with the assumptions whose conclusions we have already presented above (1. pp. 422-423). Gimbutas originally based his present study on Bosch-Gimpera's book.

intended it as a review, but then expanded it considerably, including his own "kurgan" theory. It will therefore be instructive to compare the reasoning and evidentiary apparatus of the two researchers.

Gimbutas primarily explains that Bosch-Gimpera's view cannot correspond to reality, especially since, according to him, various preliminary foci of Indo-Germanic people can be found in almost all Neolithic European archaeological cultures - except for the southern areas.

Now, if we consider that in the last two millennia, predominantly Indo-Germanic-speaking peoples have lived in Europe and still do, it is obvious that their ancestors can be suspected to be in Europe. There is therefore no improbability in Bosch-Gimpera's view that he is looking for the initial foci of the masses who have been populating Europe for 2000 years, speaking mostly Indo-Germanic languages, in Europe.

Explaining everything in terms of migrations is not a very convincing argument. Especially since no archaeological traces of these supposed "Indo-Germanic" migrations can be found. In addition, Bosch-Gimpera's assumption must also be considered logical and obvious because if the recent decades

433
===== PAGE 428 =====

If we take into account the results of his linguistic and prehistoric research — unlike Gimbutas — then we must also expect that, starting with the end of the Neolithic, during the European Bronze Age, certain inflectional linguistic foci developed as a result of certain language equalization processes among various ethnic groups, in different geographical regions and at different periods, which then grew appositionally through further contacts and smaller or larger — but of course not continental-scale — migrations, and extended to larger areas. Adding to this the massive inflecting linguistic influences of first Attic, later Hellenistic Koine, and then the Roman Empire in Mediterranean Europe and the western half of East Asia, which prevailed for a good 8-9 centuries, and the similar role of the inflecting dominant language of the Old Persian and Middle Persian empires in Southwest Asia, we can roughly outline the process of the final consolidation of the languages called Indo-Germanic, which — we emphasize — according to recent research did not necessarily originate from a single nucleus, but rather through some kind of migration of peoples.

... spread from their "ancestral homeland"

We readily agree with Gimbutas's criticism of the following objections. Namely, that Bosch-Gimpera was obviously fundamentally mistaken when he sought the ancestors of the Indo-Iranians in the Caucasus and migrated the Tocharians from the area of the Tripoli culture to the Hoang-Ho tributary. However, these are questions of detail that do not diminish the global significance of Bosch-Gimpera's approach, the correct position he took on general paleolinguistic issues. But otherwise, these misapprehensions are undoubtedly only reflections of the mass of relevant literature, the oppressive weight of which is often very difficult to get rid of. The vast majority of publications dealing with the two aforementioned questions have been repeating the same thing almost without change for many decades: almost all researchers derive the Indo-Iranians from the Caucasus-Káspi region, and the Trypillian origin of the Tocharians has also been among the most popular hypotheses for quite some time (L. e.g. E. Meyer: Die Indogermanenfrage; described in our book I, pp. 96-97).

However, the Caucasian origin of the Indo-Iranians has been refuted by archaeology. No trace of their presence there or their alleged migration through the Caucasus has been found (I. Hancar: Urgeschichte Kaukasiens; Burney-Lang: Die Bergvölker Vorderasiens).

Regarding the Tripolian origin of the Tocharians, we recall what was said on pages 29-30 and 96-97 of Book I: the common source of the parallel cultural phenomena of the Tripolian and the eastern Yang-Shao civilizations can be found in the civilizations of the Proto-Asian period, primarily in the Eridu-Ubaid culture (1. also Kussmaul: Frühe Nomadenkulturen in Innerasien).

434
===== PAGE 429 =====

However, in the following pages of his study, Gimbutas himself falls into the same mistake, which he sharply criticizes as Bosch-Gimpera. In fact, even more so, while Bosch-Gimpera repeatedly emphasizes that the picture he is trying to outline cannot be considered even a thesis, but only a vague possibility, Gimbutas continues to declare his much vaguer, more unfounded, and often even diametrically opposed ideas to the known archaeological data, as if they were indisputable, proven facts.

"Anyone interested in these questions can easily compare the two articles in Scherer's collected volume (Die Urheimat der Indogermanen) and see for themselves how cautiously and carefully Bosch-Gimpera writes, and how confidently, sometimes almost arrogantly, Gimbutas strikes a tone."

For example, when he apodictically states: "That the catacomb-burial — Andronovo-Tasabagjab block belonged to the Indo-Iranian-speaking peoples, the later development of the culture makes it certain." (1. id. m. Scherer, 542, p.)

We have already described what ethnic and cultural relations we can archaeologically prove between the pit grave and Afanasyevo cultures in the 3rd millennium, and then between the catacomb grave, Andronovo-Tasabagjab and Volga-Ural region (Balanovo etc.) Early Bronze Age cultures in the 2nd millennium. And among the relevant finds we found countless parallels with Pre-Asian, Transcaucasian, Kuban-Maykop and South Turkmen, but, surprisingly, nothing that would even possibly point to Indo-Iranians, let alone prove such peoples. We have seen that the bulk of the populations of all these cultures were agricultural settlers of southern, ultimately Pre-Asian origin.

Likewise, the further development of the cultures in question does not "prove" any Indo-Iranians, because the Iranians only first appear in Western Iran at the end of the 9th century BC, and the Indus around the Indus River at about the same time. Not to mention the fact that many researchers do not even accept the hypothesis of a unified Indo-Iranian ancestral people, but consider the Indus and the Iranians to be separately developed ethno-linguistic formations (IM Semper: Rassen und Religionen im alten Vorderasien; Trubetzkoy: id. m.; J. va- Jamint Harmatta's lines quoted above).

Let us now examine in more detail Gimbutas' basic archaeological thesis, which builds on his further conclusion. On map no. 1 (Scherer- volume, 545, page.) he presents the archaeological cultures of the Eurasian steppe and the Caucasus region in the 3rd millennium BC. According to the map, the North Pontic or We culture (Ochkersiros or Gódórsfos culture) extends from the Dnieper to the Don, in some places to the middle Volga, the Kuban, Caucasian and Transcaucasian areas south of the lower reaches of the Don and Volga-435

===== PAGE 430 =====

In the Transcaucasian Copper Age, up to and including the Van River, we find the Transcaucasian Copper Age culture (early Transcaucasian culture, or in Soviet literature the Kura-Araxes culture), and finally east of the Volga, in Western Siberia, in the northern half of Turkmenistan, and in Kazakhstan up to the Altai region — including the area of the Afanasyevsky culture — the Kurgan culture.

This 3rd millennium West Asian "Kurgan culture" existed only in Gimbutas's imagination, but not in reality. The more developed Afanasyev culture, which already used copper and was partly agricultural, appeared on the northeastern edge of these areas only in the last quarter of the 3rd millennium, and according to the data available to us, it was the creation of colonial swarms expelled from the area of the South Turkmen cultures of pre-Asian origin (Anau, Namazga Tepe, Altin Tepe, etc.) as a result of overpopulation (I. Hanéar: Das Pferd...; Grjasnow: Südsibirien; Stawiski: Mittelasién - Kunst der Kuschán; and our book I, pp. 30-31). The vast area between the Volga-Ural and the Altai remained in the Mesolithic-Subneolithic fishing-hunting stage for a good half a millennium (L. Jettmar et alii: id. m.; Propyläen Kunstgeschichte XIII. B.; Wiesner et alii: Die Kultur der eurasischen Völker — Handbuch der Kulturgeschichte), and only around the 18th-17th centuries BC, during the formation of the Andronovo civilization, it joins the development, after the eastern expansion of the Kuban-Catacomb civilization (this is the real Kurgan culture) and the Early Bronze Age cultures of the Volga-Ural region of Caucasian origin (Balanovo, etc.), the westward conquest of the Afanasyevo civilization, and the northward migration continuing from southern Turkmenistan also filled these areas of the West Asian steppe and forest-steppe with a population of southern origin.

Thus, just as he had previously placed the ochre grave culture of the 3rd millennium BC in the 4th millennium BC, Gimbutas unhesitatingly projected the archaeological, ethnic and cultural relations of the Andronovo period back to the 3rd millennium BC, in other words, he pushed back the chronology of the Andronovo culture by a good thousand years.

In this view, one can clearly recognize the same outdated position, long outdated due to the recent excavations in Turkmenistan and South Siberia, which Fodor, following Chernyvetsov, also represents in his book "Verecke famously strikes..." (1. II. of our book, pp. 310-315). However, we must write to Gimbutas's credit — unlike Fodor — that he wrote his dissertation in 1963 in America, when the results of the recent excavations in Turkmenistan and West Asia were not yet widely known, especially not in the Western Hemisphere. That is why Gimbutas also starts - presumably also following Chernyvetsov - from the Kelteminari culture, which was thought to be advanced, and then from its 2nd millennium successor, the Tazabagjab culture, which, however, was later revealed to have developed in the southern periphery of the Andronovo cultural circle, on the marshy shores of the Aral Sea, and judging from the finds, was of a lower

436
===== PAGE 431 =====

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It stood on a higher level of culture than the contemporary, actual Andronovite culture (1. A. Belenitzky: Zentralasien, p. 46).

As a result, Gimbutas' archaeological-prehistoric construction, built on outdated foundations, can only claim our attention as a curiosity of the history of science. It is a very good example of how the supporters of Indo-Germanic cultural priority immediately use even a few scattered, barely evaluable archaeological data uncritically and unilaterally to build continental-sized Indo-Germanic air castles and "Herrenschicht" migration theories on them. In support of this opinion, we will briefly present some of Gimbutas's extraordinarily fantastic "statements",

For example, he writes about the 0th and 3rd millennium BC royal tombs excavated at Alaca Hüyük and Horoztepe in northern Anatolia that they are "clearly Indo-European". However, no traces of Indo-Germans can be found in Anatolia in the 3rd millennium. Accordingly, in relation to this far-fetched claim of Gimbutas, Burney and Lang, both of whom are among the leading authorities on Anatolian-Transcaucasian archaeological research, emphatically state: "This thesis, however, must be rejected as completely unfounded." (Die Bergvölker Vorderasiens, p. 101) In Hungarian: "We must instruct you, we

These "Kurgan people" (Kurganleute) — as Gimbutas calls the imaginary population of his undatable 3rd millennium Eurasian Kurgan culture — according to him, at the beginning of the 2nd millennium BC they flooded all of Anatolia (Hittites), Mesopotamia (Mitanni-Hurrians, Kassocks), and Europe up to the Rhine, Denmark, and Greece. In this view, Gimbutas considers the Kassites, who have been detected in the Zagros Mountains since ancient times and then lived in Babylonia in the 2nd millennium, as well as the agglutinative-speaking Hurrians, who have been found in Northern Mesopotamia since the middle of the 3rd millennium, simply Indo-Germanic, Kurgans, without any explanation or proof, although he himself notes about the Kassites that Elamite records mention them in the Zagros Mountains, in present-day Luristan, as early as the 3rd millennium (1st ed. Scherer volume, p. 551), 5 at the same time

Even according to his own thesis, the "Kurgānōsi" only penetrated into East Asia after the turn of the 3rd/2nd millennium BC. Moreover, we cannot even speak of the Hittites as a whole simply as an Indo-Germanic people, because out of the 6-7 languages that can be traced back to the Hittite empire, only Nesi, Luwian and Palaic show Indo-Germanic characteristics, but even these cannot be called developed Indo-Germanic languages, but only early stages of development towards Indo-Germanization (1. Book I: p. 97. present book pp. 410-411. According to the general belief of modern research, only a negligible proportion of the population of the Hittite empire can be called Indo-Germanic — and even these only to a certain extent.

437
===== PAGE 432 =====

According to K. Bittel (Grundzüge der Vor- und Frühgeschichte Kleasiens), the Hittites cannot be considered a compact Indo-Germanic people. A. Moortgat (Geschichte Vorderasiens bis zum Hellenismus) states that not a single Indo-Germanic god name occurs among the Hittites. Their gods are of Hurrian and Hattian origin. In the famous Hittite rock sanctuary, Yazılıkaya, we also find all Hurrian deities and inscriptions in the Hurrian language. M. Semper (Rassen und Religionen im alten Vorderasien) states that the Hittite (Nesi) language is strongly mixed. Its roots are mostly of ancient Asia Minor (Hatti) origin. Only some words and grammatical features reveal an Indo-Germanic influence. The same is true of the Palaic and Luwian languages. But the names of gods and personal names of the Hittites are also all Asia Minor. Therefore, we can only count on a very small number of Indo-Germanic immigrants. W. v. Soden (Sumer, Babylon und Hittites until the middle of the 2nd millennium BC) also emphasizes that only a very thin layer of Indo-Germanic people could have existed among the Hittites. The names of their gods, kings, and even their chieftains are mostly ancient Asia Minor names. It should also be emphasized that Gyula Mészáros, in his treatise entitled "History of Eastern Europe II. — Chattiak és Scythák" (Chattiak és Scythák), stated the same thing as early as 1938 (Book 1, II., pp. 242-246).

We considered this brief digression necessary because in the relevant Hungarian literature we can only read about the Hittites as full-blooded Indo-Germans. For Gimbutas, however, the aforementioned written records and other clear data from the same period are far dwarfed by another, much more significant, decisive factor. significant "evidence". This is — most characteristically — A. Meillet's hypothesis from 1908 that "...the Indo-Europeans must have been ruled by a powerful aristocracy, capable of imposing the same language and the same social organization on wide areas of Europe and Asia." (see Scherer volume, p. 555). Gimbutas then continues: "Archaeological finds support this view. But written accounts from the 2nd millennium BC also prove it, insofar as they speak of the military courage and political importance of the Hittites, Kassites and Mitanni in Anatolia and Mesopotamia, and of the Hyksos in Palestine and Egypt." (1st ed. Scherer volume, pp. 555 and 560) This is all that Gimbutas offers as "proof" of the "Kurgānōs" Indo-Germans invading East Asia itself.

brings up.
Gimbutas is therefore obviously adding to a linguistic paleontological hypothesis that was published in 1908 the entire complex of archaeological finds in Eastern Europe and East Asia from the 3rd to the 2nd millennium BC.

===== PAGE 433 =====
mat, it is no wonder, since Meillet's "Indo-Germanic aristocracy" fits perfectly with his "Kurgan" Indo-Germanic "Herrenschicht" that dominated and cultivated all of Europe, East Asia, and even Egypt. And the Hesianism and the ability to organize politically are obviously such original Indo-Germanic characteristics, and such unmistakable evidence of the presence of Indo-Germanic ruling classes in the areas in question, that other supporting data are, needless to say, superfluous. As a result, Gimbutas does not waste his time on such things any more. However, the fact that among the peoples listed, not even the Hittites can be considered an Indo-Germanic people (1. above) — not to mention the others, whose languages were demonstrably of the agglutinative type and, according to the unanimous opinion of international linguistics, had nothing to do with the Indo-Germanic languages — does not bother him in the least.

We need to present these general Indo-Germanic questions in such detail because only after a multi-faceted presentation of the problems and trends of contemporary Indo-Germanic studies, the reasoning methods of individual schools, the real scientific weight, methodicality or non-methodicality of various arguments can we fully assess the one-sidedness of domestic Finno-Ugric linguistics, its ossified old-conservative approach, and its rigid isolation from all new insights. On the other hand, this background knowledge is also indispensable because our linguistic and pre-historical literature consistently ignores all the new methodological, language formation and language development insights and theses of international scholarship that contradict the Finno-Ugric language family theory based on the 19th century.

The current perspective of Hungarian linguistics
We have repeatedly pointed out that most of the authoritative representatives of Hungarian linguistics joined the so-called ahistorical school of Indo-Germanic linguistics, which absolutized the sound laws (e.g. Péter Hajdú, János Harmatta, etc.). Let us look at a couple of further examples of this. We have already mentioned Antal-Csongor-Fodor's book "The Languages of the World" in this yyyyyy collection (1. Book I. 79-80, pp.). In addition to what has been mentioned there, we will now also mention the peculiar method in which authors discuss the regularity of regular sound changes and, in close connection with this, the issue of separating loanwords. As an example, they chose the relationship between the Latin pellicium (animal skin, fur) and the German Pelz (fur, fur) - as we will show below, with a rather unfortunate hand. They state that according to the rules of sound change, , 4. 8 in German the Latin "p" at the beginning of a word should have an "f"

439
===== PAGE 434 =====
correspond, thus: Felz." (id. m. p. 12) Then follows the characteristic »phonetic« logic: "Since the German word has not 'f' but 'p', we can be sure that the historical relationship of pellicium and Pelz is not the same as the regular piscis — Fisch relationship. § indeed, Pelz is not the regular Germanic linguistic development of an ancient Indo-Germanic word, but a loanword borrowed from Latin." (id. m. p. 12)

habeo - German haben (to "prove", which they should first prove. The word Pelz was declared a Latin loanword in German at the time precisely because Azaz did not follow Grimm's rules of the game. Just like the Latin have, to have) or the Old English hrd - German Herz (heart) (Book II, pp. 113 and 116). So idem per idem"; a typical example of the circulus vitiosus with the law of sound.

But there are other problems with Pelz. First, according to the Kluge dictionary, its Middle High German form was bel(i)z, and the initial sound was only — consciously — restored by modern grammarians. , p", precisely by analogy with the Latin pellis, pellicium (and not pellicium, as our authors write). . On the other hand, if someone is going to make irrefutable statements about the etymology of a given German word — arbitrarily taken out of its semantic context — it would be appropriate to explain the origin and connection of the following German words with the word Pelz (common German belz, belliz) = "fur, fur":
Fleece = animal fur, wool;
Felt = fabric pressed from animal fur, felt;
Flaus = tuft of fur, sheepskin;
Plush (francia peluche) = gyapjűbőrsony;
Balg (plural balc) = a bag made of hairy animal skin, skinned animal skin, hide;
Bulge = bag, water-holding bladder; cf. lat. bulga, follis = etc.;
Polster = (original meaning) a leather cushion stuffed with horsehair.
Furthermore, the following words — without stem expansion — undoubtedly belong to the same root:
Fell = furry animal skin, fur;
Wolle = animal hair, wool;
Pelle z is thin, delicate leather.
The presented free sound changes — within the same language, and in words that are closely related in meaning; which cover the entire range of lip sounds (b, p, f, v, w), cannot be explained by any rigid, regular sound development. And if there is no "sound law" for such things; then from what kind of languages did German collect these words one by one, and why do their sound forms and meanings nevertheless stand in a wonderfully close parallel?
These are questions that must be answered with the laws of sound, regular sound-440

===== PAGE 435 =====
Linguistics working with changes is at a loss. Therefore, they simply single out the single word "Pelz" from the word family and completely unilaterally declare it a Latin loanword. However, if we do not allow ourselves to be blinded by the dogma of "regular" sound shifts viewed as a temporal process, then here too — as in the case of all word families — we can immediately recognize the real role and meaning of sound changes in languages; each one is an ancient, collective concept, not yet differentiated, with a general meaning.
From the primary root, with the help of variants of the consonants of the initial sounds with related articulation, new words and entire word clusters emerged that were semantically closely related and expressed nuanced differences in meaning. In this way, the former language developers also achieved that words that were semantically related remained in close parallelism phonetically, thus indicating their origin from a common ancient root (cf. Book I, Chapter 3, Chapter 4).
On page 75 we also come across a startling statement: they date from the . One of the Semitic languages is known from very ancient times. This language is Akkadian... The oldest texts of this language date back to the 1000s BC. beginning of the third millennium."
This is certainly not the case. The first Akkadian language records appear in the second half of the 3rd millennium, which is a huge time difference of half a millennium.
Closely related to this chronological error is the authors' remark that the period of the Old Babylonian (Akkadian) language lasted from about 3000 to 2000 BC, and that the Code of Hammurabi was written around 2000 BC (ibid., p. 84).
Well, even according to the so-called long Mesopotamian chronology, the Akkadian period only began around 2478 BC (around 2340 BC according to the middle chronology), and Hammurabi ascended the throne around 1930 / middle chronology: 1792/ and only conquered all of Mesopotamia towards the end of his reign of several decades. His code of laws also dates from these last years, in the introductory lines of which he refers to his rule over all of Sumer and Akkad (I. Müller-Karpe: Handbuch der Vorgeschichte III. B. Kupferzeit, p. 68).
Therefore, even according to the long chronology, the Law Column could not have been created before the 1880s.

It would be highly desirable if authors who consider themselves scientific experts, instead of irresponsibly tossing lottery numbers, would at least take the trouble to first look up the correct years in the appropriate handbooks.

András Róna-Tas' book "Language Affinity" does not differ much from the works based on phonetic law that we have seen so far. Apart from the fact that it provides a fairly detailed and easily understandable summary of the development of European linguistics in the last 150 years - of course, it only presents the views and theories of phonetic law linguistics exhaustively, and either remains silent about the research and findings of other trends,

441
===== PAGE 436 =====

or tactfully skims over them with a few lines of platitudes - in other respects his book can only be considered instructive in the sense that in some cases it unintentionally reveals the deep gap between the theoretical statements and methodological requirements of international linguistics and the practice of Hungarian Finno-Ugric studies.

For example, we can read with astonishment (pp. 19 and 139) that onomatopoeic words are only rare exceptions in the vocabularies of languages, constituting only a very small group: "However, among many tens of thousands of such words, there are only a few," he writes about onomatopoeic words.

After this, we can rightly ask: why is even the latest Benkő historical-etymological dictionary (TESZ) full of words explained as having onomatopoeic and onomatopoeic origins? According to the statistical compilation of Györgyné Hary, out of the 2516 words in volumes I and II of the dictionary that can be dated to before 1600, 206 words, i.e. slightly more than 8.1%, are onomatopoeic, children's language and emotional words (1. Györgyné Hary: Kiegészítések egy nyelvita történetéhez).

But the fact that the editor-in-chief of the dictionary, Loránd Benkő, referred to Haryné's original article (!) cannot strengthen our confidence in the methodological soundness of Hungarian language research. In his response (I. Magyar Nemzet, 1971. Nov. 14.), he primarily criticizes the fact that onomatopoeic words were omitted from Haryné's compilation, even though they play an important role in the vocabulary of every language. They play a greater role in the Hungarian vocabulary than onomatopoeic words."

Now, who should we believe? Benkő or Róna-Tas? - In our opinion, neither and both: in the assessment and separation of onomatopoeic and onomatopoeic words, we do not find any well-founded method in traditional linguistics based on the laws of sound. As a result, everyone declares individual words to be onomatopoeic and onomatopoeic based on their own subjective assessment, mainly on the basis of whether their related language phonemes show "regular" sound changes or not. We have already dealt with this question in more detail in Book I of our study series (pp. 116-121).

In connection with the quoted lines criticizing Benkő's article, it should also be mentioned that although Haryné only mentions onomatopoeic, childish and emotional words among the 206 words in question, in her reply to Benkő (Magyar Nemzet, November 21, 1971) she states that she naturally counted onomatopoeic words among onomatopoeic words.

We must absolutely agree with Haryne that he divided onomatopoeic and onomatopoeic words into one group and did not separate them from each other. The everyday reality is that there is practically no tangible difference between the words called onomatopoeic and onomatopoeic. That is why, for example, the Brockhaus Encyclopedia

===== PAGE 437 =====
The latest edition of pidge (1970) also discusses onomatopoeic and onomatopoeic words under a common heading, without any distinction (1, under the heading "Lautmalerei, Lautnachahmung").

Benkő's slanderous objection therefore merely reflects the efforts of onomatopoeic linguistics, with which - if they are completely in the dark on the main question, namely, what objective criteria can be used to separate onomatopoeic and onomatopoeic words from other elements of the vocabulary - they at least try to give their etymological ramblings a somewhat more scientific tone by making a sharp distinction between onomatopoeic and onomatopoeic words. However, we can see step by step in the TESZ, which was prepared under the editorship of Benkő, where this apparent meticulousness leads in reality: the dictionary is full of words of "onomatopoeic origin" about which we firmly believe that someone other than the dictionary's article writers could tell us what animal or other natural sounds they "imitate" (e.g. kap, kapaszkodik, kapatos, szápályk, etc.). The same is true of onomatopoeic words: the TESZ consistently declares words of "onomatopoeic origin" that cannot be connected in any way with any sounds (e.g. gób, gömb, gömbölyg, gom, gomboyag, gubó, etc.). What sound does, say, gubó or a göbó "depict"?

Perhaps the noises that arise from the one-sided cocooning of phonetic linguistics? We cannot know. However, we learn from Róna-Tas that grammar and language structure cannot determine genetic language affinity, since, for example, English has no grammatical similarity to Proto-Indo-Germanic (ibid., p. 160). On the other hand, if the continuity of the majority of the vocabulary can be demonstrated, then the continuity of the grammatical system is also very likely (ibid., p. 374),

Again, you may ask: then why does Finno-Ugric studies claim that the grammatical similarities and parallels are of paramount importance in establishing genetic language affinity, and why do you categorically reject, for example, the approximately 1000 clear Sumerian-Hungarian word parallels as unimportant? Incidentally, we also have plenty of grammatical parallels with Sumerian, but we do not want to involve them in our current special question. Speaking about the thesis of ancient root languages, Róna-Tas states that there has never been a homogeneous language.

The lower the level of development of a society, the less homogeneous the language (ibid., p. 367). But on the very next page he refutes this statement, because there he talks about the fact that in the Neolithic Age, the age of the basic languages of the large language families, although there was presumably a great deal of diversity, this was only dialectal diversity, and the differences played only a secondary role, subordinated to the uniform elements of the language family.

443
===== PAGE 438 =====

We must ask again: if the linguistic sequences of the Neolithic were already subject to "single language family elements", then how did these roughly uniform language family basic languages develop in the Paleolithic or Mesolithic? After all, Róna-Tas himself states: the lower the level of development of society, the less homogeneous the language. However, in the Paleolithic and Mesolithic, the level of development of society was incomparably lower than in the Neolithic. At that time, therefore, according to Róna-Tas' own statement, the linguistic differences must have been much greater, even within a relatively small group of people, let alone among the sparse Paleolithic-Mesolithic hordes scattered over half a continent speaking a presumed common Indo-Germanic or Uralic basic language, who had little contact with each other, and what contact there was between them mostly manifested itself only in killing each other. In this connection, we strongly remind you of Altheim's statement quoted above, according to which "... Only during certain historical events did certain linguistic or dialect groups become dominant..." (Volume 1, pp. 408-409).

"Two or more languages are related if a continuous movement of the most common subsystems can be detected between them and the common base language" — read Róna-Tas's book
On page 374.

We readily accept this definition. The only problem is where, when and who demonstrated — with objective scientific methodology! — the continuous movement of the most common subsystems between, for example, Hungarian and the "Uralic", "Finno-Ugric" or even just the "Ugric" basic language? To our knowledge, the Vogul-Ostiak linguistic monuments appear in the 18th century AD, the monuments of the Hungarian language date back about a thousand years, and the "Ugric" basic language is placed by Finno-Ugric studies between 2000 and 1000 BC. Where and how can "continuous movement" be demonstrated when there is a gap of almost 3000 years between the Ob-Ugric linguistic monuments and the age of the supposed "basic language", and moreover, this alleged basic language is completely unknown here too?

Let us be careful with the concepts, and let us also pay attention to the most elementary laws of rational thinking: what kind of method is it if we want to demonstrate this "continuous movement" by first deducing a hypothetical "base language" from today's Ob-Ugric languages and Hungarian according to a pre-formulated theorem — for example, with the help of "regular sound changes" — and then, using the same method, i.e., by stepping on the same ladder, only this time backwards, we trace the "continuous movement of the most frequent subsystems" of this artificial base language back to today's Vogul and Hungarian? In this case, we have done nothing but deceive ourselves. Such a method is merely a deception, a vicious circle, i.e., walking in the same place, but in no way a scientific proof,

444
===== PAGE 439 =====

This is exactly the same procedure that archaeologists used to build the Aswan Dam on the ancient Egyptian monuments there; they took them apart block by block, carefully numbered the individual pieces, and later reassembled the buildings based on the notes. That is, their "most common subsystems" were taken apart according to specific rules, and then reassembled according to the same rules, "in continuous motion." And wonder of wonders: the temples turned out to be exactly the same as they were before.

Well, Finno-Ugric linguistics does the same thing, when it deduces from the elements of the Ob-Ugric languages and Hungarian, which can be traced back to a Ugric base language believed to be 3-4,000 years old, according to predetermined criteria, and then, based on the same criteria, gradually refers the same elements back to modern Vogul and Hungarian in a "continuous movement". To put it even more simply: this process is no different from the "method" by which Baron Münchhausen pulled himself and his horse out of the swamp by his own ponytail - where the horse exemplifies the Finno-Ugric languages and the baron exemplifies Hungarian.

However, the most depressing thing about this issue is that we have to explain these self-evident things, the simplest elements of logical thinking, in such detail, because for more than a hundred years, it has apparently not occurred to any professional linguist to take these fundamental principles of rational thinking into account. If we are talking about "continuous movement", then the most elementary methodological requirement is that the starting point of this "continuous movement", i.e. the phonemes of the linguistic state 3-4000 years ago, is known from concrete, contemporary linguistic monuments. However, there are no such ones, and therefore the linguistic-historical conclusions and attempts at basic language reconstruction of traditional Finno-Ugric studies cannot be considered anything other than - to use Kronasser's words - merely very attractive soap bubbles, between which everyone can choose according to their own taste.

We have therefore chosen a different path for the investigation of these paleolinguistic questions, which, although still unexplored and laborious today, already promises much more concrete results than linguistic research has achieved in 150 years using the methods used so far. We are convinced that the enormous Pre-Asian, and in the period of interest to us (3rd-2nd millennium BC) Mesopotamian-Sumerian cultural and ethnic influences in the western half of Eurasia, which have emerged from archaeological discoveries, directly require the inclusion of Sumerian linguistic monuments in our research on language development and prehistory. In the case of the Sumerian language, which has ancient written monuments, we are not forced to invent sterile, unprovable theories, but we can compare our language, other related Finno-Ugric languages, and even the

445

===== PAGE 440 =====

other West Eurasian language groups, all of which are inextricably linked to Sumerian (Book II, Chapter 5).

Although we have already taken a step forward, we are still far from reaching the threshold of the so-called base language of the West Eurasian language families, which, in our opinion, with the knowledge we have today, we cannot even approach, at most only guess at. This oldest "basic language" is lost in the complex interactions, internal and external migrations, and mixing of the Neolithic and Chalcolithic cultures and ethnicities of Pre-Sumerian East Asia. The earliest tangible linguistic state is the language of the Sumerian clay tablets, which began in the second half of the 4th millennium BC, but which was preceded by several millennia of convergent linguistic development in East Asia. As a result, at the present stage of our knowledge, we must for the time being consider Sumerian as the "primitive language" (but of course only relatively!), we must start from the Sumerian linguistic monuments, and we must first research what kind of "subsystems" can be detected from this primitive language stage in the linguistic monuments of other West Eurasian language groups, and in today's languages. Unfortunately, we cannot speak of "continuous movement", because this is not possible in the context of specific written memories do not allow it. Thus, we can only record snapshot-like, "ad hoc" language states, mostly with large temporal interruptions.

However, in these investigations, it is invaluable to recognize that the primary and most important role of sound changes in the development of languages is semantic, and that they predominantly express not "sound development" that occurs regularly over time, but changes in meaning, indicating the semantic development of languages and the enrichment of their vocabulary. This is the only method that can be used in broader paleolinguistic research, because we saw in Chapter 5 of our Book I that languages are - fortunately - much more conservative, and the sound forms of their words - within certain limits - are much more durable than linguistics, which believes in regular sound changes viewed exclusively over time.

The so-called "accidental" coincidences found in large numbers in unrelated languages, which Róna-Tas discusses in the usual way, are also related to these questions.

For example, he mentions the phonetic and semantic similarity of the English cut (to cut; pronounced kat) and the Arabic kat (to cut) and states that "...the two words have nothing in the world to do with each other." (id. m. p. 393)

We would be more careful: cf. Sumerian "kud" — "schneiden, abschneiden, durchschneiden, scheiden, spalten, entzweien" (cut, cut off, cut through, separate, split, cut in two); Hungarian "kötli" (cut off feed stump), "kutyol" (prune grapes).

It would be good, therefore, if our linguists looked around a little more thoroughly at other languages before speaking so confidently about accidental combinations between two languages,

446

===== PAGE 441 =====

These "accidental" coincidences occupy a very prominent place in the field of phonological linguistics. They usually refer to chance when they try to explain away, or rather to exclude from the comparison of languages, the phonetic and semantic similarities or close parallels of words from different, unrelated language families - these frequently occurring facts, incomprehensible to them and quite disturbing to their circles.

It is a common belief in international linguistics that due to the limitations of human sound production, the number of possible word forms is quite low, and therefore: , If we take out the dictionary of any two languages, we will find many words that are similar to each other or even appear to be identical... Such coincidences

"We could list hundreds of coincidences." (Róna-Tas: id. m. pp. 392-393.) ' However, according to the mathematical laws of probability, random coincidences are quite rare even between words in two languages, and practically impossible between words in three or more languages. Statistics, which works with large numbers, does not recognize coincidence. Insurance is also based on this: the insurance company can calculate exactly how many houses out of a thousand burn down each year, how many people out of a thousand have accidents, or how many out of a thousand hectares are damaged by ice.

This law, applied to the words of languages - simplified, in broad terms - looks like this: if we count only 30 speech sounds - and there are many more -, then the variation formula of a root word consisting of three sounds is 30³ = 27,000. That is, there are so many different words consisting of three sounds possible. But we must also take into account the criterion of pronounceability, i.e. that consonants and vowels alternate in words, and words consisting solely of consonants or vowels are generally very rare in languages - although they sometimes occur (cf. e.g. South Slavic "smrt" = , death"; French "eau" = , water").

Thus, if we count one or two vowels in each word, the number of possible words is reduced to 18,000. However, among these there are also words like "abb" or "bba", which are also not common - at least not in Hungarian. The number of words of these language, which avoids four types is 12,000. This leaves 6,000 completely e.g. "aab", "baa", , are words that can be articulated clearly, easily and clearly (the "i", "aba" and the "a" are also - even according to the evidence of the Hungarian consonant clusters as much as possible: see, e.g. ért, int, lit, önt, ángy, ing, ölyv, örv, etc. "bab" series), to which we must, however, add a good few words from the four types above.

Ultimately, we have to count on at least 9000 normally pronounceable possibilities for words consisting of three sounds. This means that the probability of a root word consisting of three sounds being identical in phonetic and meaning in two languages is 1/9000.

447

===== PAGE 442 =====

In the case of words with four or more sounds, the probability of coincidence decreases in proportion to the power of the number of sounds. In the case of four sounds, for example, the number of purely articulable words -- again taking Hungarian as a basis -- is 120,000 (the "abab", "baba" and "abba" types), but including the "babb" series (cf. kert, most, part, hajt, sujt, etc.) we get 200,000 possibilities. Yet they encompass 80,000-80,000 possibilities. That is, in the case of words with four sounds, the probability of coincidence is only between 1/200,000 and 1/360,000. "baba" and the "babb" types are also common in most languages (e.g. German Haus, Baum; Stock, Flug, etc.), and these again

So, to stick with Róna-Tas' example, in order for coincidences similar to the coincidence of "cut" to occur in hundreds of cases in Arabic and English — but only in 6 languages — both languages would have to have a vocabulary of many millions. However, taking into account that the word pair in question is not only the same in Arabic and English, but also has a very close phonetic parallel with the words of the corresponding meaning in Sumerian and Hungarian, the possibility of coincidence is practically zero.

From all this it follows that referring to chance, especially when speaking of "hundreds of" coincidences between two unrelated languages, cannot be called scientific reasoning, but only a misconception based on the prejudice of "sound laws".

In the preface, we note that in the field of Sumerian-Hungarian word similarities and parallels, this reference to chance becomes even more absurd. We know of many polysemantic Sumerian roots, which have 5-10, and sometimes even more, meanings that either exactly match the phonetic forms of the corresponding Hungarian roots, or only the vowel varia] in Hungarian. For example (German meanings of Sumerian roots according to Deimel):

star" = "destroyed, devastated" "cut off, devastated"	tar = "to devastate, to pluck" tar-16 = "to preserve" tar-gonca = "to restrain, to tame" tyr = "to bend, to bow"	tar-ol = "thorn" tar-t 4 = "guard" tar-töztat = "split, cut, open" door	tar-aj = star-ol = tár = „giant, graben"
===== PAGE 443 =====			
= "break"	daggar	daggar	
= "tip" = "lift" = "interrogate, decide" = "black"	tor-vény = "to determine, fate"	tör-öl	tör-tenik = "to bind, to banish, to hold accountable tor-zsa tor-ol
	tor-laszt = "cutting of herbs"		

Here, nine meanings are identical in all three sounds to the roots of the corresponding Hungarian words, and in eleven only the root vowel changes in Hungarian. In the case of such massive coincidences and parallels, according to the mathematical laws of probability, the possibility of a coincidence by chance is practically zero, just as in the case of the coincidence of individual words from three or more languages.

A textbook example of phonetic reconstructions of ancient languages is the alleged regular development of the Finno-Ugric "K" sound into "tr" in Hungarian. Since the Hungarian "tr" corresponds to "k" in a certain phonological position in the vast majority of Finno-Ugric languages, it is very likely that the majority (emphasis added) here preserves the ancient state" — writes Róna-Tas (id. m. p. 395).

What does Finno-Ugricism mean here by "the overwhelming majority", which supposedly preserves the "primitive state"? The largely primitive Finno-Ugric fragmentary peoples, numbering 8 million in total but fragmented into 50 — mostly mutually incomprehensible — dialects, compared to the monolithic Hungarian linguistic bloc of 15 million, speaking a unified, almost homogeneous language, and having been at an incomparably higher level of culture for more than a thousand years. On the other hand: according to them, there would have been "sound development" only in Hungarian, and not in the other Finno-Ugric languages? There for thousands of years. But how do we deal with the much more likely possibility that the sound "tr" "k" left as "k"? of the real "overwhelming majority" and culturally highly dominant Hungarian language was once adopted by the small, backward Finno-Ugric groups, and in certain cases changed it to "k" under the influence of the phonology of their own ancient linguistic base layer? In other words, they used the well-known phenomenon of sound substitution, and that is why they still pronounce "K" today, as they did thousands of years ago. Can all these circumstances be neglected in an attempt to reconstruct a hypothetical ancestral language? We would not believe it. Finno-Ugric linguistics, however, does not raise such thoughts even as a faint possibility.

Let us now see how Róna-Tas views the Sumerian-Hungarian language analogy. Behind the question of Sumerian-Hungarian language affinity lie non-scientific considerations: a question of prestige — he writes (id. m. p. 437). He then says that in the 19th century, after the discovery of the Sumerian language, among the first attempts at language comparison "...there were such-449

===== PAGE 444 =====

"also those who tried to relate Sumerian to the Uralic and Altaic (then still thought to be one) language family." (id. m. p. 439).

Here, at the beginning of the discussion of the Sumerian question, Róna-Tas does not fight with an open helmet, because he declares from the outset that the whole thing is just a question of prestige; although it may be his own conviction, it is in any case not a scientific argument, but merely an a priori sensationalism based on his professional authority. In the second quote, he goes even further in this misleading direction: his statement that Sumerian was related to the first attempts at language comparison in the 19th century is a deliberate misrepresentation. These first attempts at language comparison almost without exception related Sumerian to the languages there were also those "that connected Sumer with the Urals" of the Urals (1. Zs. Varga: Ötözer év titóllábó). This, however, is undoubtedly mystified by Róna-Tas even where the question of their kinship is treated as if it were just an outdated, 19th-century assumption that has long been refuted by more recent research. This question is also closely related to the one - There was a considerable difference compared to the statement "there were also some". sidedness of Finno-Ugric studies. Despite the series of lexical and grammatical "then he still thought of it as one" speaks of the Uralic-Altai language family, so he presents the Uralic and Altaic languages as similarities, they flatly deny the kinship of the Finno-Ugric and Altaic languages, because in this case, a considerable proportion of their basic words, which are claimed to be of "Uralic" origin, are also found in the Altaic languages, and the thesis of an independent Finno-Ugric (Uralic) language family would turn out to be merely a linguistic fiction. As a result, there is a whispering propaganda in the Finno-Ugric ranks that international linguistics has long since rejected the possibility of the kinship of the Uralic and Altaic languages. We cannot delve into the discussion of this issue now, so we will only quote the relevant opinions of two Hungarian researchers and one international authority.

According to János Pusztay (After the "Ugric-Turkic War", pp. 140-141), the theory of Uralic-Turkic kinship "...is now more than a simple hypothesis. Ramstedt, the founder of Altaic comparative linguistics and... Collinder identified 69 Uralic-Altai common etymologies, and these belong to the so-called basic vocabulary... Of the 69 etymologies, 8 are found in all branches of the Altaic language family, 38 only in Turkic, 38 in Mongolian, 37 in Tungusic. One third of the words are found in both Samoyedic and Finno-Ugric."

"The theory of Ural-Altai kinship...has not been conclusively proven, nor refuted, in the century and a half that have passed since its emergence." (Péter Juhász: Introduction to the volume "Studies in the Sphere of Bulgarian-Hungarian Relations", p. 15).
"Recently, an opinion has gained ground according to which experts are now beginning to doubt the existence of a Uralic-Altai language family-450

==== PAGE 445 =====
"I don't know who the experts are. Those scientists who have written about the Uralic and Altaic languages in their publications over the last 50 years have all supported the idea of language kinship." (B. Collinder: Are there any relatives of the Uralic language family? Gulya J. ed.: The people of waterfowl, p. 80).
In relation to these clear lines, we only note that if we want to search for non-scientific aspects at all costs, we are much more likely to find them in the aforementioned whispering propaganda, the obvious aim of which is to maintain the prestige of the exclusive Finno-Ugric theory by non-scientific means, and even by stating outright untruths. And lo and behold, Róna-Tas himself actively participates in the dissemination of this untruth: he talks about the language family. Under such circumstances, we do not consider it the most appropriate thing for Róna-Tas to mention non-scientific aspects in relation to others. then he still thought of "Ural and Altai as one"

Our author then states that the Sumerian language was already revealed at the end of the 19th century to be an unrelated island language, and "none of the languages currently living in the world can be related to Sumerian." (id. m. p. 439)

Strangely enough, he does not suspect any "extra-scientific" aspect or "prestige issue" here, although there is enough evidence for such a thing. Let us recall, for example, Sayce's statement on this subject (1. Book II. pp. 378-379). The provocative question in Halévy's opening article ("Is the presence of a Turanian people on the soil of Babylon permissible?"; 1. Book II. p. 379) or Babelon's demagogic statement in But it would not be uninteresting if we also shed a little more light on Róna-Tas's specific argument, when he tries to support the assumption that the Sumerian "Turanian" peoples (Book 1, p. 379). language was an unrelated island language with examples:

"There is nothing surprising in this. We know many languages whose relatives are all extinct. There are such languages in Siberia, for example, the two languages also called Yenisei Ossetian... These are called Paleosiberian languages, a collective name for small, unrelated language groups living in North Asia. There are such unrelated languages in the Caucasus and the Pamirs, and in all probability Basque is such a language." (id. m. p. 439).

Well, we find a lot of "surprises" in this comparison. Namely, if there are non-scientific aspects behind these lines, then how can we explain the shocking ignorance of cultural history that is expressed in the quote - which is otherwise completely incomprehensible in a university professor with Róna-Tas's qualifications? The world's very first cultural language, which was generally used by the entire pre-Asian culture for a good one and a half millennia, is not a language that can be understood in a language that is not

==== PAGE 446 =====
It was the language of the Lags, and then — as Róna-Tas himself writes! — the Akkadians "used it for a long time as a scientific and religious language — just like we use Latin" (1. id. m. 437-438. p.), comparing it with the languages of some Paleo-Siberian fragmentary peoples, their isolation, with the languages of such people groups that were far from the development process of humanity until the 19th century, and even in the recent past lived at a Stone Age level.

Here, in fact, the roots of "extra-scientific considerations" can be felt, not among those who recognized the eminent significance of the Sumerian cultural language 651 and examined its effects — which can be expected with almost infallible certainty — in the later developed and consolidated West Eurasian languages, including Hungarian.
We do not dispute that a considerable part of these attempts attempt to approach the question with insufficiently well-founded methods, or sometimes with downright unscientific means. However, let us consider that we cannot expect anything else if those who, based on their linguistic and prehistorical training, are called upon to deal with the problem professionally research, they rigidly reject any kind of investigation in this direction — and they are not even willing to learn at least in broad outlines, at the level of general cultural-historical connections, about the complex of issues of the influences of ancient Middle Eastern civilizations on Western Eurasia.
One of the most shocking manifestations of this ignorance is the claim that the Sumerian language was an island language. Embedding this question in the cultural-historical and paleoethnological picture we have known so far, there can be no doubt for a moment that, given the archaeological data available to us today, Sumerian cannot be considered an island language.
Or would anyone consider it scientific to assume that Latin, for example, was an island language at that time? Certainly not. Yet the history of the Latin language would much rather allow this hypothesis. Originally, it was indeed an insignificant island language, not even spoken in all of Latium, but only in two villages, ancient Rome and Alba Longa, and their immediate surroundings (I. Kronasser: id. m. Scherer volume, p. 484). Only step by step, parallel to the Roman conquests, did it become the dominant language first of Latium, then of Central Italy, and later of the entire peninsula, and finally it spread throughout the Mediterranean region as the administrative and cultural language of the Roman Empire. And what is more, it was not a dominant cultural language for 1600 years — like Sumerian Summary: —, but for a maximum of 4-500 years.

Briefly summarizing the data we have learned so far, it can be clearly stated that Hungarian Finno-Ugric studies has methodologically lagged far behind the development of international linguistics,

452
==== PAGE 447 =====
The schematic language family tree theory, which has been almost universally rejected by international scholarship for decades, is still treated as an axiom. The possibility of deducing hypothetical Co-languages with the help of linguistic tools, "sound laws", which even all Indo-Germanic studies, which have incomparably more and older linguistic monuments, have long clearly shown to be a futile effort for theoretical and methodological reasons, is still held by him and even considered as the cornerstone of his linguistic and pre-historical studies. The method of "linguistic paleontology", which has also long since lost all credibility in international linguistic and pre-historical research, continues to be considered an infallible tool for revealing the cultural state and social relations of the supposed ancient peoples and for determining the geographical location of hypothetical ancestral homelands. He does not even pay attention to the discoveries and results of the archaeological research in the Middle East in the past 60-70 years in terms of pre-history, cultural history and linguistic monuments, and he rigidly refuses to draw any conclusions from them. He is also unwilling to draw any lessons from the newer results of archaeology during the Migration Period, which clearly proved that the conquering Hungarian people were in no way less educated or less developed than the other peoples of Central and Eastern Europe at that time. Among other things, despite everything, he consistently asserts the foreign origin of several thousand of our words. Although a considerable part of these were declared loanwords at the time, in the 19th century, primarily and most importantly with reference to the low level of culture of the "barbarian" Hungarians and with emphasis on the alleged high level of culture of the neighboring peoples of the same time. Indo-Germanic linguistics and prehistory research have produced numerous authoritative, internationally respected

representative, that the Indo-Germanic languages were in all probability only formed at the turn of the 3rd/2nd millennium BC, he does not pay any attention to it, and continues to reckon with Indo-Germanic, Indo-Iranian and ancient Iranian basic language influences and loanwords in his "Finno-Ugric" basic language, in the 4th-3rd millennium BC. In international linguistics, he considers the only orthodox trend that renews the basic principles of the former neo-grammatical school as authoritative, which has already taken the absolutization of sound laws to the point where even the Japanese Ainu language can be described with their help. 453

==== PAGE 448 =====
"Indo-Germanized"; and which is described as a completely unhistorical, "ahistorical" direction by one of the most representative handbooks of modern historical research, the "Reallexikon der Assyriologie".

8. The unspoken basis of all these absurdities is the fatal realization that the moment the two fundamental tenets of modern international linguistics are accepted, according to which the reconstruction of hypothetical proto-languages is a theoretical and methodological impossibility, and that Indo-Germanic languages only existed in the 1st millennium BC, began to emerge during the 2nd millennium BC, the assumed chronology of the branching of the Uralic and Finno-Ugric language families would inevitably collapse, which at the time was developed solely on the basis of the 19th century classical Indo-Germanic language family tree and in close accordance with its assumed language family chronology, in such a way that the word parallels found in the Finno-Ugric and Indo-Germanic languages were considered to be ancient borrowings taken from either the still undivided Indo-Germanic base language, or from Indo-Iranian or Old Iranian. However, since recent Indo-Germanic linguistic history studies have also shown that the formation of the Indo-Germanic languages was influenced by the so-called Uralic or Finno-Ugric roots played a very significant role, and in the case of these ancient similarities and parallels, it has become quite doubtful who borrowed from whom. We emphasize: even if we accept the common proto-linguistic ideas of orthodox Finno-Ugric studies.

However, we do not want to ignore the fact that recently, more and more people in our country have begun to challenge the rigid linguistic and prehistoric dogmas of the theory of our exclusive Finno-Ugric origin. The merit of spreading the new, multifaceted approach is primarily attributed to our outstanding, internationally renowned scientist, Gyula László. His research method, his musings on what he called "fertile uncertainty", began to shake up our linguistic and (especially) prehistory research from its previous calm complacency. /

The first "astonishment" was caused by Professor László's fundamental book entitled "The Earliest Sections of Our History", in which he pointed out emphatically in 1961 that the former ethnic relations that can be deduced from archaeological finds largely indicate that the so-called Ural-Finno-Ugric language family cannot be considered more than a linguistic chain, and that we can hardly speak of common Ural-Finno-Ugric ethnic and linguistic foundations. At the same time, he also demonstrated with convincing argument that the concepts of the ancestral homeland of Finno-Ugric linguistics are incompatible neither with the archaeological finds nor with the newer results of pollen studies. 454

===== PAGE 449 =====

with the oldest dwellings of the ancestors of today's Ural-Finno-Ugric peoples must be sought much further south than the Oka-Káma-Bjelaja region (for more information, see 1, our book II, pp. 310-318). The first echo of this book was that a year later, Peter Hajdú (Finno-Ugric peoples and languages, 1962), as if "for the first time", placed the Urals on the ice of the West Siberian tundra between the northern part of the Ural Mountains and the lower reaches of the Ob - we have repeatedly pointed out the methods used (Book 1, II, pp. 385-387) - but much more important than this absurdity is that the questions of the development of the Ural-Finno-Ugric languages and the "ancestral homeland" of the Urals have not been removed from the agenda since then, and even today there are heated debates going on around Gyula László's southern theory. In other words, the question of the ancestral homeland, which was considered to be settled a hundred years ago, has become problematic again, and thus we can hope that slowly, perhaps, Hungarian prehistoric research will also He notes the increasingly evident Pre-Asian cultural and ethnic influences in the Eastern European steppe and forest-steppe areas from archaeological finds.

It is quite typical, for example, that in his work "Introduction to Ural Linguistics" published in 1966, Péter Hajdú himself strikes a completely different chord (although in 1962 he still called all language comparisons not exclusively Finno-Ugric unscientific): "...The area designated as the Uralic homeland was the place of residence of our ancestors immediately before the separation... the language of our ancestors developed near the centers of development of several old Eurasian languages and language families... however, it is probable that, in addition to the archaeological ones, linguistic traces also point to the southern connections of the Pre-Urals." (id. m. 1966, pp. 9-10) From these lines it is obvious that Hajdú — under the compelling weight of archaeological and other evidence — was forced to give up his rigidly negative position of 1962. Another question — and also very typical of the exclusively Finno-Ugric school's lack of method — is that in Hajdú's later statements in this regard, he does not even pay attention to his own above-mentioned, fundamentally important, statements, but continues to defend to the hilt that old, outdated, only north-facing research trend, which, according to the clear evidence of the above quote — but, as we can see, only in principle — he himself does not consider satisfactory, and in practice he nevertheless avoids any specific, south-facing language comparison study (cf. e.g. Hajdú: The Sumerian Myth and Reality; and: About Our Sumerian Relations). Despite the fact that in his aforementioned work (1966) in addition to the lines already quoted, he also states the following: , The methods of comparative linguistics are insufficient for solving problems. In the future, paleolinguistics will have to approach the linguistic events of this very distant era with new procedures. Worthy of mention and attention in this regard is the application of probability theory in determining the ancient kinship relationships of different language families... its starting point

455

===== PAGE 450 =====

(of this new method) is the well-known theorem that the possibility of a coincidence of two morphemes when comparing two languages together is greater than when comparing many languages. Researchers have so far discussed the coincidences of the Uralic-Altaic, Indo-Germanic-Semitic, Indo-Germanic-Sumerian, etc. language families separately. A more reliable result, however, can be expected if we compare the languages of all Eurasian language families together." (Hajdú: id. m. 1966, p. 91) And: , It is highly probable that the Indo-Germanic, Semitic, Hamitic, Ural-Altaic, Chukchi and Kartvelian language families have a very old genetic relationship." (Hajdú: id. m. 1966, p. 92) In Hajdú, therefore, the unbridgeable gap between theoretical statements and everyday practice, which has always and everywhere been characteristic of our exclusively Finno-Ugric linguistic and prehistory research, and which we have already demonstrated countless times, appears again in pure culture. At the same time, Gyula László's concept of a language chain has not yet found followers, but there are already encouraging signs in the field of linguistics. We can consider such previously almost unimaginable "heresies" as such, for example, in the collected volume "Vendégségben yseinknél" the editors (János Boros and László Rapcsányi) cite the directly differing opinions of one or two internationally recognized foreign Finno-Ugric researchers about the Finno-Ugric language family tree, which is sacredly believed in and set as fact in our country. Vikuna Kustaa's statement: ",.... 58. page.). It is not possible to simply say about the relationship and path of peoples that they spoke a common language, then one went this way, the other that way (id. m.

KFW Meinander's statement: "... I think we cannot speak of any common ancestral people, any family tree of Finno-Ugric peoples." (id. m.123. p.) Certainly, these are completely different voices from those that the Hungarian reading public has heard from the domestic linguistic stronghold of our Finno-Ugric origin as divine revelations. And these voices - in a most wonderful way - agree point by point with the new results of international linguistic and pre-historical research that we have described in detail in our present volume (I. pp. 407-408), which Hungarian Finno-Ugric studies, however, still refuses to acknowledge to this day. It was also Gyula László who initiated the second renewal, with the thesis of the double conquest. This theory, supported by a probability that was on the verge of certainty, archaeologically and anthropologically, brought about a real revolution. It overturned almost all the outdated ideas of the Hunfalvy school — which had been treated as tacitly proven facts in our prehistory until today. As a result, the earth moved, so to speak, since the proposal of the thesis of the double conquest. No one could avoid taking a stand: either for or against it, but in some way everyone had to take a stand on this issue, 456

===== PAGE 451 =====

Sami The most important = he had to justify point 41 with scientific arguments. Under the overwhelming weight of archaeological and anthropological data, it was not possible to address the matter in the usual way - by waving a hand or simply by citing literature - although several people have tried to do so. Let us recall, for example, Fodor's "main difficulties", which, however, we could establish at first glance that they are very far from the basic theoretical requirements of systematic scientific reasoning (Book 1, Book II, pp. 362-367). During the discussions, numerous data and historical connections emerged from the sink of scientific history, which the Hunfalvy school at the time mostly threw into the trash, completely one-sidedly, without any substantive refutation, usually with the excuse that they were... For example, nowadays, no one can claim with such self-evident naturalness as was usual before, that the ancient "they contradict our Finno-Ugric origins". Hungarians had nothing to do with the Avars, Huns, or perhaps the Scythians or Sarmatians. As a result, conscientious researchers have recently been very cautious in expressing their opinions on these issues,

Kornél Bakay, for example, writes: "Our chronicles unequivocally call us descendants of the Scythians, and they give such extensive descriptions of the ancient Scythians that the kinship relationship does not seem unfounded." (Bakay: Are the Scythians our relatives? 1. Viktor Szombathy: Millennia were stated based solely on the consonance of a few personal names, and then continues: , Can a people be judged based on the names of some of its leaders? After all, on such a basis Attila would not be a Hun, or King Ladislaus a Hungarian... our current knowledge forces us to revise previous positions." (id. m. p. 70). In his conclusion, however, he cannot free himself from the 19th-century doctrine of the "Finno-Ugric origin" of the Hungarians — one of the strongest supports of which was the shaky assumption of the Iranianess of the Scythians at the time: Were the Scythian people, called Scythians, related to the conquering Hungarians who followed the Avars? In a genetic sense, we could hardly say yes, despite the fact that based on recent research we dare to increasingly doubt the Iranian nature of the Scythian peoples, but their cultural and artistic heritage undoubtedly continued and became part of the culture of the Hungarians." (id. m. p. 93).

According to Bakay, therefore, in a genetic sense, our relatives are hardly the Scythians or Sarmatians, among whose bone finds the anthropologist Tibor Tóth — in agreement with several renowned Soviet researchers — found the closest — almost identical — parallels to the anthropological makeup of the conquering Hungarians: "The bone legacy of the conquering Hungarians

3

===== PAGE 452 =====

It is among the Sarmatian finds, far from the groups of the Káma Basin. From the most recent, even wider comparisons, it can be concluded that the finds of the conquering Hungarians are very similar to the skeletons excavated from the lower Volga region, buried one and a half thousand years before the conquest. (Statement by Tibor Tóth, Boros-Rapcsányi: Vendégségben yseinknél, p. 152) At the same time, however, Bakay obviously sees no "difficulty" in the fact that has been known for 80-90 years - and has also been demonstrated anthropologically - that the Ob-Ugric peoples, from whom the Finno-Ugric school derives our language and ethnicity, are anthropologically completely different - not only from the Hungarians, but also from the other peoples called "Finno-Ugric". Géza Nagy drew attention to this as early as 1890 and repeatedly later (I. e.g. Ethnologia és nyelvészet 1890; Honfoglaló yseink, 1894).

In connection with these questions, we recall the relevant pages of Book II of our study series, in which we cast doubt on the Scythians and other ancient peoples, and at the same time we pointed out a number of data that very likely testify to the "Altaic character" of these peoples (Book I. y. pp. 224-236). In the same place, we also dealt with our Scythian relations (Book 1. II. pp. 334-336), and we showed that even starting from the traditional ideas of the exclusively Finno-Ugric school of thought, we must reckon with a very significant degree of ethnic mixing between the Scythians, Sarmatians and the Scythian-era ancient Hungarians, since researchers with a Finno-Ugric perspective themselves — for example István Fodor — place the settlement areas of the ancient Hungarians in the 1st millennium BC in the Scythian-Sarmatian region or in its immediate vicinity.

Finally - but not least - we would like to point out again the surprising Hungarian linguistic The appearance of the folk name "sabarol" (saber) in the Aral Sea area in the 2nd century BC, as well as the parallels of the name of the "Saka-Uraka" people mentioned in this region (Book II, pp. 262-264 and 270): 1. "saka-uraka" = "Könige der Saken, i.e. Lord of the Sakan" "lords of the sakas" that is, lords of the sakas; 2. The word "saka-ur" = "lord of the szakák" or (title of a certain Saka king). The "lord" was specifically a title of the king even in the time of the Árpáds.

Well, in light of Tibor Tóth's anthropological studies, these data are not so surprising. If the ancestors of the Hungarians lived in the Káspi-Aral region during the Scythian-Sarmatian period, then we can by no means consider these Hungarian linguistic traces as a coincidence, nor the fact that the (Szabir) ethnonym is (Szabir) appears precisely in this period and in this area, which was, as we know, the old "sabaro!" name of the Hungarians.

458
===== PAGE 453 =====

Chapter 2. LONGUE-LOST DARWINISM
In recent decades, a completely new philosophical trend in language has emerged in international linguistics, which seems to be opposite to the generally dominant view of language until recently. It draws on the roots of the theory of language development of positivist linguistics, which came to dominate around the middle of the 19th century, and its fundamental begins to question the validity and correctness of his theses. In the foreign scientific publications described above, we can almost without exception observe the development of this new — but in reality very old — approach to language. Ultimately, it is a matter of extremely cautiously and gradually, but actually step by step, returning to the linguistic views that existed before the general spread of the methods and language understanding of classical Indo-Germanic studies, which — we note now — were much more complex, comprehensive and rational than the one-sided "linguistic microscopy" introduced by Indo-Germanic studies, as is shown by the alleged temporality of sounds alone. "development" — a linguistic research method that Max Müller aptly named at the time. This method sharply separated the individual language families from each other, and, abstracting them from almost everything else, viewed them as things in themselves, attempted to reconstruct them separately, with the help of "regular sound changes". Thus the strange situation developed that in the etymological large numbers, sometimes declaring them to be of Indo-Germanic, Ural- dictionaries of the individual language families, which were believed to occur in a regular manner over time, we can find such basic words in Finno-Ugric, Altaic or Semitic origin — which with identical or very similar sound forms and meanings, they also occur in other language families. — Incidentally, these common words -- also with identical meanings and parallel sound forms -- are mostly found in the Sumerian language. The rejection of the thesis of rigidly separated language families, the denial of the inference of hypothetical basic languages, the systematic demonstration of the absurdity of "linguistic paleontology", all these are tangible manifestations of this new approach. With unmistakable determination, they question the entire positivist linguistic concept, almost all of its basic theses.

In these cautious initiatives, in addressing the fundamental methodological and ethical issues of 19th-20th century European linguistics, in order to objectively clarify and define these methodological and ethical foundations,
459
===== PAGE 454 =====

In the efforts aimed at, we must see, apparently, the undoubtedly manifest precipitations of the testimonies of the ancient Proto-Asian linguistic monuments. Even if the critics do not mention them explicitly for the time being. Every sober-minded professional linguist with the appropriate logical abilities must sooner or later realize that the massive phonetic and semantic similarities and close parallels of Sumerian root words with the words of modern languages, which appear multiple times in polysemantic words, cannot be accidental, and that as a result, something is wrong with the theory of "regular sound changes". According to this thesis, it is simply impossible that in a very large number of cases the sounds of words did not change at all or hardly changed at all over a period of 4-5000 years. And this is exactly the case with the Sumerian linguistic monuments.

This recognition, however, necessarily requires a thorough, objective review and reassessment of the reliability of the previous linguistic theses and traditional language research methods, covering all details. However, for this we first need to know what we want to revise: the view of language of the last 100-120 years, which we called "linguistic Darwinism" in the title.

So let's start with the most basic methodological requirement, a precise definition of the concept. What do we actually mean by the term "linguistic Darwinism"? First of all, we can state that linguistic Darwinism cannot be identified simply with Indo-Germanic studies, nor even with the thesis of sound laws and regular sound changes, although it is very closely linked to both, almost inseparably, nor can the positivist linguistic views with a characteristic 19th century flavor have very significantly prepared the ground for the spread and then dominance of the evolutionary-Darwinist approach in language research. However, linguistic Darwinism means significantly more than all of this.

We must take into account that before the advent of Darwinism, there were many researchers in linguistics with a broad perspective, a sufficient critical sense and sound logic who could have effectively countered the "linguistic microscopy" that was effectively promoted mainly by J. Grimm and F. Boppe, and which relied primarily on Grimm's earlier work, which was undoubtedly of epoch-making significance, and whose practitioners gave it the beautiful sounding name of "linguistic historical-critical comparative method", by constantly pointing out its shocking one-sidedness, its lack of foundation in linguistic philosophy and principles. Just as such voices could be heard in great numbers in linguistics, until Darwin's theory of biological evolution was originally only a worldview-460

===== PAGE 455 =====

It has not changed. It can be clearly stated from the linguistic debates of the middle of the 19th century that without the Pänderwinist global development approach, linguistics would have remained within the boundaries clearly drawn for it, that is, it would have limited itself to the horizontal analysis of language phenomena, and would not have come forward with the claim that linguistics is almost exclusively entitled to decide the most diverse historical and cultural history questions -- based on the dogma of the only salutary "language development".

This hybrid can only be explained by the Darwinian worldview turning it into an ideology, even a fetish, an idol, and extending the concept of gradual, regular "development" to everything in the universe. Well, this "regular development" was thought to be captured in Grimm's "regular sound development" thesis. After the publication of Darwin's epoch-making works, especially his book "The Descent of Man" (1871), in which he also discussed the origin of human language, the views adopted from him and soon diligently further varied became definitively dominant in linguistics. One of the most prominent figures in Indo-Germanic studies at the time, August Schleicher, was particularly enthusiastic about the new doctrines. As early as 1863, he published his work "Theory and Language Science", in which he praised almost all the theses of Darwin's first book on the subject, "The Origin of Species", published in 1859, and found them to be perfectly applicable to linguistic research. It was primarily Darwin's concept of the struggle for existence that captured Schleicher's imagination, which was saturated with Indo-Germanic cultural priority ideas, and accordingly in the aforementioned work he notes with pleasant satisfaction: "In the present period of humanity's life, the languages of the Indo-Germanic tribes are the victors in the struggle for existence." In Hungarian: "In the present phase of humanity's life, the winners in the struggle for existence are above all the languages of the Indo-Germanic tribes."

At this point, the fatal error occurred: when Darwinian theories explaining the development of the organism of living beings were uncritically transferred to the stages of the history of languages, and languages were interpreted as some kind of natural phenomenon, independent of human consciousness or only loosely, secondarily connected to it, as self-existing, independent organisms that live and develop according to their own internal objective laws -- namely, the "laws of sound". Since the spread of these views, we can speak of "phonetic Darwinism", since the pan-Darwinian worldview provided the ideological - not to say dogmatic - foundations for Grimm's laws of sound, his thesis of regular sound development,

Under the influence of these doctrines, Schleicher established the theory of the Indo-Germanic language family, and Finno-Ugric linguists also developed the "Uralic", "Finno-461"

===== PAGE 456 =====

Ugric"&s , Ugric" basic languages , stages of development. As Altheim, for example, states: "The linguistic family tree theory, which is the basis of the previous view, is not coincidentally the same as the biological theory of descent. Darwin and Schleicher were quite in agreement in their basic views." (Geschichte der lateinischen Sprache, p. 7) In Hungarian: "The family tree theory of languages, which is the basis of the previous view, is not coincidentally the same as the biological theory of descent. Darwin and Schleicher were perfectly in agreement in their fundamental views." G. Solta also expresses the same opinion almost word for word in his treatise "Gedanken zum Indogermanenproblem".

Also a consequence of Darwinism — and the positivist approach — was the persistent effort to produce frameworks for linguistic phenomena similar to the formulas of mathematics and physics at all costs, and thereby, in their opinion, Schleicher proclaimed, to elevate linguistic research to the heights of the exact natural sciences. to create "natural scientific" foundations so that — as

As a result of this effort, Grimm's sound laws and regular sound changes became definitively universally valid dogmas, which were considered to be excellently suited to serve — at least apparently — as solid, almost mathematically precise basic laws of linguistics. For this reason, they practically exclusively practiced the so-called historical-critical comparative method of language, the "linguistic microscopy", which examined only the supposed regular changes in sounds over time, the so-called development, and practically ignored all other circumstances.

The vocabularies of individual languages and dialects were collected in huge monographs and lexical works, and for almost every dialect its own special sound laws were developed, together with "substitute sound laws" that were considered applicable to the mass of exceptions. As a result, such an enormous amount of literary material was accumulated that fewer and fewer researchers were able to navigate linguistic issues outside their own narrow field of expertise and to form a comprehensive picture of them, at least to some extent. Thus, slowly but surely, ever higher walls were erected between the individual linguistic specialist groups, even within Indo-Germanic studies itself, where representatives of Romance, Germanic, Iranology, etc. locked themselves in ivory towers in their own narrow fields of research, and with a few exceptions, they barely recognized themselves in the thematics of the neighboring specialist group. The mere Philology, the sterile obsession with sound changes, and linguists have gradually lost all sense of larger, more comprehensive, more general linguistic issues.

However, the Schleichers were not consistent in one thing. While Darwin derived the modern human species from a single ancestor, i.e.
462
===== PAGE 457 =====

interpreted the biological development of humanity as monogenetic – 8 in this paleoanthropology essentially agrees with Darwin even today –, while the supporters of linguistic Darwinism have denied and still deny today the possibility of a monogenetic origin of human languages.

We do not want to get involved in the millennial debate between monogenesis and polygenesis, much less take a position on this extremely complicated issue. However, we must definitely point out the superficial and fundamentally unscientific procedure with which, at that time, especially Schleicher and AF Pott, even the possibility of partial, areal linguistic connections was rejected, and

researchers who argued for a possible common ancestral origin of the West Eurasian language families were dismissed. Let's look at a couple of typical examples.

Pott has been scathingly criticizing a book by Professor G. Ph. Chr. Kaiser, a professor of linguistics at the University of Erlangen, published in 1840 (Über die Ursprache oder über einer Behauptung Mosis, dass az Ur 1840. yizünden? — he writes — such as alle abstammen). How can one dare to do so? After all, the great French encyclopedists had already established in the 18th century that the common ancestral language of humanity was only a "pleasant chimera", Sprachen der Welt von einer einzigen, der Noachistischen i.e. a ridiculous phantom.

We can ask, in passing, what kind of archaic linguistic studies and data did the "great French encyclopedists" rely on to formulate this incontrovertible statement?

their revelation? At least we do not know of any scientific research in this direction from the 18th century, when even the relationship of the Indo-Germanic languages was unknown.

5 After this truly highly scientific "refutation", Pott does not even hesitate to openly admit that he "kenne ich indessen nicht aus eigener Anschauung" (I do not know Kaiser's book in question from my own perspective) - that is, he has not even read the work he has dismissed. Further commentary is unnecessary.

R. v. Raumer, who in the early 1860s demonstrated a series of word similarities between Indo-Germanic and Semitic languages, did not fare any better. Pott and Schleicher were not left behind with the "scientific" refutation here either.

Pott explained in great detail - let us pay attention to the "overwhelming" logic! - in principle not absurdly, but only in the distance the physics for the languages moves on the unscientific level of all kinds of "Jagd nach einer mobile. - Again, it is unnecessary to add anything to this truly comprehensiveness. It speaks for itself,

"methodical" argument-

Schleicher accused Raumer of having no idea what the rules of the game of phonetics were, "modern science of the hand" — by which, of course, Grimm's which, as a dogma he held sacred, could not be shaken even by concrete linguistic facts. Since Raumer also used Semitic words with the initial sound "uh" as Germanic words beginning with "h",

463

===== PAGE 458 =====

compared with the Germanic "h", in Schleicher's eyes this was unscientific from the start, because according to Grimm the Germanic "h" supposedly goes back to Indo-Germanic "k", possibly "kht" or "gh", and besides, it is "kein ursprünglicher Laut" - he notes - i.e. "not an original sound".

In connection with this question, we recall Chapter 3 of our Book I, in which we showed that Indo-Germanic studies misunderstands or omits this in all those cases when considered the Germanic "h" to be a "non-original sound" the corresponding words of the so-called Indo-Germanic basic degree - in the case of the Latin "habeo" (cf.) or the German Herz" (heart) and the Old Indic "hrd" (cf.) we find the "h" sound, like in German "have" (to have, (1. Book I, pp. 116-123).

In his response to Raumer (Schleicher and the ancestral relationship between the Semitic and Indo-European languages), he calmly notes: "Mr. Schleicher treats the relationship that I have demonstrated between the Semitic and Indo-European personal pronouns with a mocking sneer. But I calmly leave it to the future to decide on whose head this sneer will fall."

It is already clear from the results of our study series so far that these were prophetic words. In the meantime, it has been proven that the personal pronouns in question are parallel not only in Semitic and Indo-Germanic languages, but also in Uralic-Finno-Ugric and Altaic languages, and the common source of close correspondences was the Sumerian language:

Uralic Sumerian Indo-European Pott and Schleicher

therefore obviously came from the same linguistic evolution assumptions, and by forcing a faulty approach they significantly contributed to the fact that linguistics fell into the for; for-e (you) impasse from which it has still not been able to get out. First of all, we can consider them the midwives of linguistic Darwinism, which, born in the second half of the 19th century from the marriage of Grimm's sound laws and the blind belief in evolution of pan-Darwinism, completely displaced the previous global linguistic approach from linguistic research, and, based on the one-sided, unreasonable arguments presented above, branded it as unscientific, "metaphysical linguistics".

This school, which self-consciously calls itself a "positivist" school -- a name that naturally implies that everything else

===== PAGE 459 =====

research method "negativist", i.e., unscientific -- rejected the oldest philosophical concepts of language, but also the philosophy of language itself, and even , even banished the logical aspects of language from linguistics. Under the influence of the Darwinian "development" dogma, in linguistics, only one-sided, so-called historical linguistic studies were considered scientific, i.e. they were mainly concerned with the detailed interpretation of completely insignificant linguistic phenomena, with philological trifles, and not least with how the identical words handed down to us from different linguistic monuments with very often different phonemes can be interpreted with the help of "source criticism" in the sense of Grimm's ant transformations. "correct", "correct" sound form then naturally always became the one that could be classified in the appropriate columns of the sound fragments, while the rest were declared scribal or copyist errors, or were simply silenced: let us remember, for example, the sounds b/p, d/t and g/k, which are completely randomly mixed in Old High German language monuments (Book II, pp. 109-110 and 129).

The "positivist" linguistics also captivated the vast majority of Hungarian researchers to the point that they ignored all previous efforts, such as the very significant and promising initiatives of Czuczor and Fogarasi, branded them as completely backward and obsolete, and from then on they were only concerned with Finno-Ugric studies of the history of language based on sound laws. The still undisputed basic truths of the philosophy of language, such as the organic intertwining of language and logic, or the inseparable relationships between form and function, no longer even emerged among the objectives of linguistic studies. As a result, even today they cannot determine the essential nature of the stem with sufficient certainty, nor can they clearly illuminate its universally valuable aesthetic, thought-generating and development-regulating norms. In other words, we are groping in complete darkness, so to speak, precisely in the field of the most specific, most fundamental linguistic questions.

: on the other hand — perhaps as an overcompensation for this deplorable state of their own court — they intervene in all sorts of prehistoric and cultural history issues in a voice that tolerates no contradiction,

The exclusively "positivist" linguistics that has been pursued for more than a hundred years has led to the point that today hardly anyone has reliable knowledge of the basic principles of the philosophical approach to language, which is discriminated against as "metaphysical". Questions of primary importance, such as whether the languages that have proliferated over the millennia ever had some kind of common substrate, and if so, whether this was a semi-animal makopás yolt or the work of a language creator (or creators) with high intellectual abilities, are mostly not even dared to be raised. And if these paleolinguistic problems are still touched upon very rarely, they are általa-465

===== PAGE 460 =====

In short, they are dealt with by rejecting any 651 that would go beyond the framework of the language families developed in the 19th century, and even the possibility of such a thing is categorically denied. Polygenetic linguistic origins, which are considered a priori indisputable, are usually treated with the so-called . They support this with a "principle insight" that the "collective consciousness" imposed the meaning of the different sound groups + i.e. words -- differently in each ancient tribe, according to the "internal norms" of the respective ethnic group.

We don't even want to dispute the probability of such a thing. The only surprising thing is that the moment they start talking about "Proto-Indo-Germanic", "Proto-Finno-Ugric" or "Uralic" basic language, they suddenly forget about these collective tribal "consciousnesses", "internal norms", and -- in these cases provide the highest independence they find it self-evident without further ado that the "collective consciousnesses" and internal norms of ethnic groups living exactly the same in the case of the later "Uralians", scattered over vast areas, consisting of numerous hordes, diametrically opposed signs, genera, tribes, and anthropologically different from each other, were, by the inscrutable will of fate, "Finno-Ugric" or "Indo-Germanics" - but only in these cases - and consequently the phonemes and meanings of their original words were the same, or at least they were very similar to each other.

We have already seen so far, especially in Chapter 5 of our Book I, that the truly surprising coincidences and parallels of these ancient words cannot be fitted into the framework of classical language families, but can be traced back to the ancient Proto-Asian linguistic koine, or more precisely to its dominant linguistic state for millennia, and to Sumerian, at least in Western Eurasia.

We cannot conclude the discussion of the question of "linguistic Darwinism" without referring — at least in a few lines — to the general scientific epistemological ideas of the middle of the 19th century. The boundless respect and admiration of the linguists of that time for the exact natural sciences, the laws of mathematics, physics, and chemistry stemmed from the firm conviction of 19th-century science that the laws of classical physics are universally valid throughout the universe, and with their help all terrestrial and celestial phenomena, the origin, history, and development of the cosmic world order can be known and interpreted without a trace. This conviction was only strengthened by the seemingly dizzying results of 19th-century natural science research, and was elevated to the realm of scientific axioms — indeed, we could say, religious dogmas.

Well, it is within this broader framework that we must view the 19th-century linguists' obsessive insistence on "sound laws", which is completely understandable and can even be said to be consistent in such an atmosphere.

466

===== PAGE 461 =====

As mentioned above, the Schleichers' efforts were primarily aimed at giving linguistics a solid, natural scientific foundation and at conducting linguistic research similarly to the methodological principles of the exact natural sciences.

However, today, scientific research, primarily modern physics - especially atomic physics and astrophysics - has long since moved beyond the scientific epistemological beliefs of the 19th century. In the 19th century, everyone would have considered it absurd to think that the laws discovered and developed by physics

There are limits to their validity. However, atomic physics has shown that the axioms of classical physics, even within the framework of physical reality, are not valid for all phenomena: they completely cease to be valid in the border areas. For example, on the scale of atoms, electrons and other elementary particles — the so-called, but they are equally inapplicable on the scale of the macrocosm, i.e. the phenomena of the universe, stars, star systems in microcosm —, (galaxies) for explanation.

The validity of the axioms of classical physics is now known. The limits of the laws of atomic physics can also be outlined, at least in part. Modern natural science will naturally continue its research as long as it can draw these limits precisely. However, the new scientific objective differs significantly from the old one. While classical physics was certain that it would one day be able to explain the phenomena of the universe in full with its own methods and instruments, modern natural science assumes with a probability that is on the verge of certainty that this attempt - despite all progress - is doomed to failure. According to Husserl's recognition, which is one of the most essential principles of modern phenomenology, neither the sphere of reality of classical physics nor the detailed realities of atomic physics - that is, the detailed realities that can be known with their help - can be identified with absolute reality itself, viewed in its entirety. Heisenberg's so-called The uncertainty principle (Unsicherheitslehre), according to which we cannot actually learn about pure natural phenomena in their reality through physical scientific experiments, but can only examine their substantially influenced, as it were, denatured state by experimental means, those phenomena to which the material under examination is forced by the experimental scientist himself with the help of the experimental apparatus.

However, all these modern scientific and epistemological discoveries are ignored by phonological linguistics. Linguistics has remained at the epistemological level of 19th-century classical physics and still firmly believes that a law that is in principle identical in all respects to the axioms of classical physics, namely, the , phonological law, is the basis of all linguistic phenomena.

===== PAGE 462 =====

"laws" and the doctrine of "regular sound changes" can be applied to all phenomena of languages without restriction, its validity has no limits, and with its help the development of human languages, the nature of the relationships between individual languages and language families, their course over time, etc., etc., can be fully understood and fully explained. We emphasize: in the world of modern natural scientific discoveries, this concept is outdated and unscientific in every respect, even when applied to the exact sciences themselves -- let alone in a field to which the laws of the exact sciences cannot be applied even in their 19th century classical physical interpretation. Language is not some kind of natural phenomenon — as the linguistic Darwinist perspective imagines — but an acoustic communication system consciously developed and continuously improved by humans to facilitate communication, the exchange of thoughts and experiences between them.

a system that, according to them, is a function of human reason and consciousness in all its phenomena, rather than some kind of natural scientific law independent of our consciousness. Consequently, we can state as a fundamental principle requirement that no law introduced in linguistic research by analogy with the axioms applied in the study of physical phenomena, borrowed from the methods of the exact natural sciences, can be used successfully, because in the case of languages we are not examining objective, material phenomena, but rather a characteristic form of manifestation of human consciousness. Therefore, since languages are indisputably characteristic manifestations of the functioning of the human brain, their systematic research is possible only from the perspective of human psychology, the laws of human psychology, and primarily from the perspective of the functions of the human intellect, the associational abilities of our brain, and the processes of image association, that is, in general, most importantly in the semantic projection of the words of languages, and with the help of the exploration and examination of the logical features of their structural structure.

All these theoretical considerations therefore also support our conviction from an epistemological perspective, which we have already explained in detail in Chapters 3, 4 and 5 of our Book I — approaching the question from a completely different angle —: with the help of the combined semantic-phonetic language research method we have used so far, we can not only research the development of languages and language families and their relationships with each other much more comprehensively and effectively than with the usual one-sided methods of linguistics that only pay attention to "sound developments" that are considered regular, "sound law" methods, but in addition, the indisputable fact that the development of language is a function of the image association processes of human consciousness and the brain directly requires the application of this method.

468
===== PAGE 463 =====

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In other words, the belief of phonological linguistics that languages are, so to speak, independent of our consciousness, autonomous organisms with their own imaginary internal "develop" according to their own laws, beneath the speakers' consciousness, or at most at the lower threshold of consciousness, is a completely unfounded, fundamentally unscientific position. However, Finno-Ugric studies still holds this view today, as Róna-Tas writes, for example! "Language is a dynamic, objectively existing social phenomenon, relatively independent of consciousness..." (Language affinity, p. 343)

Here is the fundamental logical fallacy. It is not "language" that is relatively independent of consciousness, but its applied form, everyday speech. And this is a very important difference. Róna-Tas brings up the almost automatic reactions of a practiced car driver as an analogy to prove the above-quoted theorem, who, barely knowing the mechanism of the car, subconsciously uses it to achieve his goal. However, the analogy is fundamentally flawed. Indeed, the driver applies the practiced movements with which he operates the car relatively independently of his consciousness, but we hope that Róna-Tas does not believe that the mechanical engineer designed that car on the threshold of his consciousness, in a state of lunatic mania. Similarly, there is a small difference between the student who memorized the Pythagorean theorem and Pythagoras.

We do not know how languages developed at the dawn of ancient times. However, it is undeniable that they constitute more or less logical systems of thought communication — to the extent that the intellectual abilities of their creators and refiners have expanded. The logical system of languages should therefore be constantly and consciously developed, nurtured, and cleansed of inaccuracies and irrationalities. Anyone who denies this and lets the vulgarities of public discourse run wild, under the slogan that such things are "language development" and that we are powerless against it anyway, is not doing a good service to the development of humanity.

Language is undoubtedly one of the most important pillars of culture. With its impoverishment, communication of ideas becomes more difficult, social contacts become shallower, and intellectual development, which is now tangibly evident not only in Hungary but almost idea- "y" - in the narrow meaning of 826, understood as the exchange of ideas - gets stuck, and over time regression occurs. This throughout the world, is undoubtedly due to the more than one hundred-year rule of the phonological linguistics that preaches "language development": teachers who teach the mother tongue already let loose linguistic nonsense at school, because at the time they themselves only learned that languages develop independently of our consciousness.

All these issues are discussed very thoroughly and in detail by Ferenc Kemény in his work "Das Sprechenerlernen der Völker", with clear examples.

===== PAGE 464 =====

As proven by the Dacian series, the language degradation that is taking on ever greater proportions as a result of the cultural policy of tonal linguistics (1. Ke. = mény: id.m. pp. 25-27., 38-40. and 117-118.)

Here we would like to recall that Ferenc Kemény's insights and theses greatly support and strengthen our own paleolinguistic theory, and that this fundamentally important work of his provided significant assistance in developing our approach to language formation and development outlined so far.

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470
===== PAGE 465 =====

Chapter 3. WORDS OF URAL-FINNOGORIAN ORIGIN IN THE HUNGARIAN LANGUAGE Methodological guidelines
In Chapter 1, we were introduced to the latest research in Indo-Germanic linguistics and prehistory, the most important result of which is that in the case of the Indo-Germanic languages we cannot speak of common ethnic and linguistic foundations, genetic kinship or language family tree, but that they most likely developed as a result of very complicated ethnogenetic and multi-stage language equalization processes, roughly in the sense of areal languages and language alliances; moreover, in relatively late times, after the turn of the 3rd/2nd millennium BC.

However, in Chapter 5 of Book I, we also pointed out that the first foci of the Indo-Germanic and Semitic language groups apparently arose through a more or less distorted adoption of the proto-Asian language koine of the Neolithic-Early Metal Age, or rather its dialects, i.e. through pidginization-like processes, and then subsequently consolidated into separate language groups in larger geographical regions as a result of independent internal language development and the mutual interactions of neighboring, similar pidgin-like languages. These linguistic blocks, which are different from each other in their most ancient basic layers, independent, but secondarily areally equalized, and in which, in addition to their own, ancient, unique linguistic material and the characteristic similarities and parallels that have developed as a result of the language alliance-like unification process, we can also find hundreds of more or less distorted basic words of the proto-Asian ancestral language, as evidence of former pidginization, are today called language families.

Our paleolinguistic approach — with reference to the West Eurasian language families — can therefore be briefly and schematically summarized as follows.
1. Stage: in the Upper Paleolithic, Early Mesolithic Western Eurasia, we originally find countless smaller or larger linguistic communities with different vocabularies and language structures - but in no case affecting thousands of kilometers - which independently developed the meanings of the various sound groups - i.e. words - according to the "own internal norms", "collective tribal consciousness"of the individual fishing-hunting, gathering clans and tribes,

Stage 2: With the consolidation of the East Asian food-producing economic form, the population in these areas increased dramatically, and at the same time, cultural and linguistic ties also became stronger.

471
===== PAGE 466 =====

became more and more common, which eventually led to the emergence of a more or less unified cultural and linguistic koine around the 6th-5th millennium BC. The explosive increase in population density from the same period forced mass migrations, and as a result the languages of the primitive peoples of the nearer and more distant areas gradually came under the massive influence of the dialects of the large number of advanced-cultured Proto-Asian settlers, and

They incorporated a mass of words from the Proto-Asiatic koine language into their original, separate languages, mostly with pidgin-like distortions. At the same time, their grammar; their language structure also underwent more or less changes - although presumably not to as great an extent as their vocabulary.

3rd Stage: the primary pidgin-like communicative languages (lingua francas) thus formed on the periphery of the Pre-Asian cultural zone subsequently became areally equalized in certain regions as a result of independent internal linguistic development, as well as favorable geographical factors, closer cultural relations, and smaller migrations, and secondarily closer linguistic relations developed between them: these areal language associations formed in larger areas through mutual interactions are called basic languages of language families by European linguistics.

4th Stage: the latter-mentioned paleolinguistic processes can be dated to the 4th-3rd millennium BC, i.e. precisely to the era of Sumerian cultural and linguistic hegemony, and since the mass migrations caused by overpopulation continued uninterruptedly during this period, especially in the second half of the 3rd millennium and the first half of the 2nd millennium half, as a consequence of the Mesopotamian burden of the Semites, we can very probably state that the above so-called "language family basic languages" - in reality, however, areal language associations - in their final development and consolidation were in the highly developed, urban, organized imperial culture of that time.

We must attribute a primary role to the language of Sumer, which dominated large areas, and to its migrating masses.

We have now reached the point where we can extend the general linguistic development studies of our findings to the Ural-Finno-Ugric languages, the most important, dominant representative of which -- as we have shown above (1. 401-406, pp.) -- is Hungarian.

All traditional Finno-Ugric linguistic and pre-historical ideas agree that - assuming a common ancestral language, ancestral people and ancestral homeland - the early Magyars were located somewhere in the southernmost part of the Uralic or Finno-Ugric block. Assuming - but not allowing - that the ancient Magyars-472

===== PAGE 467 =====

If they once really belonged to this Uralic language group, even in this case it is not only permissible, but methodologically fundamental.

It is necessary to examine the relations between Hungarian and the ancient southern languages, because it is highly probable that the cultural and linguistic influences from the south reached the Finno-Ugric tribes further north through the mediation of the ancestors of the Hungarians living in the southernmost part of the country.

That is, even if we accept in principle, in line with the Finno-Ugric school, the possible existence of some kind of loose ancient unity, we cannot exclude ourselves from the research of the reasonably and rightfully assumed cultural and linguistic influences of the pre-Asian primary cultures; and this is especially not the case with the southernmost pre-Hungarian ethnic group and the Ösnyely.

Let us not forget that in the middle of the 3rd millennium BC the massive influences of the Proto-Asian-Sumerian culture reached the regions of Kuban (Maykop), Pontus and Ukraine (Tripoli) (Book 1, pp. 25 and 39-40). On the other hand, Gyula László demonstrated with very compelling archaeological and linguistic arguments (The Earliest Stages of Our Primitive History) that the Uralic or Finno-Ugric homeland

— As far as we can talk about such a thing at all, it should be sought much further south than the Oka-Káma-Bjelaja area. There are many things to think about in this regard, László

The dictionary statistics of Gyula's mentioned work also testify to the fact that the Hungarian language occupies a unique and separate place among the Uralic-Finno-Ugric languages (for details, see 1. below, in the relevant subsection).

It is also necessary to mention some very remarkable insights of Elemér Móór, who sometimes holds rather heretical views, but undoubtedly has a very sharp critical sense. Although Móór is unquestionably based on the Finno-Ugric perspective, he has nevertheless emphatically pointed out on several occasions that the traditional ideas of the Finno-Ugric school are often in no way compatible with linguistic and historical data. Among other things, he considers our so-called Old Turkic loanwords to be much older than Hungarian linguistic and prehistoric research following Gombocz — and he is certainly not wrong in this (1. y. book, pp. 235-236). According to his thorough argument, the Sabirs and Onogurs, who allegedly originated from Western Siberia and the Urals, in the 1st century AD. We have already pointed out against the thesis of his 5th century wanderings (Book 1, II, p. 265).

We saw that he hit the nail on the head on this question as well, because the Sabirs and Avars migrated to the Kuban region along the southern shore of Lake Caspian and through the Caucasus, not from Western Siberia and the Urals, as Hungarian prehistoric research teaches.

However, from the point of view of our current questions, the most important thing is his recognition that the Hungarians were taught horse breeding and farming by a people who came from the south. According to him, this people was Caucasian, but they were strong and powerful.

===== PAGE 468 =====

with full Asia Minor connections. If we see how many unknown words of ma- origin were taken There is a lot of talk, we must state that the Hungarian language — and with it the Obi-Ugric language — has a lot of from this southern, probably Caucasian-Scythian language - he writes in his paper "The main problems of Hungarian prehistory". If this thesis is correct - continues Móór, then we must derive all our words and suffixes --, from this language (e.g., they are missing from the locative language. (The Sumerian suffix of the locative is "-ta", "da"; cf. "ott", - - suffix), which are only found in Obi-Ugric, but in other Finno-Ugric "oda" and the ancient Hungarian place name suffix "-d-")

Like Gyula László, Elemér Móór also believes in the multi-branch ethnogenesis of the Hungarians. Regarding our tribal names, he states that the generally accepted Turkish and Finno-Ugric etymologies are not proven at all. For example, the Finno-Ugric "Nyék" has no equivalent in any Finno-Ugric language. Similarly, the Turkish derivations of the other tribal names are on very shaky foundations. The seven tribes had a heterogeneous composition, but in broad terms they are still related ethnic groups that lived closely together for a long time (Móór: Studien zur Früh- und Urgeschichte des ungarischen Volkes).

We can therefore see that even based on an exclusively Finno-Ugric ethnogenetic and linguistic development theory, we cannot rule out the possibility of a southern, East Asian expansion.

the probability of contacts and contacts of Neolithic and Early Metal Age cultures with the supposed Urals or Finno-Ugric peoples. Moreover, archaeological discoveries of recent decades clearly show that the ancient Pre-Asian civilizations extended their massive cultural influence to Turkmenistan, the Pamirs and Altai in the east, to Southern and Central Europe in the west, and to the Southern Urals, Oka and Middle Volga regions in the north. But not only these: mass migrations can also be detected, on the one hand, from Turkmenistan to the West Asian steppe, all the way to the Altai and Yenisei regions, and on the other hand, from the Transcaucasus and Kuban regions to the Urals and the Middle Volga.

That is why we consider it a fundamental methodological omission that our exclusively Finno-Ugric linguistics and prehistory do not want to acknowledge the results achieved in the last 50-60 years in the research of the more distant effects of the ancient pre-Asian civilizations. Finno-Ugric studies, and in its wake Hungarian pre-history research, still clings convulsively to the hypotheses formulated in the 19th century and conceived in a pan-Indo-Germanic spirit, according to which the entire Eurasian steppe was once populated by Indo-Germans or Indo-Iranians. As a result, they only consider certain vague "Indo-Germanic" or, within this, not much more specific "Indo-Iranian" or "Ösirani" influences on the ancient Finno-Ugric peoples, excluding all other possibilities from the outset. Yet, the languages of the ancient pre-Asian cultural peoples, led by Sumerian, whose linguistic monuments are thousands of years old,

474

===== PAGE 469 =====

predate the earliest Indo-Germanic sources, we have also known for several decades that they were almost without exception inflectional languages, meaning that their language type was the same as the Hungarian-Finnish and Altaic languages.

According to the evidence of the aforementioned archaeological data, it is therefore much more likely that these ancient agglutinative languages influenced the development of the languages of the more northern regions much earlier than the Indo-Germanic or Indo-Iranian branches that appeared much later. These linguistic relations, viewed globally, can apparently also be attributed to the fact that agglutinative languages are found everywhere in the northern-northeastern fringes of the primary cultural circle of East Asia, from the Volga to Mongolia.

To avoid misunderstandings, we emphasize that we are only referring to the general linguistic influences of the Proto-Asiatic Koiné language in these areas, without raising any closer questions of genetic language affinity for the time being.

However, Finno-Ugric linguistics rejects even the possibility of such globally viewed Nelyian influences. At the beginning of our century, the position was formed that the distance of several thousand km separating Mesopotamia from the Ural region makes any linguistic connection illusory from the outset. This argument, with which the investigation of possible connections between the Hungarian and Finno-Ugric languages and Sumerian is rejected a priori, has not fundamentally changed since then, despite the aforementioned archaeological discoveries. Our linguists and prehistorians did not even think about the fact that in the 1000s BC. In the first half of the 2nd millennium BC, in the region of the middle Volga, the Oka and the Southern Urals, i.e. precisely in the region of the traditionally assumed Ural-Finno-Ugric homeland, particularly massive Kuban-Transcaucasian, ultimately distinctly Sumerian cultural influences appeared, in parallel with the massive influx of settlers from the south, who were agriculturalists and animal breeders and had a developed bronze culture, lasting for almost half a millennium (see Book I, pp. 46-51).

We have already seen that Hungarian research (e.g. Bartha A.) believes that these colonists "possibly" discover ancient Iranians (Book II, p. 47). However, for theoretical and methodological reasons, we ourselves reject all kinds of "ancient Iranian" ideas until we have thoroughly and conscientiously examined those languages about which we have not only ideas, but also concrete, contemporary linguistic monuments - that is, from the turn of the 3rd/2nd millennium BC, and precisely from the southern neighborhood of the original residences of these Volga-Ural colonists, the Caucasus-Transcaucasus areas.

Such languages are primarily Sumerian -- the dominant cultural language of East Asia at that time -- and Hurrian, the vernacular language generally widespread in Northern Mesopotamia and Transcaucasia during the period in question. So if

475

===== PAGE 470 =====

If we want to proceed methodically, then we must first compare these ancient languages in detail and objectively with Hungarian and Finno-Ugric, and if in this comparison we do not find any significant similarities or parallels, only then can we think of languages that were still unknown in the 2nd millennium BC, such as Iranian. We are convinced that only this process can be called methodical and scientific.

All the more so because we also have concrete archaeological and historical data from this period about the Hurrian migrations starting from Transcaucasia, as well as the sudden, seamless appearance of the formal treasures and craft skills of Sumerian bronze art in the princely tombs of Alaca Hüyük and Horoztepe in northeastern Anatolia (2500-2200) and in the Maykop kurgans along the Kuban River (2400-2300); then 2-3 centuries later, the same bronze art appears in the Volga-South Ural region, accompanied by the aforementioned southern colonists (Book II, pp. 39-51 — see also literature).

It should also be noted that from the same period, during the IIIrd Uri Dynasty, we also know of a Sumerian commercial-colonial settlement in Anatolia, at Kane5 in Cappadocia. (Kültepe), which fell into the hands of the Old Assyrians after the fall of the IIIrd Uri dynasty and flourished for another [6,200 years (1. Schmökel: Das Land Sumer, p. 88). In contrast, we do not know that Finno-Ugric linguistics and prehistory conducted any such studies before developing their ancient Iranian concepts.

Regarding the question of southern immigrations at the beginning of the 2nd millennium, we must also consider the statement of Finno-Ugric linguistics to be very remarkable and worth considering, that the disintegration of the so-called "Finno-Ugric unity", i.e. the beginnings of the separation and crystallization of the individual Finno-Ugric language groups, can be dated to around 2000 BC. In other words, Finno-Ugric linguistics places the period of the formation of Finno-Ugric language subgroups in the first half of the 2nd millennium, precisely at the same time as the appearance of the southern colonists. This striking chronological agreement suggests that the language (or dialects) of the southern settlers who arrived in large numbers and had a developed culture had a decisive impact on the languages of the sparse indigenous groups found there, at the Mesolithic-subneolithic level of culture, and strongly influenced, one could say determined, their later development. In other words, the process that Finno-Ugric studies interprets as the disintegration of Finno-Ugric unity apparently corresponds in time to the equalization of these languages, the formation of the areal Finno-Ugric language association, which was presumably consolidated by the unifying influence of the dominant southern cultural language in the second half of the 2nd millennium BC, in the Andronovo period.

Besides, it cannot be ruled out, and it is quite probable, that there were some kind of more or less 476

===== PAGE 471 =====

balanced contact languages, arealis, lingua francá“ka between some smaller groups of the languages of the indigenous fishing-hunting population (vő, II. könyv, pp. 333-334). However, these cannot yet be called "Uralic" or "Finno-Ugric" basic languages, at most partial pre-Uralic or pre-Finno-Ugric language associations. Their traces – if they can still be found – can be sought in the Paleosiberian, Paleo-Europid and Arctic basic layers of some modern Finno-Ugric languages, but especially in the Ob-Ugric, Samoyed and Lapp dialects.

However, we cannot count on the fact that these supposed pre-Uralic "lingua francas" will ever be explored in more detail, since we are talking about very ancient times and the complete absence of linguistic monuments. All in all, it seems likely that the later Lapp, Samoyed and so-called Ob-Ugric groups may have originally belonged to two or three such regional Arctic language associations limited to smaller areas (for more details, see 1. below).

However, Hungarian linguistic and prehistory research with an exclusively Finno-Ugric perspective remains faithful to its entrenched, one-sided methods in this matter as well. Despite the fact that, according to the clear evidence of archaeological finds, these southern colonists arrived in several waves, en masse, and were highly educated, and the archaeological remains of the local indigenous population appear only in the form of sparse, scattered Paleolithic-Mesolithic settlements consisting of one or two fishing-hunting families, with a very primitive culture, our linguists and prehistorians believe that the southern colonists soon became part of Hungarian prehistory.) It is not necessary to prove in more detail that this assumption can be called absurd in the light of the archaeological "they merged" into the local indigenous population, i.e. the Finno-Ugric people (1. Bartha A.: Society and Economy of the data available to us.

In the light of the aforementioned archaeological and cultural-historical data, we must therefore consider the comparison of the relics of the Transcaucasian Hurrian and the Mesopotamian Sumerian language with the Hungarian and Finno-Ugric languages as a primary methodological consequence. However, we do not yet know Hurrian well enough to use it for language comparisons. Apart from its declension type, we only know that it contains many Sumerian features in its structural elements and vocabulary, and that it was a close relative of the later Urartian language (1. J. Friedrich: Einführung ins Urartäische; ed.: Kleine Beiträge zur hurritischen Grammatik; IM Diakonoff: Hurriter und Urartäer).

Sumerian, on the other hand, is a sufficiently well-known language, with several thousand written records, longer and shorter texts, and Akkadian-Sumerian glossaries, from which the exact meaning of about 6-7000 Sumerian words has been determined. 15 Added to this is the eminent significance of the Sumerian language and culture in the history of culture: according to our current knowledge, it seems beyond doubt that it was the oldest cultural language of humanity, and that Sumerian civilization was the first,

477

===== PAGE 472 =====

culture in the true sense of the word, whose European and West Asian influences we have already become acquainted with (Book I, Chapter 1). It is also a well-known fact that hundreds of Sumerian words were transferred to later Semitic and Indo-Germanic languages (IH Schmökel: Die ersten Arier im alten Orient; W. v. Soden: Der Nahe Osten im Altertum; but cf. also Book I, Chapter 5). Based on these considerations, it seems beyond doubt that in the complex research of the formation and development of the Uralic-Finno-Ugric languages as a language family, we cannot dispense with the inclusion of ancient Sumerian linguistic monuments.

We believe that the simplest and most effective way is to proceed step by step, starting from the results of Finno-Ugric studies to date, and to include Sumerian in Hungarian linguistic research — in addition to the Altaic and Indo-Germanic languages. With this method, we can greatly expand our horizons, the study of our language becomes more complex, and above all, we can utilize the lessons of an inflectional language for a better approach to the development of the Hungarian and Finno-Ugric languages, which, with the period in question - the 3rd-2nd millennium BC — there are linguistic monuments from the same period.

However, this also means that in the case of parallels and correspondences, our conclusions gain a much more solid foundation than in the case of the Altaic and Indo-Germanic connections that have been considered by Finno-Ugric studies so far. Neither Finno-Ugric, nor Altaic, nor even Indo-Germanic languages have linguistic relics that go back to such ancient times, and thus the conclusions drawn from parallels and correspondences naturally remain very uncertain and hypothetical in every case, since we have to compare the hypothetical words of the Finno-Ugric or Uralic proto-language, which are also only inferred — i.e. again 058% extrapolated — Finno-Ugric or Uralic base language, not with concrete linguistic relics, but only with inferred — mathematically speaking: extrapolated —, assumed Altaic and Indo-Germanic proto-language word forms. At the same time, however, we must not forget that, in the absence of ancient linguistic monuments and historical data, even the mere existence of the aforementioned languages or language families is merely an unprovable hypothesis in the linguistically and historically silent prehistoric times in question.

So far, hypothetical Finno-Ugric Proto-Lingual words reconstructed using the method of assumed regular sound changes and sound laws have been compared with equally hypothetical Indo-Germanic or Altaic proto-language words, and moreover, assuming that all these basic languages actually existed in the 5th, 4th, and 3rd millennia BC — for which, however, we have no specific data in the case of any of the mentioned language families. In Hungarian, this method consists of four superimposed assumptions, which are "regular sound development" — itself just a hypothesis.

This method of language comparison was already established by Budenz in Finno-Ugric studies, and has since become one of the methodological foundations of Finno-Ugric language comparison.

===== PAGE 473 =====

remained full. Behind its application lies a firm, non-religious belief in the validity of sound transformations and the infallible certainty of the sound reconstructions of the Ss language deduced from regular sound changes. This blind belief still prevails in our linguistics today, despite the fact that Indo-Germanic studies themselves, which was the first to develop and apply this method, were thoroughly troubled by it after the discovery of the Indo-Germanicity of the Hittite language: it turned out that this oldest Indo-Germanic language showed the least in common, not only with the basic language word forms deduced with the help of "regular sound changes", but also with the reconstructed ancient Indo-Germanic grammatical system (I. pp. 76-77 and 410-411). These facts primarily led modern Indo-Germanic studies to the realization that the attempted reconstruction of the assumed basic languages of a language family on the basis of sound laws is theoretically impossible (I. pp. 407-424).

Let us now return to the starting point of our thought process: Finno-Ugric linguistic and prehistory research itself places the ancestors of the Hungarians on the southern border of the presumed ancient Uralic-ancient Finno-Ugric settlement area. For this reason alone, it is essential to look for the linguistic (and cultural) effects of the colonists who arrived from the south between 2000 and 1500 BC primarily and mainly in the Hungarian language (and ancient culture). If our working hypothesis is correct, then we should find the most Proto-Asiatic parallels in our language (and folk culture). Finally, if we manage to demonstrate such parallels in greater numbers, then it also follows compellingly that Hungarian conveyed the southern linguistic (and cultural) influences to the Finno-Ugric languages (and peoples), i.e. the Hungarian language (and ethnicity) in this case truly has a key position in the formation and development of the Ural-Finno-Ugric languages (and folk traditions).

This would close the logical circle, at the beginning of which we started from the assumption that the development of our language, its homogeneity almost without dialects, is the earliest evidence of our linguistic memories.

The emergence of the Hungarian ethnic group, the numerical dominance and cultural primacy of the Hungarian ethnic group among the Finno-Ugric peoples forcefully admit that our language and the pre-Hungarian ancestral ethnic group played a decisive role in the process of formation and consolidation of the languages now called Uralic and Finno-Ugric, but also in the cultural development of the Uralic-Finno-Ugric peoples.

Finally, we would like to note that the aim of this volume is primarily to criticize the methods, one-sided word derivations, and word explanations of Finno-Ugric linguistics that work with sound laws and regular sound changes, whether we look at our basic vocabulary considered to be of Finno-Ugric origin or at our so-called loanwords. Therefore, this volume will only present Sumerian-Hungarian linguistic similarities and parallels to the extent that Finno-Ugric studies can provide this information.

479

===== PAGE 474 =====

It is necessary to present the broader proto-linguistic contexts of our words claimed to be of Ugric origin or considered to be loanwords of various origins. The Sumerian-Hungarian correspondences found in the following chapters therefore do not cover all the demonstrable linguistic parallels, but rather represent, without any claim to completeness, only a few examples taken "ad hoc" to support our argument. We would like to deal with the systematic examination and detailed description of the Sumerian-Hungarian linguistic parallels in a separate volume.

Comparative linguistic methods in 19th-century Finno-Ugric studies

When examining the thesis of the Finno-Ugric origin of the Hungarian language, we must definitely remember the work of József Budenz, who came to Hungary from Göttingen in 1858 — at Hunfalvy's invitation — and who was the first to consistently apply the method of linguistic analysis that had become dominant in Indo-Germanic studies at that time, using sound laws and regular sound developments. He laid down — with validity that still applies today — the fundamental principles of Hungarian-Finno-Ugric language comparison.

methodological principles, and alongside Hunfalvy, he was the first to categorically assert the Finno-Ugric origin of our language. It is therefore essential to briefly review the methods that Budenz applied at the time and established as exclusively valid in Hungarian linguistics, because according to our own statement, the methodology of Hungarian Finno-Ugric studies is still based on them today, and our linguistics still calls Budenz the founder and most outstanding master of truly scientific, methodical Hungarian-Finno-Ugric language comparison. Budenz's greatest work was the "Hungarian-Oroi Comparative Dictionary" published between 1878-81, in which he derived 996 of our words from Finno-Ugric languages. This number has apparently become a canon in our linguistics: our linguists still use about a thousand Finno-Ugric words to this day.

Hungarian words are counted; despite the fact that international Finno-Ugric studies consider only 450 -- at most 500 -- Hungarian-Finno-Ugric word correspondences acceptable (e.g. Collinder; 1. II. our book, p. 305), and in the meantime, Hungarian linguists themselves have rejected countless Budenz-like etymologies — as we will show below.

[We must also state at the outset that the parallels of Budenz's 996 Hungarian words derived from Finno-Ugric — but also the correspondences of our words that are said to be of Finno-Ugric origin by our modern linguistics — appear completely irregularly in the languages of the individual Finno-Ugric peoples: they appear here and there, from Northern Norway to the Yenisei. This fact can be established beyond all doubt from Budenz's word correspondences, which will be presented below. Moreover, the same situation

480

===== PAGE 475 =====

There are also Finno-Ugric parallels to the grammatical and structural elements of our language: these also appear sometimes in Finnish or Lapp, sometimes in Mordvin or Zürjén, and sometimes in the Voguls or the Ossetians. In each Finno-Ugric language or language group, we occasionally find only a few features that are parallel to Hungarian, since these languages show much closer and more organic connections between each other.

The Hungarian-Finno-Ugric dictionary, grammatical and structural correspondences - which, when viewed as a whole, constitute a truly impressive quantity, and Finno-Ugric studies always list them together in this way to prove the Finno-Ugric origin of our language - were collected by our linguistics from here and there, language by language, plucked out like raisins from the Finno-Ugric cake. However, Finno-Ugric studies consistently ignore this fact of eminent importance, because with such a distribution of linguistic parallels, the only logical conclusion could indisputably be that Hungarian once played a decisive role in the process of areal unification of the languages of the individual Finno-Ugric peoples, and that is how certain elements of the Hungarian language - its words or grammatical features - ended up in this and that Finno-Ugric language or linguistic subgroup. We base this opinion not only on the aforementioned linguistic facts, but also on the very strong support of virtually all known anthropological, demographic, cultural, historical, and archaeological data (cf. pp. 401-407 of this book).

However, Finno-Ugric studies, despite everything, claim that our language derives from the Finno-Ugric languages. Our linguistics tries to support this fundamentally one-sided and unreasonable procedure by assuming, on the one hand, the "forgetting" of the Hungarian parallels that are consistently missing from individual Finno-Ugric languages, and on the other hand, it argues that the multitude of incompatible dictionary, grammatical, and language structure elements "certainly" developed during the separate lives of individual independent Finno-Ugric languages, i.e., in the times after the breakup of the presumed basic language. At the same time, however, it does not even briefly consider the much more obvious possibility that these incompatible linguistic features in individual Finno-Ugric languages may very likely belong to the languages' own ancient basic layers, of which each language has preserved more or less — both during and after the language family-like unification process that once occurred.

In addition, from the aforementioned fundamental work of Budenz, we can also learn how the father of Hungarian Finno-Ugric studies tried to "adapt" the words of our language to the Finno-Ugric languages, often with the help of hair-raising semantic comparisons or breakneck sound change constructions.

481

===== PAGE 476 =====

In support of this opinion, we present below some excerpts from Budenz's etymological dictionary. In compiling the words, we started from the word grouping of Vámbéry's work "The Origin of the Hungarians"; despite the fact that, according to the opinion spread by Finno-Ugric studies, Budenz "actually crushed" Vámbéry, and despite the fact that we ourselves do not accept Vámbéry's basic idea about the Turkish, or more precisely, the Turkish-Finno-Ugric mixed language origin. In any case, from the following it becomes completely obvious what this "crushing" actually consisted of, but also that Vámbéry was right to question Budenz's claim that the Hungarian language is of Finno-Ugric origin, and undoubtedly demonstrated the unfoundedness of the methods of Hungarian language comparison that were exclusively Finno-Ugric in nature. It is another matter whether his theory was also wrong.

- Vámbéry examined 663 words from Budenz's 996 Hungarian-Finno-Ugric lexicon. He found that these words could be divided into four groups.
1. Those that have obvious parallels in both Finno-Ugric and Turkic languages (217 words).
 2. Words that are either not found in the Turkic languages, or if they are, the Finno-Ugric equivalents are closer and more expressive (210 words).
 3. Those that can only be phonetically and conceptually matched with Finno-Ugric words with difficulty, and sometimes even completely incorrectly, while they have much better parallels in Turkish (208 words).
4. Word associations that cannot be justified either in the Finno-Ugric examples listed by Budenz or in the Turkic languages, and which we must consider "...only as products of heated etymological fantasy..." (28 words)

Let us first look at word group number 2, in which the Finno-Ugric relationship is even stronger than the Turkish one, according to Vámbéry (1. Vámbéry: id. m. pp. 559-582). Among these, we find — with only a cursory overview — the following words (first we present Budenz's etymology, then briefly the opinion of TESZ).

1. Face: zür. ord, cser. örtöz , side"; mor, irdes , rib"; Lapp ertek TESZ: the combination "ribs", of the nose and the mouth; possibly already formed in the Finno-Ugric or Ugric period. The Finno-Ugric agreement as a single word is wrong.
2. Forget, forget: Finnish pelasta "to release, to set free".

TESZ: of uncertain origin; Finno-Ugric derivation is incorrect,

3. Fenytv: vog. piment , to command, to forbid"; part. pamiji , 482 advises". TESZ: the root word is disputed: a) the verb fen (this is of Finno-Ugric origin); b) the verb fedd (of unknown origin).

===== PAGE 477 =====

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. Boiling. fi. porise , to swell, swell"; Estonian purise , to bubble, to boil",

TESZ: probably an ancient onomatopoeic word from the Finno-Ugric, possibly Uralic period; identical with the root for of the word family forog, výdavých etc.; regardless of this, a Finno-Ugric correspondence is unlikely, Genny: fi. kina , sticky mucus, foam, saliva".

TESZ: of uncertain origin (perhaps Slavic); Finno-Ugric correspondence is incorrect. Gorbe: la. jorba , round", jorbak , truncated", jorbad , round", TESZ: of South Slavic origin; Finno-Ugric correspondence is incorrect.

Steam: fi. kaasu "fog"; vot. kvaz , air"; la. kasad . Bite: cser. kar , , fog", TESZ: unknown origin; Finno-Ugric correspondence is incorrect.

eat"; zyr. kurtsi , bite".

TESZ: probably Finno-Ugric; cf. zür. kurtal-, vot. kurtidi- , bite".

. Harmat: vot. zor rain".

TESZ: of unknown origin; Finno-Ugric correspondence is incorrect.

Hasad: fi. broken , break, shatter"; la. kaikane , tear, split"; vog. kasl , tear"; cser. kusked , tear".

TESZ: the root word has- is an ancient heritage from the Ugric period; cf. vog. künkash- , to crack, tear, split". Other Finno-Ugric correspondences of the root word are not acceptable.

Mountain: fi. kärke "fullank, the peak of something, peak, roof, spire"; la. garga , flame".

TESZ: probably of Finno-Ugric, possibly Uralic origin, but the correspondences are disputed; cf. fi. kasa Cold, cool, cool: fi. hyy "frost, something prominent, corner". Other Finno-Ugric correspondences are incorrect.

frost", silty "cools, becomes cold".

TESZ: word family of unknown origin; Finno-Ugric correspondence is incorrect. Hord: fi. kuorma "a load to carry"; zür. karnau "a shoulder strap, harness for carrying a load".

TESZ: probably an ancient legacy from the Ugric period; cf. Vog. kart- etc. , pull", The agreement is only acceptable if the Vogul words , t" is a suffix. Its agreement with other Finno-Ugric words is te- , road, furrow"; zür. kirta "furrow";

ves. Horny, groove; cser. korno, road,

öst, xur path".

TESZ: word family of uncertain origin. The verb hornyol is perhaps a derivative; the root word horny may be an ancient legacy from the Finno-Ugric period; cf., cser, korno , furrow, road, strip". However, the agreement has semantic weaknesses and phonological difficulties.

. Sheath: vog. sipel , sheath",

TESZ: of uncertain origin; perhaps a derivative word; its Finno-Ugric correspondences cannot be accepted for phonological reasons. 483

===== PAGE 478 =====

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484

Present: participle jilip, vog. jälpel , new"; participle jidep, jedep, cser. jal "young". TESZ: a derivative of the noun jel, which is of Finno-Ugric origin; the independent Finno-Ugric agreement is incorrect.

Done: la. kisses, kisses, done",

TESZ: of uncertain origin; perhaps a Finno-Ugric heritage; v6. la. kisjuk , ready, willing, willing". The agreement can only be accepted if the Lapp internal [y] can be traced back to Finno-Ugric "c". The fact that the word has an equivalent in only one distantly related language (i.e. Lapp) also makes the comparison very uncertain.

It is derived from: Finno-Ugric kás, anger; gaz, kas joy.

TESZ: a derivative of the verb tesz, which is probably identical to the adjective tesz; its other derivation is incorrect.

Down: class down , low"; vog. lajl , foot"; cser. lüväl "the bottom of something".

TESZ: of uncertain origin; perhaps a Finno-Ugric heritage; cf. cser. 130611 lower part, lower". The comparison has several difficulties. Other Finno-Ugric comparisons are incorrect.

float: zur. leb , emerge, emerge"; vot. lobal "rise", etc. , fly"; vog. áp , Float,

TESZ: a word family of onomatopoeic origin; words with similar phonetic forms and related meanings also exist in other Finno-Ugric languages. Their common origin and the relatedness of Hungarian words to them are possible, but cannot be proven.

Fly: fi. lentä , flying skill"; la. lije , flying".

TESZ: Finno-Ugric origin; cf. zür. ledz Dejlt: fi. , hornet, horse hornet", vot luz , hornet, hornet". Other Finno-Ugric correspondences are incorrect.

loitava "slowly descending".

TESZ: probably derived from the adverb le. The Finno-Ugric correspondence is unacceptable.

Goose: fi. lapse , child"; mord. lëks "allat- 65 bird offspring", TESZ: the animal call originated from words like li, lib; the Finno-Ugric correspondence is incorrect.

Flour: fi. lese "to sift", sift", listime , fine flour"; cser. loZas , sift"; mord. lokstem , sieve",

TESZ: of uncertain origin; perhaps a derivative, and the root word lisz- is probably an ancient legacy from the Finno-Ugric period; cf. fi. lestä , to sift, to sift, to grind finely"; Estonian lese "the husk of barley".

Any other Finno-Ugric agreement cannot be accepted.

Lucsok: fi. losko porridge "; slurp. nocko, mord. nacka "watery, damp",

TESZ; of onomatopoeic origin; its Finno-Ugric correspondences cannot be verified.

===== PAGE 479 =====

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Mászik: fi. mata, mati „to crawl, to climb"; Estonian madu , worm"; la ,mode "hangs, climbs",

TESZ: of unknown origin; Finno-Ugric correspondence is not convincing.

Far: la. meée , removed", mecen long" (temporal determinant).

TESZ: its root word, the far- probably a Finno-Ugric heritage;

cf. fi. metsü ,forest"; Estonian mets , bush, forest, pasture; lost;

outside, out"; la. met"Slid "far away, far out, distant; to the shore closer; close to the forest".

Note: we believe that this derivation does not need any special refutation.

Dirt: fi. musta, Estonian must "black"; la. nueske, black, dirty, filthy.

TESZ: of unknown origin; Finno-Ugric correspondence is incorrect.

Morzsol: fi. murska , something completely broken, crushed"; vog. morce "little of something".

TESZ: of disputed origin: either derived from the verb mar - which onomatopoeic, but probably a legacy of the Uralic period - or of onomatopoeic-onomatopoeic origin.

Note: that is, it is an onomatopoeia either way.

Moves: fi. matka , plays", journey, road", matkaa to make way"; change mod

TESZ: of uncertain origin; presumably the root word moz- is perhaps ancient a word for a voice-over artist from the Uralic period; cf. Samoan for "moleskira" correspondence of the root word müs- "wobbles, moves". The Finno-Ugric moves, works", mozorado' is incorrect.

To crush (to crush, to crush): fi. muserta , breaks into pieces"; zür. la. mutke masti breaks into pieces".

TESZ: of Bavarian-Austrian origin.

Nap: participle novi "light, bright, white".

TESZ: of uncertain origin; perhaps a derivative, and the presumed root word na may be an ancient legacy from the Ugric period; cf. vog. nai , fire, has serious , day". This derivation from the Hungarian word ekszek-nap"; part. ndi phonological and morphological difficulties. Other Finno-Ugric correspondences are incorrect.

Marriage: begins to make love, takes a wife"; Estonian naeze Finnish nai "woman, wife"; Latin naitte "marriage".

TESZ: uncertain origin; perhaps German loanword; Finno-Ugric agreement is unlikely.

Juice: fi. neite, neitehe feces", , moisture, dampness"; vot. ned , mud,

TESZ: unknown origin; Finno-Ugric correspondence cannot be accepted,

Noun:- noun, pronoun, participle, etc. , name",

TESZ: ancient heritage from the Uralic period. The possible Indo-European-Pai, Yukaghir and Chukchi connections of the Uralic word require further investigation. 485

===== PAGE 480 =====

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Note: this word obviously cannot be used in Part: vot. bord to be appropriated for the "Uralic" language family, because it can be found almost throughout Eurasia. , wall"; zür. berdin „-on, -en, -on, -näl, -n&l"; cser. pürdüz ,wall, dividing wall".

TESZ: of Italian origin; cf. porto, port, harbor, harbour"; the Finno-Ugric correspondence, as well as the explanation as a variant form of mart, are also incorrect.

Note: the separation of part from mart - which is of unknown origin - cannot be proven by anything, only by the fact that an Italian word has a similar sound and a meaning that can be squeezed out and twisted by further violating semantics.

Pici, piciny: Estonian pizut , little"; Vot. pici , small"; Fi. pisku , small". TESZ: word family of onomatopoeic origin; Finno-Ugric correspondence is incorrect. Pofa: Fi. pove , fist, bay"; Estonian pou, mord. pov etc. "chest". TESZ: of onomatopoeic-onomatopoeic origin; Ugric derivation is unlikely.

Bucket: change. viedel , to wrap, to wrap"; zyr. bidmal see- fon": fi. vääntä , screw, coil".

TESZ: probably of onomatopoeic-onomatopoeic origin; Finno-Ugric correspondence is unlikely.

of uncertain origin; the , hangs, clings, sticks"; participle ragilt , touches"; vog. raulaxt Ragad: fi. ruohtu TESZ: approaching".

root word rag- is probably a Ugric heritage; cf. participle rönkintü "clings, sticks". No other Finno-Ugric agreement can be accepted.

Redves: from radax, loose, fragile; from erdä, "the pith, heartwood of a tree".

TESZ: probably a Hungarian development: from the word revés, which has an unknown origin.

Rest: Vog. rac , delays, lingers."

TESZ: unknown origin; Finno-Ugric correspondence is incorrect.

This is enough for information; Budenz can discard at "method" is already clearly recognizable. So, from the 210 words of this group, we can already make a running overview, as it were, "on the fly" least 42 words (20%), although these are the words that Vámbéry did not even bother with. Today, the derivation of these words is summarized as follows;

Uralic, Finno-Ugric, Ugric; a) like Budenz
probably Finno-Ugric disputed, but possibly Finno-Ugric
486
b) otherwise
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uncertain; a) maybe Finno-Ugric b) 7
not Finno-Ugric 1 5 onomatopoeia, sound painting, animal calling 9 Slavic 1 Italian 1 German
unknown 8
That is, Budenz e 42 From the "most certain" etymology — we repeat, not even Vámbéry objected to these! — one word has proven to be correct today (not quite 2.4%) — but we have seen that even then the "Finno-Ugric origin" can only be established with very, very great restrictions, because the name — we can say with all due respect — is Eurasian "wanderer", and in no way can it be attributed to the alleged Uralic language family.

Then — to put it very clearly — we can state that Budenz's etymologies cannot be considered a systematic, scientific language comparison, but merely a mechanically attempted — but fundamentally flawed — application of Grimm's Indo-Germanic "sound laws" learned in Göttingen to the comparison of Finno-Ugric languages and Hungarian, in which, however, — following from the essence of the regular sound development method — Budenz took semantic connections into account in a very superficial, secondary way. Moreover, we will show below that even the basic principle of the method, the "regular" sound changes, became quite "irregular" in Budenz's hands.

We cannot now deal in detail with the etymologies of the TESZ that we have previously learned, and therefore, in addition to the above few comments, we only draw attention to the fact that out of the 42 words presented, only four are definitely Finno-Ugric (9.5 90), in 13 cases (31%) the TESZ cannot make a clear decision, while the remaining 25 words (59.5%) are definitely not of Finno-Ugric origin.

Can we hope that this uncertainty is perhaps a sign that the editorial board wanted to leave the door open for possible future etymological investigations leading to more thorough, concrete results? If so, this would certainly represent a very significant advance.

Let us now take a look at the 208 words of Vámbéry's third word category, which according to him have much better parallels in the Turkic languages than in the Finno-Ugric ones. However, this time we will not present excerpts, but rather, as a representative cross-section, we will highlight the first 31 words from this group in alphabetical order, 15% of the 208 words (1. Vámbéry: id, m. pp. 582-592). First, we will present Budenz's Finno-Ugric concordances, then Vámbéry's (V.) Turkic parallels, and finally the modern derivations.

1. Ag: fi. onke; la. vuogg, ogg; zür. vugir; cser. angir , fishing hook"; V.: csag. agaf , branch, stick"; oszm. 0206 , tree"; TESZ: of disputed origin; a) Ugric; cf. vog. tag, tar "äg" etc.; 487
===== PAGE 482 =====
488
b) Finno-Ugric; cf. fi. hanka "campo". Other Finno-Ugric correspondences are incorrect. Agg: fi. ankara "healthy, strong, powerful, influential"; tser. ungor "zäpfog"; ost. ogor , tall".
V.: tör.-tat. ag, ak TESZ: , white, gray, respectable"; aga, aka , elder, superior, lord".
Finno-Ugric origin cf. cser. song) , old". The other Finno-Ugric correspondence of the word is incorrect.
Brain: participle ofit, verb arit V.: . horn".
csag oj , thought, mind".
TESZ: of uncertain origin; perhaps an ancient legacy from the Finno-Ugric period; cf. mord. uj , marrow"; fi. äivo , temple, brain". Other Finno-Ugric derivations are incorrect. 4 Bed: vog. alax , nest, camp, bed"; zür. vol' , voles , bed"; fi. vuotehe , lying place, bed".
V.: tör.-tat. jat , lies"; jatak , bed, couch, nest".
TESZ: of Finno-Ugric origin; cf. vog. alät Aj (valley, , bed"; zür vol , felt, reindeer-deer skin (bed), voles „underlay", etc.
opening, cut), door: fi. aukaa , opening"; mord. anksima , hole"; s. omasä , door"; vog. ojt , to take out (a slough); ots. wii s mouth, mouth"; s. vom , mouth".
V.: csag. oj , valley, depression"; tör.-tat. aja , open hand",
TESZ: the word aj is an ancient heritage from the Uralic period — just like in Budenz; the root word for door is disputed: a) Ugric-Ugric period; cf. vog. ayk , take off (clothes) "; fi. avata , open, expand, expand" etc. b) Uralic period, the root word is aj (like Budenz).
Want: vog. angurmät , glance"; partitive ankirma , wish, look there".
V.: new bakar want"; jak. bagar , want, desire".
TESZ: of uncertain origin; perhaps an ancient legacy from the Ugric period — like Budenz.
Note: the combination of wants and looks is a semantic impossibility.
Bless: vog. jolent , ask, persuade"; jolt , create, prepare"; jalt "protect"; zyr. jol , scold"; cser. ult "ker, beg, pray".
V.: csag. alga, alka , bless, magnify, praise"; alkis , blessing, praise"; olda, yyyj , bless"; osm. and , oath, blessing".
TESZ: the root word äl- is probably an ancient legacy from the Finno-Ugric period; cf. ost. ällita "(el)Jätköz"; ts. ultém prays" etc. Note: the vog. jolt , to prepare" is obviously related to the Hungarian szágyát, while the jalt , to accuse" and the zür. jol' , to scold" are related to the verb to slander. Bargain: vog. alt , to confirm"; ost. olt "to forge",
V.: csag. alku , purchase"; alku-bergi , trading, buying and selling". TESZ: derivative of the verb expressing adaptation; the same
===== PAGE 483 =====
the root has the opportunity, it sets in, it creates in words too, The alk,- or, perhaps the al- root is of uncertain origin, perhaps an ancient legacy from the Ugric era; cf. vog. öltilaxi , to grow together"; alti unites, connects, adds, increases" etc.
Note: the word alkonyodik obviously belongs to the family of al, lower, compounded with the verb konyul; the Vogul älti, "unites" is the equivalent of the Hungarian olt (plant).
Stand: vog. jomilt , to tread, step"; jomes ,step",
V.: osm. ol 10. "to be, to exist, to be somewhere".
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TESZ: Finno-Ugric origin; cf. zür. sulavni , to stand, to , stands, gets involved in something";
vot. sil- stop, to calm down, to become involved in something, to be valid". The
The word's other Finno-Ugric derivation is incorrect.
Father: Vog. aberäs "father's grandfather".
V.: csag. aba , paternal grandfather".
TESZ: disputed origin; "ap- root word or Ugric heritage,
or Hungarian children's language word. Similar sound and meaning
words are found worldwide, so the Ugric correspondence is uncertain.
Small, small: participle nambir , waste, rubbish"; participle nampri , small".
V.: csag. opurak , small, small"; opra "aprit".
TESZ: uncertain, perhaps of Old Turkic origin; cf. Ujg. opruk , feldara-bolt"; Csag.
opra "aprit" etc. Derivation of Ugric or Finno-Ugric
unacceptable.
Ar, stream: vog. järve, la. jaure, mord. erke , stream", tö".
V.: osm: art , grows, multiplies"; csag. art , lake"; ost. lar, iar
vog. für, tor let", The Finno-Ugric correspondenceärterü-TESZ: Ugric origin; cf.
of the word is incorrect.
Ratio (direction): öst ur "line, stroke".
V. tör.-tat. oran , measure, proportion, direction"; kirg. oral , proportion, mode,
manner"; csag. lord, oram TESZ: , street, row of houses".
the word family is probably "ar- "ir- the root word is uncertain ere-0616; perhaps an
ancient legacy from the Ugric period; cf. vog. jár, jér , place";
"Öst, jr , edge, edge of something", the Turkish derivation is unacceptable.

Note: phonologically and semantically flawless Turkish
Sterile denial of correspondences is far from proof.

Arat: Budenz classifies it as a verb of the Turkish origin, and the Ugric togr etc., "cut, break" so he negotiates.

V.: OSZM. ora , arat",

TESZ: of disputed origin; a) a separate variant of the Finno-Ugric written verb; cf. part: lorta, iorta , shear, cut (hair) "; zür. širni , shear, cut; vot. Sir , , cut, cut off"; b) of Old Turkic origin; cf. csag or- , cut"; acc. ord" to cut, to cut with a sickle"; tat. ur» reap" etc. The first explanation 489

===== PAGE 484 =====

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more likely. The Finno-Ugric agreement independent of irt is unacceptable. Note: the Turkic parallels are again much more obvious, but the Finno-Ugric origin is still more likely according to TESZ. yék: vog. tarom , sky, air, weather"; ost. torim "sky, air",

V.: tör-tat. aran ,shed, stable, yard"; eren ost. orim zür. orní to offer oneself for protection". TESZ: known origin; Finno-Ugric | its agreement is not acceptable. Árt: vog. ur , to become thin"; , harm, damage" TESZ: its root word is an , skinny"; zur. or "to suffer". Vi: wg. arta , harm"; artak ancient heritage from the Finno-Ugric era; cf. vog. dít- ,to be tired"; part. ýýýýý , to break"; to break". The question of possible Turkic connections of the Finno-Ugric root word is unclear. The Turkic derivation is incorrect. Note: here not only the Finno-Ugric words are completely

We must point out not only the different meanings of the words, but also that although the question of possible Turkic connections is unclear even according to the TESZ, they still flatly claim that the Turkic derivation is wrong. This procedure is called objective methodical Finno-Ugric studies; and since the time of Vámbéry, the word arta = "to spoil, to ruin, to destroy" has become known in Old Turkic as well (cf. Av Gabain: Altürkische a yasit is a tone-paint, Grammatik, glossary). As, ásit: vog. uosint, vot. vusil, cser. ust , to ass"; mord. "in verder", i.e. lék". V.: tör-tat. es , to poke, scrape, scratch"; esgek , shovel; ac "to open". TESZ: a ás is of uncertain origin; which may have developed already in the Finno-Ugric period. ankse

There is no connection between the two words. Aszó: fi. laakso , valley, depression, hollow". V.: kirg. asak , meadow, , below, below, as a cake below TESZ: its root word is of disputed origin of the verb from a possible word csag or- is Ugric szelt

tat, ast , lord"; vot. öksej , lord, prince"; V.: oksun, aksin in Turkic , , below"; astik "bottom, under"; as, 64 "below, under, beneath", TESZ: of unknown origin; Ugric and Turkic derivation is incorrect, Assón; zür. ökst sok: ost. oms, ums , üll, leüs", V.: tör-, whiteness, white color"; cf. white person" (female). TESZ: of Alanic origin; Finno-Ugric concordance is incorrect, Atkoz, curse; according to Budenz it comes from the same root as the word áld

===== PAGE 485 =====

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(see above); both contain the concept of saying, command, "strong saying".

V. Turkish aítuk , saying, speech"; Kara-Aituk "scolding, cursing word", literally means "black speech",

TESZ: derived from the verb áld, which is probably a Finno-Ugric heritage. The explanation for the opposite meaning is "... that the "taboo" position of the individual offered to the spirit was unfavorable, "So whoever was offered as a sacrifice was actually cursed."

Note: everything can be "proved" in this way, even also why black is actually white.

As.: Budenz with the Finno-Ugric words tuo, tu, to "that, that, that there" matches, from which word-initial deletion, then , word-final with expansion: the Hungarian language would have developed.

V.; passion, os, os-bu , az"; is-bu , ez"; I give, I give"; csag. oş-bu

it", iş-bu , this"; -ondan TESZ: of from there?" (the -bu is just an accent).

Uralic origin; cf. zür. a-: ata ,here here", all ,here there", asi ime (there)"; vot. o-: oti is "there" etc. Other Finno-Ugric coordination wrong.

vis , sick", vis , sick", vika "body defect, disease"; vot. vis III: zür

V.: csag. бүтүк , running out, coming to an end".

TESZ: of unknown origin; Finno-Ugric correspondence is incorrect.

pay? , pay, give"; fi. pay, Estonian pay, la. pay Bizik: mord. will pay.

ujg. бүт believes, believes, trusts, has confidence"; alt.püt , V.;

TESZ: of uncertain origin; perhaps an ancient legacy from the Finno-Ugric period;

cf. vot. baz- , hopes, trusts, has confidence, dares-

This word votyak is apparently related to the Tatar baz- "mer, elhatä-, "ý

"rozza ságnat" (to break himself), which has no equivalent in the Altaic languages: thus it cannot be decided which is the borrowing of the other. Other

The Finno-Ugric alignment is incorrect.

Bog: cz. bugil , protruding part, hump, bulge"; Estonian pung , protruding part, round"; fi. puka "ball"; s. pongo, mord, panga , mushroom";

V.pF tör-tat. bogun "bog, knob"; csag. bag "tie, rope"; kaz. bogol ,boglya".

TESZ; a phonetic word; or a Finno-Ugric heritage (like Budenz).

Or the Hungarian language originated in its own lifetime. Similar

Onomatopoeic words are also found in Altaic and Indo-Germanic languages.

Berry: fi. marja, la. muorje , berry"; tan. mör , strawberry"; "ca";

mord, mar , apple"; vot. muli , berry",

V.: kirg. mug, bug, berry,

TESZ; probably a word of onomatopoeia; or an ancient heritage of Finno-Ugric 491

===== PAGE 486 =====

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from the era; cf. zür. pul, fi. puola , "red cranberry", etc.; or it arose in the separate life of the Hungarian language. Its older association with Finno-Ugric words beginning with "m" is untenable.

Wander: Estonian molku , move"; ts. mu'ukt , shake"; vog. mol'eml "hurry",

V.: 6588. bulga "to stir up, to stir up, to häborgat"; Kaz. bolga , to shake, to stir up, to turn".

TESZ: probably of Hungarian origin; the comparison of Ugric and Finno-Ugric is incorrect.

Burit (burul): zür. pör , falls, 61661". vot. pogral; vog. pürmät ,falls",

V.: tör-tat. bor, bór , to cover, to put on, to obscure, to obscure", TESZ: probably a word family of Old Turkic origin; cf. osm. bur-, buru-, bür-, bürü "to cover, to cover, to obscure" etc.

The Finno-Ugric alignment is incorrect.

Borzad (borzas): fi. puffy, puffy, swollen, stiff state",

V.: passion. borsan, borsanda, borzad, fölretten".

TESZ: word family of onomatopoeic origin; Finno-Ugric correspondence is incorrect. Revenge: la. puose, puosak, malicious, grumpy, angry, biting, stinging".
V.: uig. bosi , anger, hatred"; bos , gets angry"; csag. bos, bosuk "gets angry"; kaz. bosan , gets angry".

TESZ: probably of Old Turkic origin.
Wise: fi. miele , , reason, intellect"; mord. meġ , reason"; la. miälä , reason, intelligence".
V.: tor-ġat. bilizi ~~wadon~~ ~~bilis~~ knowledge".

TESZ: of Old Turkish origin; cf. Ottoman büyü, "sorcerer, witch", etc.
According to the etymologies accepted today:
Uralic, Finno-Ugric, Ugric; a) like Budenz probably Finno-Ugric; like Budenz disputed, but possibly Finno-Ugric uncertain; a) maybe Finno-Ugric
onomatopoeia, onomatopoeia
Turkish Alan
unknown
b) otherwise
b) not Finno-Ugric
We WRN the 4th »

In this case, Budenz's methodical approach is even more evident in the previous word group, because now we have compared the words in order, without selection, with their modern derivations, and the result is: two out of 31 words
492
===== PAGE 487 =====

correct (6.4%) and two probably correct (also 6.4%) matches are contrasted by 27 definitely incorrect (87.1%) etymologies.
However, upon closer inspection of these 31 words, we can also observe how later Finno-Ugric studies tried at all costs to refute Vámbéry, or at least to save the handle of the lost axe - precisely from this group of words that Vámbéry considered to be of Turkic origin.
First: four words — despite the obviously spoof semantic connections and the great differences in sound — are still of the same Finno-Ugric origin (two certainly, two probably) as in Budenz.

Secondly: for seven words, Finno-Ugric parallels were found that were more obvious than Budenz's hair-raising etymologies, but for four of these words, the correspondence is very questionable, especially in light of the much closer Turkic parallels.
Thirdly: seven words became uncertain 676061006 — also by ignoring or outright denying the clear Turkish equivalents, and at the same time five of these
perhaps of Finno-Ugric origin".
Fourth: two of the four onomatopoeic words are probably of Finno-Ugric heritage."
Fifth: even out of the three recognized Turkic parallels, two are only "probably" of Turkic origin, while obvious Turkic correspondences such as arat become controversial, where a Finno-Ugric origin is more likely.

In any case, it is thought-provoking that even despite such tricks, they were unable to justify more than four of Budenz's etymologies, and even then only by throwing the semantics on the trash heap, as is the custom of phonological linguistics (cf. e.g. the derivation of áid and átkoz from the same root). Moreover, at the same time, the much closer semantic and phonetic correspondences of the parallel Turkish words are often ignored without any justification, while at the same time, Finno-Ugric words with very different phonetics are successively made the ancestors of the corresponding Hungarian words (cf. e.g. ág-tag; agg-songA; all-sulavni), and even in the case of onomatopoeic words they speak of "Finno-Ugric heritage" (cf. e.g. bogyó-pol), undoubtedly in order to increase the number of words of "Finno-Ugric origin" in this way.

In the end, we must indisputably agree with Vámbéry that the Finno-Ugric-Hungarian etymologies of Budenz and Hunfalvy at that time did not in any way prove the Finno-Ugric origin of our words. Moreover, as we have seen from the above, the situation is not much better in this area today. This is also supported by the fact that in the TESZ we find words that are claimed to be of Finno-Ugric origin, the "sub-linguistic relations of which require further investigation." I mean, in the Turkish languages these words have at least the same [6 + if not closer ~ párfu-493

===== PAGE 488 =====
They have the same sounds as the Finno-Ugric ones, but if the common origin were not openly acknowledged, the number of Hungarian words that can be declared to be of exclusively Finno-Ugric origin would shrink to even fewer.

Finally — to better illuminate Budenz's real linguistic capacity — we will also present a bunch of word comparisons from the 663 words examined by Vámbéry in the "Magyar-Ugric Comparative Dictionary", which do not differ in any way from the previously familiar Pictet-like etymologies (Book 1, I, p. 57), or even from the word explanation methods of "István Horvát". These are not often mentioned in the incessant mention of Budenz's timeless merits... 1. ankle-tree stump; 2. great-great loneliness; 3. hide-climb-climb; 4. cuck-bedug; 5. reprimand-forg; 6. navel-tongue (reasoning: both, slender body"); 7. neck-move, run; 8. shell-enter; 9. csik-hit; 10. dĕrŭ-known; 11. prize-wedding gift; 12. fast-strength; 13. lime-bark; 14. profit-growth; 15. small-chip; 16. age-tall; 17. self-body; 18. big-many; 19. kíván-"Notices what is missing"; 20. dare-say, urges out loud (reasoning: in dare we should see the "self-awareness of one's own strength, the firm will", which finds expression in saying, commanding"); 21. lust-drive, drive; 22. forbid-bezár; 23. wind (Rand)-breast; 24. falls-runs away; 25. fine-poor; 26. wing-leader; 27. commands-greets; 28. egg-puppy; 29. ýýýýý-ýýýýý, heat; 30. village-outside; 31. grass-, snow-free place"; 32. stomach-round and eggs; 33. flock-meat; 34. across (does not want)-

supports, stretches; 35. sin-gives, sells; 36. proud-man, fellow, ram; 37. 181-161, page; 38. hell-dog; 39. strange-j6; 40. saw-beam, plank, log; 41. care, think-tool, tool; 42. stiff, stiff-narrow, tight, crowded; 43. shows-finds; 44. mf, works-feels, touches, grasps; 45. morning-fog, darkness; 46. drunk-heat, steam; 47. bad-c, to be rejected, useless thing"; 48. wild-forest and mountain; 49. lung-anger and spleen (Miltz); 50. hill-bump and island; 51. to fail-to be born; 52. forest-many, multitude, mass; 53. wound-soft, delicate; 54. find-come, come; 55. lie-sit; 56. birch(tree)-branch, 4g; 57. dirt-, thick bark"; 58. szín-látsik; 59. hip (Hüfte)-neck and withers; 60. man--,child of the air"; etc. etc.

Here, again, more than 9% of the 663 words of Budenz's Finno-Ugric-Hungarian dictionary, which, together with the 68 (41 + 27) etymologies that are certainly wrong even according to the TESZ presented above, reveals the great scholar's 128 grossly incorrect word matchings even in this cursory review (19.3% of the 663 words!). However, if we extend the lesson learned from the first 31 words of Vámbéry's 3rd word group (2 definitely correct, the same number probably correct etymologies, i.e., taking the average value, 3 comparisons that can be said to be correct) to the 208 words of the group, then in the end we can only count on about 20 flawless etymologies in this group-494

===== PAGE 489 =====
we, i.e. about 188 word matchings can also be considered incorrect with a high probability. This already means a total of 289 incorrect etymologies (about 43.6%) — and we haven't even examined Vámbéry's word group number 1!

In light of all this, it will be very instructive to compare the above facts with the "generosity" with which the current representatives of our linguistics present the course and results of the scientific debates they call the "Ugric-Turkish War" to the interested Hungarian reading public — but also to non-linguistic experts.
János Pusztaý, for example, emphatically emphasizes that Vámbéry did not take into account the principle of regular sound correspondences, because, for example, in his study entitled "Hungarian and Turkish-Tatar word correspondences" (1869), Budenz's thorough criticism, carried out with scientific method, revealed such phonetic correspondences: bs; hb; fk; fs; pd; bt; tp; (I. Pusztaý: After the "Ugric-Turkish War", pp. 95-96.)

However, our author forgets to mention two very important circumstances here. One is that in Vámbéry's main work on this subject, "The Origin of the Hungarians"(1882), we hardly find such sound correspondences, even in the index, meaning that Vámbéry accepted the correct criticism. But the fact that Pusztaý has to refer to a work by Vámbéry from 1869, even though the "Ugric-Turkish War" actually only began in 1882, precisely with the publication of "The Origin of the Hungarians". The other thing that Pusztaý also "forgets" is that in Budenz's etymological dictionary, which he says criticizes Vámbéry with "scientific method", the "regular" sound changes are described as follows - based solely on the word examples presented above: Hungarian b - Finno-Ugric b, v, m, psz, fksz;

0 d,t,zs; ýý ýý t, szt: "of g, k, j, kv, ng, nk: "gy" j, l, r, ng, nt, ny, rg, rk, rj; "oh ~~h~~ ~~u~~ ~~g~~ ~~z~~ SZ; "agn" 9 p,v,b; , SZ " n, j; ap lt SZ, S, ZS, cs, t, d, ksz, msz; a" Vv " 2 t, d, ksz, p, m; pi,

seems that Budenz was certainly not subject to the fundamental rules of linguistics, the absence of which Vámbérynál so "methodically" criticized. He was free to declare cricket-bug as a "regular" sound change.

495
===== PAGE 490 =====
Pusztaý then explains that at that time linguistics was not yet at such a high level as it is today, and therefore (emphasis added) we cannot accept it It happens, for example, that some arguments of the Finno-Ugric with our current knowledge. (Ibid., p. 98) — Compare this "some" error of the Finno-Ugricists with Budenz's above-presented, at least 43.6% incorrect etymology!

Then he writes: "Pál Hunfalvy... points out Vámbéry's methodological error, that he compared a Hungarian language with the Turko-Tatar language." (Pusztaý: id. m.103. p.)
Pusztaý here again "forgot" to mention the indisputable fact that Finno-Ugric studies did exactly the same thing — and still do today — with the entire Finno-Ugric language. (1. pp. 480-481.) Yet Vámbéry had already pointed this out emphatically. Material was collected from all over the mentioned language area, that is: for Finno-Ugric linguists, due to the absolute lack of unity of the Finno-Ugric languages, the evidence one form, analogies were found among the Ossetians, Voguls and Mordvins, for another among the Finns and the Zürks, and for a third again among the Lapps." (Vámbéry: id. m. p. 256.)

Well, the Finno-Ugric languages are indeed very far from each other, we cannot speak of mutual intelligibility between them even in the narrower sense of the smaller language groups, and indeed, very often in the case of the dialects of individual languages (cf. e.g. the 6-7 Ossetians, with their mutually incomprehensible dialects; 1. Antal-Csongor-Fodor: The Languages of the World, p. 120). The Turkic languages, on the other hand — with the exception of one Chuvash — are still barely different from each other, so to speak, every Turk can make himself understood by the others. j

So who made the much bigger methodological mistake in this area: Vámbéry or Finno-Ugric studies? And especially: what right did Hunfalvy have to speak about a methodological mistake in this very question — in relation to others? So once again, the typical Finno-Ugric illogicality appears, which is incomprehensible to sober-minded adults: they take it for granted that everything is allowed to them, but if someone else wants to apply their methods, they immediately call it unscientific. This kind of thing is only acceptable in kindergarten, among unruly children, but hardly in scientific research.

József Thüry directly claimed — Pusztay notes —, "... that József Budenz's Finno-Ugric kinship theory was born a priori, and in the Hungarian-Ugric Comparative Dictionary he tried to collect evidence for this theory. To prove how ineffectively, in addition to his many mistakes — he also picked out some truly incorrect etymologies: bŕzni..., beteg..., idŕd." (id, m. p. 106)

It is important to note that all signs point to Budenz's theory of the Finno-Ugric origin of the Hungarian language (not 496
===== PAGE 491 =====

about the relationship, as Pusztay writes with misleading intent) was indeed conceived a priori, because it can be proven — then and now it was only the fact of the relationship, but not the Finno-Ugric origin of our language. However, Pusztay again sees it better to keep this very important circumstance silent. He does not even mention that the resistance of truly serious researchers was provoked by this unprovable, and to a large extent improbable a priori theory of Finno-Ugric derivation, and not by the claim of the Finno-Ugric relationship of our language. At the same time, our author, as he did before, in the above quotation again tries to make the matter appear as if only "some" of Budenz's etymologies were incorrect. He adds to this below by noting: "Budenz showed how comparative linguistics should be practiced." (ibid., p. 107)

We emphasize again: we saw above that even a partial overview can clearly establish that at least 43.6% of Budenz's etymologies were fundamentally erroneous, and in fact were often downright etymological nonsense (by the way, Pusztay provides another addition to these nonsense; time-belt; 1st ed., p. 106).

As a result, we can rightly state — in sharp contrast to Pusztay — that Budenz's etymological dictionary showed exactly how not to make linguistic comparisons. That is why we chose János Arany's epigram on Budenz as the motto of this volume:

"Dissect it, linguist! So what's the harm in losing the victim? Is this your fault?... One thing is certain: not your mother!"
From the above, it is undoubtedly clear that János Arany was not wrong in his assessment of Budenz's work.
Our aim is by no means to idealize Vámbéry, and even less do we want to break a spear in support of his theory. He was also wrong many times and seriously, and his thesis that our language originated from a Turkic-Finno-Ugric mixture was nothing more than a variant of the Indo-Germanocentric linguistic and prehistorical approach of the time. We have only shown — we hope, convincingly way — that the reasoning and scientific methods of the Finno-Ugric camp were not any more well-founded than Vámbéry's science, and — despite all the hype in this regard — it is in no way possible to say that Budenz and his colleagues "overthrew" their opponents, much less to claim that they had proven the Finno-Ugric origin of the Hungarian language with scientific methodology. The denial of the Finno-Ugric origin was Vámbéry's cardinal thesis, and he was not wrong in this. His mistake began precisely when, adopting the Finno-Ugric method, they themselves refuted Vámbéry, and did not "re-evaluate" their success as if this had also proven the Finno-Ugric origin of the Hungarian language, wanted to "derive" our language, only not from Finno-Ugric, but from Turkic. In this area — but only here — the Finno-Ugrics truly

497
===== PAGE 492 =====

We see the most painful consequence of the "Ugric-Turkic war" in the fact that Finno-Ugric studies has become even more self-absorbed than before, fleeing into an unapproachable sterile denial in the face of all reasonable objections, and even no longer listening to arguments that challenged the other-directional connections of our language. While, for example, the Finno-Ugric trend previously acknowledged the fact of the countless Uralic-Altaic linguistic parallels, which clearly testify to the former close connections of these language families, Finno-Ugric linguistics has increasingly withdrawn from examining the connections between the Uralic and Altaic languages, and has barely taken Turkic correspondences into account in its etymological and linguistic development research. All signs point to the fact that during the discussions of the "Ugric-Turkic War", Finno-Ugric scholars themselves noticed the multitude of organic connections that bind the two language families together, but also that we can hardly speak of a separate Uralic or Finno-Ugric language family, ancestral people or ancestral language, and that this becomes all the clearer the more deeply they deal with the Turkic-Finno-Ugric linguistic parallels.

Linguistic statistical considerations.
Let us now turn to contemporary Finno-Ugric linguistic and prehistory research issues. In the introduction, we present some tabular compilations of the correspondences between the Uralic-Finno-Ugric languages, based on Gyula László's book "The Earliest Sections of Our Philology" (1st ed., p. 36-39).

1. Samoyedic equivalents in other Uralic languages:

Lapp	211 Baltic Finn	248 Volga-Finnish a) Cheremis (Mari) 161 b) Mordvin 217 b) Vogul (Mansi)	143 Permian	a)
Zürjén (Komi) 177 b) Votyak (Udmurt) equivalents in other Uralic languages: Samoyed a) Zürjén 67 b) Votyak Permian 38	140 heart-shaped	a) Ostjåk (Khanty) 142 Baltic-Finnish	178 Hungarian 109 Volga-Finnish a) Cheremis 81 b) Vogul	b) Mordvinian 7 75
	62 Ob-Ugric	a) divide	77	Hungarian

498
===== PAGE 493 =====

3. Sub-language correspondences in the Uralic languages (the first number is the older, the second indicates the newer layer):

Samoyed (by dialects) a) Jurach 43+12 Volga-Finnish 3741 b) Tavgi 2830 c) Yenisei 0 d) Selkup 0 e) Kamassz 31340 Lapp 36+8 Permian a)	399 Baltic Finn
a) Cheremis 34+7 b) Votjak 31+8 Ob-Ugric 24+10 4. Indo-Germanic 32+9 b) Mordvin the Uralic languages: Samoyed 35+9 b) Vogul 3? Lapp 50 Volga-Finnish a) Cheremis 19 b) Mordvinian 37 Permian a) Zürjén 12 b)	a)
34+11 Hungarian	Vogul 18 Hungarian 21
16 Baltic fin	
31 b) vote	26 heart-shaped
	a) divide

Ad 1. From the distribution of Samoyedic matches, it is obvious that among the Finno-Ugric peoples living further north (Lapps, Baltic-Finnish, Ossetians) we find much more matches (between 248-211) than among the southern ones (Ceremis, Zürjén, Vogul: between 178-161), while among those living furthest south (Mordvin, Votjaks) the number of matches decreases significantly again (between 143-140), while in Hungarian there are even fewer than this (124).
To summarize for illustrative purposes (taking most matches as 100%):
northern band 248-211 100-85.1 % hand-held sav 178-161..... 71.8-64.9 % southern band Gyula László concludes 6-56.4, 57 % Hungarian 124 eee 50 %
from this that in ancient times the geographical location and spatial relationship of the individual Uralic peoples must have been similar to today, only not as widely dispersed as we find them today.

499
===== PAGE 494 =====
Ad 2. The distribution of vocabulary shared with the paper shows the same picture: it is largest in Samoyed (142) and Baltic-Finnish (109), roughly even among Volga and Ob-Ugric (81-75), somewhat smaller among Permian (67-62), and — compared to the others — virtually insignificant among Hungarians (38).
For example: for the Samoyeds 142 100%, for the Lapps 109 76.8%, for the more distant 38 yy. 26.7 60 81-62 57-43 .7% for Hungarians

This distribution largely confirms the above statement by Gyula László.
the correctness of his conclusion: the linguistic material shared with the sheet, as well as the number of Samoyed matches, decreases regularly in direct proportion to geographical distance.
Ad 3. The older Altaic word layer is remarkably evenly distributed in all Finno-Ugric languages (43-31), except for Hungarian, where the number of ancient correspondences is quite low (24): significantly fewer even than the Samoyedic-Altaic correspondences (37-28).
For example: for the Finno-Ugric peoples 43-31 100-72% for the Samoyeds 37-26 86-65.1% for the Hungarians, the uniformity in any case suggests DA veecscsecssee 55.8 % This is the that — as László Gyula states — the Uralic peoples may have lived very close to each other." (ibid., p. 38) He At the time of this early influence of the Altaic languages, there were still territorial also points out that according to these figures, we should perhaps place the ancient settlement areas of the Altaians further west than further east.

In this regard, we would like to remind you that many renowned foreign researchers place the ancestral homeland of the Altai peoples in Western Turkmenistan, roughly around the Aral Sea; e.g. O. Franke, Liu Mau-Tsai. W. Eberhard etc. (I. Book 11, pp. 240-241)
However, Gyula László does not elaborate on the remarkably low number of ancient Altaic word cognates in the Hungarian language, which amount to only slightly more than half of the ancient Altaic word layer found in other Finno-Ugric languages. Yet this is in stark contrast to the clear conclusion that can be drawn from the Samoan and Lapp cognates described above.
with lessons, because according to them we should expect the strongest Altaic linguistic loan to be found precisely in the southernmost pre-Hungarians.
This contradiction obviously indicates that something is wrong with our so-called Old Turkic loanwords – even if we look at the issue from the point of view of Finno-Ugric studies and loanword vol-500

===== PAGE 495 =====
We accept them conditionally for the time being, and it seems that at least some of them do not come from the period of the "separate development of the Hungarian language" — as our linguists claim — but from the so-called Ural-Finno-Ugric times, and these words are not found in the other Finno-Ugric languages because they only reached the southernmost Proto-Hungarians at that time. Otherwise, the glaring contradiction that even in the Samogitian dialects there are much more common ancient Altaic elements than in the Proto-Hungarians located in the southernmost zone of the supposed Ural-Finno-Ugric settlement areas can hardly be resolved.

We would not think that we would be greatly mistaken if we assessed the surprisingly low number of Hungarian-Old Altaic word cognates as a by-product of the infamous "Ugric-Turkish" war, when Budenz's main aim was to – in order to refute Vámbéry – declare as many ancient Altaic-Hungarian word cognates as possible as later loanwords (cf. the opinion of E. Moór, I. above, pp. 473-474, and what was said in Book II, pp. 234-235 and in this book, pp. 492-493). In any case, these questions are of such eminent importance that they absolutely require further in-depth, objective investigations.

n. We will return to all this when discussing our Turkic loanwords. Ad 4. The Indo-Germanic-Uralic word cognates only confirm the outlines of the prehistoric and linguistic-historical picture already concretely outlined above: the most cognates are found in the languages of the Finno-Ugric peoples who still live further west and south today — the Balts and the Mordvins — (50-37), and their number decreases continuously towards the north and east; there are 16 cognates in the Lapps, 18-12 in the Ob-Ugrics, and only three in Samoyed, but even these are questionable.

Illustratively: western groups (Balts) eastern groups (Lapps, SO nennen 100 % intermediate group (Volga, Perm, Hungarians, Voguls) 37-1874-36 % northerners, Ostyaks) 16-12..... 32-24 90 Samoyeds This distribution also indicates — notes Gyula László — that at the time of the ancient Indo-Germanic language influence, which in all likelihood reached the Finno-Ugric peoples from the southwest, the ancestors of the Ostyaks and Lapps already lived very far from the others, and this influence had barely reached the Samoyeds (id. m. p. 37). From the distribution of the ancient word cognates presented, Gyula László draws his conclusion as follows: "... from what has been said, we can conclude that in the Uralic period we cannot imagine a complete linguistic and territorial unity, but rather that the Finno-Ugric clans and ethnic groups lived in a fairly significant territorial division - roughly corresponding to the later separation."

===== PAGE 496 =====
"It seems that there was a completely unified Uralic basic language - and in theory, even then, we can speak of the most elementary expressions. In the times still accessible to us, we may have been talking about sister languages of nationalities that were close to each other, but which already had, even if only to a lesser extent, separate vocabulary. Among the linguistic concepts, the language chain theory is the closest to this idea." (id. m. p. 38)

We would only like to add to the above that, in the light of the new insights of Indo-Germanic studies, we cannot in any case place the age of ancient "Indo-Germanic" language influences earlier than the turn of the 3rd/2nd millennium BC, and on the other hand, we can at least just as reasonably interpret the individual vocabulary of the "nationality sister languages" as the typical, genuine, most ancient basic language of the individual Pre-Uralic (Pre-Finno-Ugric) dialects, on which the cultural language of the southern-originating Volga-Ural colonists was dominantly superimposed in the 2nd millennium BC, thereby causing linguistic equalization between them (more on this later).

The following table by Gyula László also raises very thought-provoking questions (ibid., p. 39). The distribution of the Uralic vocabulary of the Hungarian language according to Collinder's dictionary:
fi. la. mord. cser. vot. zur. vog. ost. samoj? sure 76 83 124 x)press error 62 619 54 60 51 58 71 74 95 uncertain 9 8 7 10 11 10 5 9 29 total 71 67 61 70 62 68
(?) instead of 59.
If we compare this table with the Finno-Ugric kinship series expressed in percentages compiled by Eeva Kangasmaa-Minn (K.-M.) and Alo Raun (R.) (L. László: id.m. p. 34), we can see that they mutually complement and reinforce each other, and only show certain insignificant differences between the Volga and Permian groups.
The % relationship of Hungarian to other Finno-Ugric languages:
fi. la. mord. cser. vot. zur. vog. ost. jurák-szam. K.-M.: 21 20 All three compilations 265 26 25 22 34 27 — R.: 27 = 25 3-0 — 26 13
therefore show that the Uralic correspondences of the Hungarian language, and the values of the degree of affinity expressed in %, are very evenly distributed among the individual Uralic-Finno-Ugric languages. We will return to this question of primary importance again. For now, we will only emphasize that the Hungarian-Uralic correspondences Epp; show such constant numbers as the ancient Altaic descent found in the Uralic languages. That is, if we follow the same logic in both cases and apply the same measure, we can only conclude that

502
===== PAGE 497 =====
that in this respect, Hungarian is in principle in the same relationship with the Urals as the Proto-Altaic language, with the only difference being that this homogeneous Hungarian language influence is much stronger in them than in Proto-Altaic.

Regarding Collinder's table, Gyula László raises the question: "What can be the explanation for the fact that, of all the Finno-Ugric languages, Hungarian has the most Uralic commonality with Samoyed, while Samoyed has the most words of common origin with Finnish and Lapp, and has much more in common with the other Finno-Ugric languages than with Hungarian?" (id. p. 39). He suggests that groups with larger populations were better able to preserve the traditional vocabulary than those with smaller populations.
"Perhaps this could be the reason for the many ancient archaic elements found in the two largest Finno-Ugric peoples, the Hungarians and the Finns, today?" (id. m. p. 39)
However, the picture can be explained differently, and with much greater probability, if we do not adhere to the rigid language family tree theory of Finno-Ugric linguistics.
There are many indications that — and many Finno-Ugric linguists also claim — that the Voguls, Ostyaks, and Samoyeds — like the Lapps — were not originally Finno-Ugric or Uralic-speaking peoples, but spoke some Paleosiberian, Arctic dialects, and only changed languages relatively late: they adopted the language of the Finno-Ugric peoples living nearby and partly among them. If we accept this thesis — which is made much more probable by very strong anthropological and linguistic data than the hypothesis of a common "Uralic" proto-language — as a starting point, then we can very clearly interpret the striking fluctuations in the distribution of word names between the individual Ural-Finno-Ugric languages.

First of all, the large number of Lapp-Samoyedic correspondences, to which we can also include Ostyak, becomes clear. A significant proportion of the words shared by these three languages, namely all those correspondences that are not found in the other Uralic languages, must in this case be attributed to an Arctic, Paleosiberian base layer. As a second level, we can obviously also include the Baltic-Finnish group, with its 248 Samoyedic and 109 Lappic correspondences, as well as the Voguls, since on the one hand the linguistic influences of the Arctic base layer are also strongly evident in these languages — 178 Vogul-Samoyedic and 77 Vogul-Lapp correspondences — and on the other hand, since the close relationship of the Ob-Ugric languages is indisputable.
These considerations result in the fact that the Uralic, i.e. Finno-Ugric-Samoyedic language family in the classical sense can be rightly divided into a northern and a southern group, which, however, could originally only have had loose relations with each other, and only through long-term contacts and settlements did some kind of areal language equalization develop between them over time.

503
===== PAGE 498 =====
We are not the first to raise this question. Completely identical views are held, for example, by Gyula László (id.m. 1961, p. 68, note 49), and by G. Rank (Völker und Kulturen Nordeasiens, pp. 5-6; J. Wiesner et alii: Die Kulturen der eurasischen Völker). The relevant literature can be found in both places. Similarly, according to Ravila, Nielsen, Lagercrantz, etc., the Lapps and the Ob-Ugrians not only differ anthropologically from the other Finno-Ugric peoples, but also have common features in their language that are absent in the other Finno-Ugric languages, but are present in Samoyed (I. Juhász P.: Introduction to the volume "Studies in the Sphere of Bulgarian-Magyar Relations", p. 9).

All this therefore corresponds exactly not only to the line of thought of Gyula László, which we have already mentioned in our book II (pp. 333-334), but also to the convictions of many other renowned researchers. According to them, the ancestors of the Baltic group were found in the ethnic group We can look for the Lapps in the population of the Paleolithic-Mesolithic Kunda culture discovered in the region of Kum and the upper Volga.
of the Sigir culture in the Ural region, which was presumably more or less related to the Kunda! — let us think of the large number of Lapp-Samoyed word matches — the ancestors of the Samoyeds (László: id.m. p. 193). At the same time, Gyula László seeks the ancient Urals in the narrower sense in the ethnic group of the late Svidrian archaeological culture extending from Central Poland to the Oka. These Swedes were from the 3rd millennium BC.

"... they penetrated north and northeast (there is concrete archaeological evidence of these migrations!), and their western lineages — the ancestors of the later Finnic groups — the proto-Lapplids, I think this would correspond to the linguistic the Ob-Ugric people — the proto-Samoyed Sigiris. It could also serve as an explanation for the remarkably strong Samoyedic connections facts, and perhaps mixed with the Kundans, and the eastern ones — the ancestors of preserved in the Hungarian language," concludes Gyula László (ibid., pp. 194-195).
We would add to this truly convincing argument that we can hardly see in the Swedes proto-Urals in the sense in which this ethnicity — and their language — is defined by Finno-Ugric linguistics.

Indeed, taking into account the clear, irrefutable results of anthropological research, we must indisputably include the distant ancestors of the Ob-Ugric peoples in addition to the pre-Samoyeds: the anthropological characteristics of the Voguls and Ostyaks show no commonality with the groups of Finns, but they are closely anthropologically related to the Samoyeds, and their languages also contain much more Paleosiberian-Arctic elements, which are only found in Samoyed, than in the proper Finnish languages.

If, therefore, there were no ancestors of the Samoyeds and Ob-Ugric peoples among the Swedericks — and this follows from the above with certainty —
504
===== PAGE 499 =====
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We can probably say —, then the language (or rather We cannot speak of "Uralic" or "Finno-Ugric" pre-ethnicities in their case, at most of them are proto-Finnic. Going further, it follows from this that the languages) of the Swedes cannot be called a "Uralic" or "Finno-Ugric" basic language, but only a proto-Finnic language chain, since the language chain extending from the Vistula to the Oka In a vast area under Paleolithic-Mesolithic conditions, with a very low population density necessarily resulting from the fishing-hunting-gathering economic form — which is also confirmed beyond any doubt by the relevant archaeological finds (the small size of the settlements and their great distance from each other) — it is downright absurd to assume the development of some kind of basic language of even a relatively uniform language family.

Continuing our discussion above, with the penetration of the Swedish ethnic groups south of the 6161407, pre-Ugric" 65 64 groups, i.e. the Kunda-Sigirians, to the north and northeast, the mutual influences between the dialects of the Kunda-Sigirian Arctic tribes and the Swedish proto-Finnic groups became increasingly intense due to geographical proximity, and the process of language equalization began (cf. Book 11, pp. 333-334). We can also clearly date these migrations, and with them the earliest beginnings of the development of the primary pre-Uralic or proto-Finno-Ugric areal language chain, archaeologically: we can obviously consider all of this to be simultaneous with the appearance of comb-pitted pottery in the Baltic, Oka (and later in the Urals), i.e. the middle of the 3rd millennium BC. This date, however, we emphasize, can only be applied to the very first stage of its current processes, because paleoanthropological research has also shown that the Lapp race dominates in the anthropological material of the early northern finds of comb-pitted ceramics (Lipták: Questions of Hungarian Prehistory Based "Uralic" languages language leveling on Anthropological Research), so these are not relics of the later Finno-Ugric peoples, but come from the Kunda pre-Lapps who have not yet mixed with the Swedes, or who have only mixed to a very small extent, and who, according to them, learned the craft of pottery making from the Swedes at the very beginning of their contacts.

We note here that the existence of certain looser linguistic relations between the Kunda-Sigiris and the Swedes is conceivable even before the latter's northern expansion, but we are convinced that only after the establishment of a closer language community that began to develop as a result of the Swede migrations that began in the middle of the 3rd millennium — which could have been consolidated a few hundred years later, roughly after the turn of the 3rd/2nd millennium — can we speak of some kind of, but in a broader sense, not necessarily interpreted as a unified basic language, but only as an areal language association, a kind of "lingua franca"-like connecting language.

505

===== PAGE 500 =====

* In these considerations, however, we must take the southern neighborhood into account 13" and we must not forget that the Swedes were displaced from their previous places of residence from the middle of the 3rd millennium by the northward expansion of the Southeastern European cultures (Tripoli, Kuban-Maykop, North Pontic culture), which were of pre-Asian origin and partly already had developed metalworking, \$ and that these southern cultures reached the forest belt by the turn of the 3rd/2nd millennium, which forced the northern fishing-hunting tribes to even narrower areas. We must view the northern and northeastern migrations of the ancestors of the Lapps in the west and of the later ancestors of the Samoyeds and Ob-Ugric peoples in the northeast as the northernmost aftershocks of this expansion 16 a Towards the end of the 3rd millennium (cf. László: id. m. pp. 193-194).

Since the Swedes had lived in the immediate vicinity of the aforementioned southern agricultural cultures for centuries in their former places of residence, we must definitely take into account the significant influences of the language or languages of these developed, populous cultures in their languages and dialects. That is, the Swedes who moved north around 2500-2200 BC originally brought with them linguistic elements originating from the southern, ultimately Neolithic-Early Metal Age, pre-Asian linguistic koine area.

Going further, in the northernmost genera of the Swedes — which, living closest to the Kunda-Sigirian Proto-Alapians, Proto-Samoyedians, and Proto-Obi-Ugrians, were the first to penetrate their territories and mixed most with the Kunda-Sigirian groups that remained in place — we must see those ethnic and linguistic units of the early Finno-Ugric peoples in which the most Proto-Alapian and Proto-Samoyedic basic layer word correspondences are found. These would therefore be the Finno-Ugric languages forming the northern band mentioned above, first of all the Baltic-Finnish group, and further east the Volga and the northern wing of the Permians, Cheremis and Zürjen, with their 248 (Baltic), 161 (Cheremis) and 177 (Zürjen) Samoyedic cognates, and 109 (Baltic), 75 (Cheremis) and 67 (Zürjen) Lapp cognates. It can even be stated that the Proto-Lapp mixture had a deeper effect on the Finns than on the Samoyedic, because the number of Lapp cognates is almost the same for the southern peoples of the Volga and Permian groups, the Mordvins and the Votyaks (75 and 62, respectively), while the Samoyedic layer is significantly smaller for these southern ones (143 in Mordvins and 140 in Votyaks). This difference corresponds exactly to the evidence of archaeological finds: the Kunda (pre-Lapp) culture was much more widespread than the Sigiri (pre-Samoyed) culture.

It would be a shame to have to explain the remarkably large number of Hungarian-Samoyed word correspondences compared to the Hungarian equivalents of other Finno-Ugric languages. However, for this we need to make a small digression,

For comparison, let's first look at a completely different kind of word formation, András Róna-Tas 506 His book "Language Affinity" page 448.

===== PAGE 501 =====

page, he provides a table with the synonyms of the first twenty Hungarian words beginning with "f" in alphabetical order, said to be of Uralic-Finno-Ugric origin, in other Uralic languages:
per language } 12 f-words in Finnish 9 f-words in Estonian 9 f-words in Volga-Finnic We a) find
a) Cheremis 5 12 Permian a) Zürjen 7 Ob- 3
b) Mordvin 9 b) Votjak b) Votjak 13 (southern) c) Tavgi So, counting the coincidences ~=
occurring
in the individual languages 13 f-words in Samoyed 18 words out of twenty (90%) identical in Ob-Ugric, 1
7 in Solkup 2 11 in Baltic, b) Sayan 1
Volgaic, 9 in Permian, and 15 words out of twenty in the entire Finnic group, while only four in the Samoyedic group. Róna-Tas draws the conclusion from this distribution that Hungarian is much closer to Ob-Ugric languages than to the other Uralic groups.

However, if we look at the Hungarian word equivalents that occur together in each of the languages of each Uralic language group (i.e. how many Hungarian words are found together, for example, in Vogul and Ossetian), rather than counting the parallels that can be found alternatively - as Róna-Tas does, a picture emerges from the comparison: --, then it's completely different

Lapp	3 +1 Baltic (Finnish + Estonian)	9	0 Volga (Cheremis + Mordvin) 1 1 0 Permian (Zürjen + Votyak)	7 Obi-Ugric (Vogul + Odzhan)	6 1 1
Samoyed (Northern + Southern)	2				

But from this it emerges that — with strict restrictions of course, since they were once particularly closely related to only twenty — We took into account the word beginning with "f" -- that the predecessors of the Hungarians were not only the obi-Ugric groups, but at least to the Permians and the Baltic-Finns

507
===== PAGE 502 =====

they were bound by as many - if not more - linguistic connections. In other words: if the theory of the Finno-Ugric language family tree is correct, then this szövegösszitas -- which, by the way, also remarkably coincides with the percentage of Finno-Ugric affinity of the Hungarian language (see above) -- could only be interpreted as meaning that the Proto-Hungarians, as a kind of "spiritus rector", located in the center of the Finno-Ugric peoples, were just as closely related to the Baltic-Finnic and Permian groups as they were to the Ob-Ugric ones.

From this, however, we can draw two important conclusions. On the one hand, the circle closes again: the Hungarian-Finno-Ugric word correspondences are equally evenly distributed among the individual Finno-Ugric subgroups — except for the Volgaic ones, which, however, is undoubtedly only a coincidence due to the narrow cross-section of twenty words, once again highlights the special position of Hungarian among the Finno-Ugric languages.—, like the ancient Altai agreements, i.e. On the other hand, the migration of Finno-Ugrics, or rather the separation of its languages, apparently took place in a completely different way than Finno-Ugric studies assume. How could, for example, the individual languages of the Baltic-Finnic group have preserved much more Hungarian correspondences (9) than the Ob-Ugric languages (6), if the Proto-Hungarians — according to the language family tree theory — had coexisted with the latter for a good thousand years, after which the Western Finns had long since separated from the language community?

Do these similarities not rather indicate that at some point in ancient times, when the Finno-Ugric language subgroups that have now split into separate languages still lived together, they all underwent a roughly even, homogeneous Proto-Hungarian-like linguistic influence — the Hungarian words found side by side in each of the group languages would be a reminder of this primary connection — and then later, when separate languages had already developed from the Finno-Ugric subgroups, some of these again came into closer contact with Proto-Hungarian, and from this second contact would come the additional Hungarian word similarities found in each separate Finno-Ugric language, which have no equivalent in the other individual languages of the subgroup in question, and whose number would, according to this view, vary according to how close this secondary connection was between the already separate, independent Finno-Ugric languages and Hungarian?

Specifically, this would mean in Hungarian that the Ob-Ugric languages are closest to Hungarian not because our language derives from the so-called Ugric root language, but because Proto-Hungarian, after a primary, general linguistic influence on all Finno-Ugric languages, later came into closer contact with the Ob-Ugric languages and again influenced their languages.

In any case, this assumption seems much more probable in the light of known historical, cultural-historical and anthropological data than the idea that

508
===== PAGE 503 =====

that the Hungarian language and ethnicity originated from the same root as the Obi-Ugrians, who are anthropologically completely different, belong to a completely different anthropological species, and, in fact, were originally hardly Finno-Ugric in language.

These conclusions, of course, cannot be considered as proof in themselves, since they are based on only twenty extracted words. However, another aspect of Róna-Tas's table is much more significant. It reveals that out of the twenty questionable Hungarian words, only four are found in some Samoyedic dialect, and not even in all of them. Moreover, even one of these four (farag) has no parallel in any Finno-Ugric language other than Vogul. We get a similar picture if we also take into account the three uncertain Samoyedic correspondences in the table: two (far and fázik) also have parallels only in Ob-Ugric.

These connections again only testify to the fact that Hungarian occupies a special position among the Uralic languages: three of the four 012105 and three uncertain Hungarian-Samoyed word parallels — i.e. 43% of them — are found in the Ob-Ugric and Samoyed languages, and are absent from all three Finnic groups. Even from this narrow cross-section, it is obvious that the assumption of a common Uralic proto-language is highly improbable, and the Samoyeds are by all indications only a strong Finno-Ugric? — let's call it that for now — language influence, which was largely transmitted to them by the Ob-Ugrians, and in the process they adopted numerous lexical, grammatical, and language structural elements. In other words, we have once again gained strong support for the fact that, if we can speak of a Uralic? language community at all, it is not a primary formation developed from common ethnic and linguistic foundations, but can only be considered the result of secondary languageequalization through complicated processes of ethnic and language mixing.

But let's move on. Now, after drawing the lessons of the Róna-Tas table, we are on much firmer ground to answer Gyula László's question above: why does Hungarian have the most Uralic commonality with Samoyed among all related languages, while Samoyed itself is much closer to other Finno-Ugric languages than to Hungarian?

From the Róna-Tas table we could see that these correspondences were collected by Finno-Ugricists from all Samoyed dialects one by one, and the total number of words thus obtained measures the degree of Hungarian-Samoyed linguistic affinity. However, we also showed that if we consider these parallels reduced to the northern and southern Samoyed dialects, their number is already reduced by at least half: out of the 4 Hungarian-Samoyed correspondences in Róna-Tas' compilation, only 2 are found in at least one northern and one southern Samoyed dialect.

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===== PAGE 504 =====

Considering the linguistic order, we do not find a single coincidence that occurs in all Samoyed dialects. In other words, this distribution leads to the same conclusion that we mentioned above in the Hungarian-Obi-Ugric context: the large number of coincidences counted together can be explained much more clearly by repeated areal linguistic influences than by assuming genetic linguistic affinity.

But from the table, as well as from the way Róna-Tas evaluates the word correspondences in question, we could also notice that Finno-Ugric linguistics really considers every single Hungarian-Finno-Ugric word parallel together and lists them to prove the "Finno-Ugric origin" of our language, even though these correspondences are found completely irregularly in the individual Finno-Ugric languages, appearing here and there (cf. pp. 480-481).

According to our theory, the Uralic language family in the modern sense was consolidated in its final form by the fact that a population with a much higher cultural level, speaking an agglutinative language, once exerted very strong influences on the emerging Proto-Finno-Ugric and Proto-Samoyedic - or, if you like, living ethnic groups, before the final consolidation of the language alliance, i.e. approximately in the times "pre-Ural" — language chain in primitive conditions immediately following the turn of the 3rd/2nd millennium BC. These massive linguistic influences must have first reached the Proto-Finnish, Proto-Obian, and Proto-Samoyed groups on the eastern wing of the "Uralic language chain" outlined by Gyula László (1. id. m. p. 38), mostly in the region between the Volga and the Urals, but also extending to the eastern slopes of the Urals. It is very likely that the western groups of the later Finns — with the exception of the predecessors of the Lapps — had not yet reached the shores of the Baltic Sea during the period in question, and their settlement areas only extended to the upper reaches of the Volga at that time. For example, the Finns only appeared in the territory of present-day Finland in the 1st century AD (R Dencker: Die Kultur Finnlands, Wiesner et alii: Die Kulturen der eurasischen Völker

in volume 1). Likewise, the Samoyeds could not have been far from their original Central Uralic-Eastern Uralic dwellings (Sigri): Finno-Ugric linguistics itself teaches that the Samoyeds were still living together in the Irtys region around 500 BC, and the unity only broke up at the beginning of our era (I. Juhász P.: id. m. p. 15). - Incidentally, this late migration also very likely indicates that the Uralic language family is a much younger formation than orthodox Finno-Ugric studies believe.

The linguistic influences of the people with a higher culture therefore -- according to our working hypothesis -- initially directly reached the ancestors of the Finns and the Ob-Ugric peoples, as well as their eastern neighbors, the Proto-Samoyeds, on both sides of the Ural Mountains and in the middle Volga region. After this period - i.e. the first centuries of the 6th and 2nd millennium BC - the Finnish groups moved further and further west, and the 510 a

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===== PAGE 505 =====
contacts, while the ancestors of the Ob-Ugric and Samoyeds remained in close contact with the southern colonists for centuries: the Early Bronze Age civilizations of the Volga-Ural region did not expand westward, but primarily eastward, and played a very important role in the emergence of the 18th-17th centuries BC. in the formation of the Andronovo civilization, on the northern and northwestern borders of which the pre-ethnic groups of the Samoyeds and Ob-Ugric peoples lived. This also solves the issue of the particularly numerous Hungarian-Samoyed and Hungarian-Ob-Ugric word correspondences: longer-term contacts resulted in more word borrowings. In addition, however, even later repeated contacts can be proven between these southern steppe, forest-steppe ethnic groups, as well as the Ob-Ugric peoples and the Samoyeds: let us think of the 1st century BC. to the Ananino culture of the 1st millennium BC in the Southern Urals, which ultimately was a Scythian-Sarmatian colonial culture in the forest-steppe zone (cf. Book II, pp. 331-332), or to the southern "Ugric" ethnicities of the Lomovotov culture, which appeared in the first half of the 1st millennium AD in the Urals, the middle Volga and the Kama, in the Perm region (Book I, pp. 262-263).

In the light of the latest studies and insights of international linguistics on language development (1. pp. 407-428 of this book), the presented hypothesis is much more probable and more modern, even from the perspective of linguistics, than the traditional language family tree theory of Finno-Ugric studies. In addition, from a prehistoric point of view, it also has the invaluable advantage that, while previous ideas have been clearly refuted by recent archaeological findings, showing that in the period (6th-4th millennium BC) and region (the Oka-Kama-Ural region and the northern regions of Western Siberia) assumed by the Finno-Ugric linguistic and prehistoric school, not even traces of human life can be found (I. László Gy.: On the study of the Uralic homeland), let alone such a compact population density that would have allowed the development of a more or less uniform language family base language. is indispensable; in the meantime, our paleolinguistic-prehistoric approach outlined above can be effectively supported archaeologically.

We already know that in the first half of the 2nd millennium BC, settlers with developed agriculture, animal husbandry and a high-quality bronze culture arrived en masse, in several waves, from the Caucasus to the region of the middle Volga, the Oka, the Kama, the Byetaja and the Southern Urals, and even to the area around the Ob and the Irtys. These highly educated colonists evidently settled on part of the local indigenous population, subjugated them, and in a manner similar to the Spanish conquerors of Central and South America in the 16th century, enslaved them and made them do the hardest work, primarily the cultivation of the Ural ore mines. Other indigenous tribes apparently avoided them and moved north, northeast or northwest; for example, it can be archaeologically proven that the Volga-Kama tribes

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===== PAGE 506 =====
- that is, the Proto-Finn groups - migrated westward in the Volga River valley around and after 2000 BC (1. Juhász P.: id. m. p. 13), and the ancestors of the Samoyeds and the Ob-Ugric peoples - also around the turn of the 3rd/2nd millennium BC - migrated northward and northeastward in the Ural Mountains region (I. László Gy.: id. m. 1961, pp. 193-194). These migrating groups were later joined by those refugees who, fed up with servitude and the difficult and unusual work, undoubtedly continuously escaped from their southern masters in droves — taking with them, however, the elements of culture they had learned from them in the meantime and, not least, the southern cultural language, which they had adopted in a pidginized form and mixed with their own original language, incorporating its sound system, which language then became the language of communication, the "lingua franca" of all the northern tribes in the area.

We must also take into account the very important circumstance — and we emphasize it again — that according to archaeological findings, the southern colonists arrived en masse in several waves, in contrast to the sparse, scattered indigenous population, with only a few fishermen or hunters per settlement. Therefore, a huge numerical superiority also contributed, and the two factors together undoubtedly amplified the linguistic effects on the Pre-Uralians living among and around them.

In all probability, these highly educated and massively arriving southern colonists formed the nucleus of the "Ugric" people in the true sense, which - since it appeared on the stage of history - has towered above all other peoples, now called Finno-Ugric, in terms of culture, society and politics, as well as in terms of numbers. According to the evidence of the linguistic, cultural history and ethnographic data we have known so far, we can have little doubt that this more developed language, which once dominated the pre-Uralic dialects - whose unifying influence ultimately led to the formation of the actual Uralic language family - was either directly Proto-Hungarian, or at least a very close relative of it.

However, with the above assumption, we could also explain why there are supposedly so few "Ősáltáj" elements in the Hungarian language, much less than in any other Uralic language, while at the same time the number of our so-called Old Turkic loanwords far exceeds the later Turkish equivalents of the other Uralic languages. We have already mentioned that it would be very necessary to thoroughly review these Hungarian words declared to be of Old Turkic origin in terms of how many of them are really later loanwords and how many are ancient equivalents (I. see what was said above on page 500). Our linguistics cannot explain these Old Turkic loanwords in a satisfactory way anyway, because their phonetic form-512

===== PAGE 507 =====
They vary from ik amongol through Old Turkic and modern Common Turkic all the way to yyy- vas (1. Book II. pp. 234-236). To shed more light on the validity of our doubts, we will again rely on the latest results of archaeology. Since + as we have already said = -these southern Ural-Volga-Ob region bronze-working settlers also played a significant role in the formation of the Andronovo civilization that flourished in the middle of the 2nd millennium BC, a cultural sphere that extended from the Volga to the Altai and south to Turkmenistan, it is very conceivable that the language of these colonists - together with the language of the population of the Afanasyevo civilization, which is completely identical to them anthropologically and also shows many parallels in the cultural field (1. Book I, pp. 31-33 and 49-50) - also had a strong influence on the later developing Altai languages, and from this southern cultural language - or from several dialects close to each other - the so-called A large proportion of our Old Turkic loanwords entered the Altaic-Turkic languages sometime during the Andronovo period (more on this later).

Let us now look at some of the concrete linguistic evidence for this paleolinguistic approach. Excerpts from Lakó-Rédei's etymological dictionary "Finno-Ugric elements of the Hungarian vocabulary". Below we will examine the vocabulary of our language determined to be of Uralic, Finno-Ugric or Ugric origin, and examine the more distant proto-linguistic (Sumerian) connections of these words. Since we cannot deal with every word of Finno-Ugric origin, which our linguistics has estimated at around 1000, in the framework of this work, we will only present excerpts. However, in our compilation, we are not dealing with examples selected arbitrarily, unilaterally, on an idea or perhaps purposefully, but with a truly representative cross-section. Our working method was that, starting with the randomly opened page 298 of the Hungarian-Finno-Ugric etymological dictionary (HM) volume II, edited by Lakó-Rédei, we listed the words published there that are definitely or probably of Uralic, Finno-Ugric or Ugric origin, until we had fifty, so to speak, first

"We found no obvious Sumerian parallel that could be recognized at a glance."
1, hón (hón-álj) "AchseI, Achselhöhe", Finno-Ugric (Uralic?) base form; *kon, or *kana, Sumerian "kun" = 1 We are indebted to Sándor Csýke, . Schuler" (shoulder); cf. "hón-álj" =unter der Schulter" (the part below the shoulder). who provided valuable assistance in compiling the Sumerian correspondences | 513

===== PAGE 508 =====
hord "carry"; hordó "Barrel"; hurcol "sleppen, tragenherum". Uralic root forms: * kur, - Sumerian: "gur., guru," = * "tragen" (carry); *gur., guru," = "Tonne, Graineemagazin, ein Hohlmass" (barrel, grain store, a kind of measure of space); in compound form: "e.-gur," = "Speicherhaus" (raktárház); cf. also góre; "bara," = "Beerkass" (söröshördö). horhó, horhos "deep mountain path, hollow path, steep path dug in a jagged mountain slope (by water), narrow cut-in path between two mountain slopes". Finno-Ugric root forms: *kur, Sumerian: har" = "Bachtal, Schlucht" (stream valley, narrow mountain gorge, ravine); , har-ra-an" = to groove, to rip"; groove, groove „Nút, Kerbe, Einschnitt". Finno-Ugric base form: *kurna. Sumerian: „har" = „eintitzen, kerben" (scratches, grooves); ,kur, kuru, kurum" = „schneiden, einschneiden" (cuts, , way" (út). hornyol , notches, folds, grooves, fluting, raked, cuts). horol, hurul „reiben, wetzen, kratzen". Finno-Ugric base form: *kor,- or *korwa-. Sumerian: har" = „ritzen. kratzen, zerreiben, zerkaue" (scratches, grinds, crumbles, chews); cf. also bite (beissen), whose Finno-Ugric base form is also *kor,- (or *kar,-) long "lang". Uralic base form: *kond, or kod, Sumerian: „us"

= "Längsseite lang, lang sein" (long side, long); *Kus, = "ein Längenmass, Elle" (a kind of length measure, elbow, cubit). , to, -hez, -höz "zu, dazu, hinzu". yyyý alapatak: "küé., Szumér: „aS, eS . -uS" = "Nominal suffix: "zu, für, hin" (noun suffix: -to, -hez, -höz, -for, to). lady "Dame, Hermelin"; original meaning "Weibchen,

weiblich", Finno-Ugric base form: *kað'wa or *kað'w., Sumerian: "gal., gala., kala," = "vulva, begatten, begattet" (female genitalia, copulating, fertile woman, or female). hös "Held, heldenmütig, junger Mann". Uralic base form: *kada. 520068: "giS, 21%, ges," = Mann, Mensch, Herr, Held" (man, male, ur, hös).

314
===== PAGE 509 =====

10. hügy "Star", Uralic root: *kurié, or tkuc, Sumerian: bud" = "glänzen, leuchten" (shine, shine, shine); in compound: "bud-hud" = "das Leuchten (Fimmern) der Sterne" (the brilliance of the stars). 11. hügy "Urin, Urin". Uralic root: *kurice or *kué. Sumerian: , bud," = "erstrahlen lassen" (to radiate, to spurt out); »kaS," = "Urin, Urin" (urin, urine). 12. hull "fallen, flowssen, eingehen, crepieren". yyy (Finno-Ugric) root: *kul-. Sumerian: "hal" = "rasch fliesen, kacken, fallen lassen" (to flow quickly, to defecate, to fall); "bal," = "quellen, fliesen" (to flow, to flow); "hul" = "verderben, zu Grunde gehen" (ruins, perishes). 13. vulva "Augen schliessen, blinzeln, zwinkern, entschlafen".

Uräli alapalak: + kuna-. SzumEr: hun" = "to raise the eye, to rest" (szemet emel, nyugszik). 14. hupolyag "swelling, bulge, growth".
Of disputed origin; a) perhaps compatible with the Finno-Ugric equivalents of szállóg;
b) perhaps a derivative of the onomatopoeic origin hop-, hup-ty.
Sumerian: "gub., kub., kab" = "Dicke, Mächtigkeit" (thickness, fatness); cf. fat, göboly (fat cattle); "gab, gaba" = "Brust, Buckel" (breast, hump); cf. kebel, göb, gubö, kápa (bulge on the saddle); "gug" = "Geschwür, Pustel" (ulcer, boil, purulent blister); cf. in the Eme-Sal dialect „g“-„b“, e.g. "dug," (beautiful) = "ze-eb, si-ib" (twenty). 7 15. twenty "zwanzig".

Its antecedent may have been the Uralic root *koje meaning "man, male", to which the false indicative element was added: "the man (i.e.: the man with his fingers and toes)",

Finno-Ugric basic form: *koj/e/cy or *kus3.
Sumerian: , giS, ges," = "Mann, Mensch, Zweig, Hand, Gliedmassen" (man, male, branch, hand, limbs); cf. also kacs, kacsó, hand;
05, gis, = "sechzig" (sixty); see. the number 60 is the first major unit of the Sumerian six-ten combined number system: it means completeness.
515

===== PAGE 510 =====

16.
17.
18.
19.
20.
21.
22.
516

húz "to pull, tear, haul".
Ugor alapalak: *hand,—
Sumerian: "uS" = "ziehen" (pull);
"guz" = "to tear apart" (to tear apart, to tear apart).
nerve "Nerv, Sehne, Strick"; original meaning was "in; Sehne".
Uräli alapalak: *jante.
Sumerian: "sita" = "Band, Sehne, Seil" (ribbon, string, rope).
young "jung, Jüngling"; its original meaning may have been "young or little boy".
An obscured compound from the Pre-Hungarian or Proto-Hungarian period, formed from Ugric and Finno-Ugric elements.
Sumerian: "ibila, ibilu" = "Sohn, Erbsohn" (son, male child entitled to inheritance).

yes, ja, sehr"; the original meaning could have been "gut, wohl".
Of disputed origin; a) perhaps a Finno-Ugric heritage; b) possibly derived from the derivation of the e-, i-demonstrative pronoun ijen.
Sumerian: , gin, gi-in, gi-en, gi-na" = "feststellen, feststehen, fest sein, bestehen" (establish, confirm, certain, matter is, certain, exist); in compound: "ag-gi-na" = "Wahrheit, Recht" (truth, right).

He worships "in the flesh, in the flesh".
Of uncertain origin; perhaps compatible with zürjen vomid'z „durch den
Evil eye, bewitched damage, illness" (szemmelverés)
damage, disease caused by)
Sumerian: me" = "beten, huldigen, preisen" (pray, worship, praise);
"mu,-mu," = "beten, verehren" (to worship, to respect);
"im, imi" = "Himmel" (heaven, sky); in compound form:
"im-dul, imi-dul" = "Kult" (worship, cult); for the meaning, cf. German
"anhimmeln" (worship, love passionately, worship) — literally: "raise to the sky, praise to the heavens". in "Sehne, Flechse, Leine, Strick".
Uralic base form: *söne or *sene.
Sumerian: "gan, gana" = "Band, Sehne, Leine, Seil, Tau" (ribbon, in, twine, rope);
"in, in" = "Flachs" (hemp, flax); for the transfer of meaning, cf. German "Flachs" (hemp, flax) and "Flechse" (tendon, sinew, tendon), as well as "Lein" (flax) and (twine).

"Leash"

indul "to set out, to depart, to leave";
ing, inog "to waver, to wobble",
Uräli alapalak: *,jom,-.

===== PAGE 511 =====
SzumEr: "in@ = "to waver, to sway" (ing, inog); "in-di" = "gait, course" (menet, futás); "in-di-di" = "to climb, to ascend" (felmegy, felemelkedik). 23, ipa "father-in-law".
Finnugor alapalak: *appe.

Sumerian: "ab, abba, ap, appa" = "Vater, Grossvater, alter Mann, Greis" (father, grandfather, old man, elder); v6. father-in-law is obviously just a mixed vowel of father semantic variation.
24. ir "write, draw, paint".

The origin is disputed; a) perhaps it is consistent with the Ostiak jeritá "to draw, with the word "einen Strich ziehen" (to draw, draw a line); b) it is more likely that aon- ben is a Bulgarian-Turkish loanword; cf. Chuvash Sir- "schreiben" etc.
Sumerian: sar (Ser., Sir,) = "write" (write); s AT, ur, (ru)" = "write, scratch, draw" (scratch, draw); cf. also ró.

25. iramodik "to run, to run fast",
Of uncertain origin; a) perhaps of Finno-Ugric origin; cf. zürjen yrmasny "hurry, hurry" (hurry up) etc.; b) more likely of onomatopoeic nature.

Sumerian: "ir, = "to go quickly" (to go quickly).
26. iránt "against, opposite, in the direction; irányoz "to direct, to aim, to focus sharply"; ardnoyo (aránt) "to divide evenly, proportionally". Ugor alapalak: tar, vagy tur,
Sumerian: "ir, ar, ur," = "zu, auf, für, gegen" (to, for, towards, towards); the word family is therefore of a directional nature; cf. also Sumerian "as, 68, US, 187 = "zu, auf" and Magyar.-hoz, -hez, -höz; Sumerian "de,, da" = "zu, auf" and Magyar. ide, oda; Sumerian "la" = "zu, auf" and Magyar. —ra, -re.
21. mercy, have mercy "Barmherzigkeit, Erbarmen, sich erbarmen"; 1192-95 (Death Sermon): "iorgossun w neki" (have mercy on him), i.e. arch, iorgos or iorgoz.

Derivation disputed; a) perhaps Uralic; cf. e.g. votják jromi- , to get lost", Samoyed (Solkup) yrgak , to get lost, etc.; b) perhaps it is with the verb jár in connection.
Sumerian: "ar-hus" = "Have mercy, have pity; have pity, have compassion" (mercy, compassion: have mercy, have pity);
"ty; = "Weinen, Wehklage" (cry, lamentation, wailing);
in compound, "ir,-ga,-ga," = "flehen, das Flehen" (begs, implores; supplication, entreaty); for the meaning, cf. begs-begs,
517

===== PAGE 512 =====
28. irt (orot, rit) "to exterminate, to destroy, to reap"; arat "to reap".
Uräli alapalak: *Sur,-.

Sumerian: "ur,, uru,," = "to extinguish, to erase, to sweep away, to plough the field" (to extinguish, to destroy, to erase, to plough the field); cf. irt, orot; in compound: "su-ur,-ra" = "to extinguish, to erase" (to erase, to erase); cf. with the Uralic base form derived from the stone;

"Tt" = "to tear out, to remove, to erase" (to tear out, to remove, to erase); cf. rit; "ur," = "ernten" (to reap). 29. also "auch".

Derivation disputed; a) perhaps the derivable Finno-Ugric tice or "ise

its original meaning was "shadow; Schatten" (2311):

b) perhaps it originated from the suffix -s nomen possessoris.

Sumerian: e5 (15.), e5," = "gleichwie, anstatt, wie" (like, so, as, instead of, as); hence not "shadow", but a conjunction; cf. also and.

30. "smell, smell, smell, brand, wound brand, cancer, an evil spirit"; its original meaning may have been "shadow".

Finno-Ugric base form: "íée or "ise.

Sumerian: "izt" = "Feuer, Brand, Hitze, Fieberhitze, heiss" (fire, burning, heat, feverish heat, hot); cf. glows, sweats; thus the meaning "evil spirit" is obviously semantically related to the destructive, harmful spirit of fire, which manifests itself in the form of inflammatory diseases, wound fever.

31. drink "trinken, saufen"; drink "Getränk"; drink "tränken"; drink "Trunk,

Trinken". The "sz" of the present tense stem is originally a demonstrative adjective volt.

Finno-Ugric root: "juke- or "juge-.

Sumerian: "a, e" = Wasser" (water); cf. the root of the Hungarian word i-; cf. also Sumerian "1, id," = "flowing water, river, canal" (flowing water, river, canal) and Magyar ügy (stream, river).

32. iz "taste, marmalade, jam"; édes (arch. ézes) "sweet, dear".

Uráli alapalak: "ípse.

Sumerian: "esi," = "gut, schön, süss, vortrefflich" (good, beautiful, sweet, kitűnő). 33. iz "limb, -fold, degree of kinship, part, section, joint"; iziben

"Quickly, on the spot, immediately".

Of disputed origin; a) Ugric heritage; Ugric basic form: "jyt.; b) possibly

Finno-Ugric; basic form: "jäse or "jäsne.

Sumerian: "iz, is" = "Knochen, Zweig, Baum, Holz" (bone, elägazódás,

faág, fa). »gis" = "bone, branch, arrangement, limbs, hand"

518

===== PAGE 513 =====

(bone, 6148876045, arrangement, limb, member, hand); cf. also kacs, kacsó, hand; in compound: , g15-giŠ" = "jns Quadrat erheben" (raises to a square); cf. the meaning of taste "mal" (-time);

"vizi" = "eilen" (hurry); cf. iziben (1. under point 34).

34. sweat "to sweat, to be very hot, to be very warm"; glow "glowing". Of uncertain origin; perhaps a Finno-Ugric heritage; cf. e.g. votják ast- , to heat, to be heated", etc.

Sumerian: "izt" = "Fire, heat, heat, boil, flare, be excited by the heart, glow, hurry" (fire, hot, hot, boil, blaze, get excited, get excited, glow, hurry); cf. still is, is (Brand, Wundbrand), is (-mozog), is agitated, in taste; in taste" therefore obviously belongs here, and not to the word taste (Gled) .

35. jár , to go, to walk around, to go back and forth".

The origin is disputed; a) it may have evolved from the meaning "screw, coil", this

in this case the Finno-Ugric basic form is "jork,-; b) "rolls, rolls" derivative,

in this case the Finno-Ugric basic form is tjor.-.

Sumerian: "gur" = "wenden, umwenden, drehen, sich wenden, sich umwenden, zuruckkehren" (turns, turns, twists, rolls, turns, turns, returns); cf. rolls, rolls, circles, circles, circles;

"gur,, gir," = "laufen, rennen" (run, rush); cf. fast.

36. plays "spielen";, its original meaning could have been "sprechen, erzählen; speak, tell".

Finno-Ugric root: tjukta-.

Sumerian: "gudu" = "sprechen, nennen" (to speak, to say, to name);

»gudda» = «dancing, hopping, jumping» (dancing, hopping, leaping).

37. I "Eis".

Finnish language: "jäpe.

Sumerian: "seg, = "Eis" (ice); cf. also Sumerian "sid" = "Frost, Kälte; sich beruhigen" (frost, coldness, calm down) and Magyar. hiedg, chitul.

38. sign "Zeichen, Signal, Merkmal"; mark "Zeichen, Kennzeichen, Merkmal"; the original meaning could have been "mark, stain, line; Spur, Fleck, Strich". Finno-Ugric basic form: tjülke.

Sumerian: "gir, = "Fuss, Schritt, Weg, Pfad, Tritt, Spur" (foot, step, hit, stroke, trace);

"gug" = "Siegel" (seal, property mark).

519

===== PAGE 514 =====

39. good "good, fairly good, good, fat, thick"; better "the rights"; 1192-95: iov, 1448: yog; vernacular: gyaó, gyó.

Ugor Alapalak: tjom, va am.,

Szumer. dug," = "good, beautiful, friendly, kindness, benevolence" (jó,

beautiful, friendly, goodness, goodwill);

"gub,, kab" = "die Linke, fett, dick" (left, fatty, fat); cf. even fat: in antiquity (e.g. even among the conquering Hungarians) the left side was the noble, the favorable, the "better" side; the same can be said for the Sumerians:

"gub, kab" = "die Linke, Gehilfe, Stellvertreter" (the left, assistant, deputy) — that is, someone's "left hand" and not "right hand", as we say today.

40. comes, comes "kommen"; vernacular: gyön, gyün, yün.

ýýýý (Finnish?) alapalak: "j&p;-.

Szumér: "gi, gin, gen" = "go, send, turn, return"

(goes, sends, turns, comes back).

41. jut "to come, to reach, to befall, to befall"; népnyelvi: gyut. ýýýý alapalak: "jukt,-.

Szumér: "gid," = "to come, to arrive, to come near |

men" (gets there, reaches the destination, arrives, comes close to); in compound ~~tois~~ "to come" (delivery, delivery); cf. "8u" = , Su-gid" = 42. kéd-: ekkédig , this time; "Hand".

original meaning could have been "circle, hoop,; Kreis, 1 Ring, Reif", and is probably related to the word kégy with the same meaning (1. there).

Sumerian: "gid," = "festhalten, abgrenzen, abschliessen" (to fix, to delimit, to close, to finish); hence: "to the limit" (in place and time).

43. grace, RKegebogen"; grace "stadium, circus, curriculum, meta" (measure of length, circle, racecourse, running, race, turning point, boundary, endpoint); the original meaning could have been "circle, hoop".

Disputed origin; the presumed Finno-Ugric root form is tkec3

Sumerian: "gid," = "to be long, to measure, to determine, to delimit, to end" (to measure,

to determine, to delimit, to end); "gig, = "to turn back, to turn, to twist, to coil together" (to turn back, to turn, to revolve, to go around, to twist, to coil, to coil).

44. lust "Lust, Wollust, Wonne"; kény "Willkür, Lust"; the original meaning could have been "rumbling, roaring; balzen, Balz".

Finnish language: "keje(-).

Sumerian: "ki-ag," = "lieben, Liebe, Lust, Gunst" (love, affection,

pleasure, grace); cf. also grace, gracious;

520

===== PAGE 515 =====

45.

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"gig" = "Raserei, Besinnungslosigkeit" (frenzy, ecstasy); "gin" = "willig sein, gehorchen" (to be willing, to obey); cf. also , gin," = "Sklavin" (slave woman). kel "aufstehen, sich erheben, keimen, spriessen,

wachsen, aufgehen (Teig), entstehen, anfangen, Abgang finden (Ware), anschwellen, reifen (Geschwür), gehen, kommen"; east "Date"; spends "dichten, (Geld) ausgeben"; moves "ziehen, fortziehen". Ugric (Finno-Ugric?)

base form: "kälä-. SumÉR: "gal," = "heben, erheben, hochbringen, hochkommen, aufziehen, voll sein, füllen, festsetzen, werfen" (raise, raise, raise, raise, fill, fill, determine, throw, scatter); cf.spends: "he throws the money around"; "kel, kil, gil," = "walk, sprout, sprout, grow strong, mature, fully ripe fruit, bake (bread)" (salad, sprout, sprout, grow strong, Erik, ripe fruit, bake bread). kemény , hart, fest". Finno-Ugric (Uralic?) base form: tkümö.

Sumerian: "kim," = "to fasten" (to put together firmly); "gim," = "to be hard, solid". stirrup "Steigbügel, Bügel, Hosensteg; Schlaufe oder in einer Schlaufe endender Strick; Riemen am hölzernen Dolch, der beim Maize-husken am Zeigefinger befestigt wird; Band aus Leinen oder Leder, mit dem die Pelzveste hinten zusammengebunden wird; Henkel eines Topfes, Juges oder Korbes", Of uncertain origin; perhaps the "Kreis, Ring,

Reifen" is a derivative of kégy (1st place); the stirrups of the conquering Hungarians were loop-shaped, and the vernacular meanings all roughly denote a loop-shaped object; for historical reasons, they cannot belong to the Ugric period: the beginning of the use of stirrups can be dated to the first centuries AD. Sumerian: "gan, kan" = "Gestell, Band, Binde" (stand, ribbon, string, bond); "gen, gin" = "feststellen, festmachen, gehen, treten" (fix, tighten, go, step, tread); in compound: 815 gir-gin = "Littlrbrett, Schemel" (step, stool) — literally , (made of wood) lábta- mastztó"; the so-called determinative , gîš", which does not need to be read, only specifies the material of the object, here, , made of wood, of wood"; "gir," =Fuss" (foot); 521

===== PAGE 516 =====

48.
49.
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"gil" = "wickeln, verwickeln" (to wrap, to coil); it can therefore be concluded: "gin-gil" = . bark "Rinde, Borke, Kruste", support wheel, fixing loop"; cf. ken-gyel.

Finngor alapalak: "kere. SzumeEr; , gar" = "covering, to coat, to enclose all around" burok, burkolat, beburkol, körülvesz, körülzár); »Kar,, kara," = "enclosure, surrounding" (körülzárás, ancient fence); cf. also corral, fence, frame, garden, to chew again, to mull over something, to mull over", Of disputed origin; a) perhaps a Finno-Ugric heritage; Finno-Ugric base form: tker,-; b) possibly a Turkish loanword; cf. Old Turkish "kevir- , to chew"; Ottoman gevir- , to chew, to chew". Sumerian: "gar, bar, hir," = "to grind, to grind, to chew" (to grind, to grind, to grind); cf. also bite;

"Kir,, gir," = "gnaw, nibble, bite" (rág, megrág, harap). kerûl "to go around, avoid, come, reach, get"; kerek "round, circular"; kerék "wheel"; kereng, kering "to circle, run around, turn in a circle, circulate"; kergert "to chase, pursue, drive"; kerit "to enclose, fence, surround"; kert "garden, fence, enclosure". Hangfestő jellegű szócsalád, az eredeti jelentés , kerek, forgó; round, rolling", illetve , forog, forgat, fordít; to turn, rotate, twist" lehetett,

Finno-Ugric base form: "kerá(-) or "kerá(-). Sumerian: "gar, kar," = "enclose, enclose, enclose in rings" (bekerit, frame, surround, enclose); "gu" = "to turn, to turn towards, to return, to surround, to enclose" (fordul, odafordul, visszakerül, körülvesz, körülkerit, kortlizar); "Kur,-kur," = "to bend, to curve, curvature, circle" (görbit, bend, curvature, circle). knife "Messer". Finno-Ugric basic form: tkeg,. Sumerian: „gîš," ("815 has") = "Sichel, Krummschwert" (sickle, curved sword); 085, kaz" = "abschneiden, zerschneiden" (cut, cut); cf. also hasit, kasza (the latter is a Slavic loanword according to our linguistics). So we soon achieved our goal. Only the dictionary 61 each other

We had to look at the next heading after , and among these we found 51 obvious Sumerian equivalents. This means that the Hungarian vocabulary determined to be definitely or probably Finno-Ugric heritage is represented by this

===== PAGE 517 =====

In Tatty's cross-section, 83.6% of the words have a very close phonetic and semantic parallel in Sumerian that is recognizable at first glance, and in the vast majority of cases the meanings are exactly the same.

Note: the planned 50 words were expanded to 51 because the word "krek" obviously cannot be of Finno-Ugric origin for cultural-historical reasons - as the Lakó-Rédei dictionary also states. The exact ratio is therefore 50 Sumerian correspondences out of 60 entries, i.e. 83.3 90. In the first place, this is enough to prove that the ancient Proto-Asiatic languages - and within these primarily Sumerian - cannot be ignored in any way when examining the formation and development of Hungarian and the related Ural-Finno-Ugric languages. At the same time, the fundamental theoretical question inevitably arises as to what extent, under such circumstances, those words of our language that have more or less acceptable equivalents in some Finno-Ugric languages — but by no means all of them! — are of "Finno-Ugric origin." This very important circumstance, which we mentioned above (see pp. 480-481 of this book), was pointed out emphatically by Vámbéry a hundred years ago when he stated that "... Finno-Ugric linguists, due to the absolute lack of uniformity of the Finno-Ugric languages, collected the evidence throughout the mentioned language area..." (1. above, p. 496). Vámbéry then continued his train of thought as follows: "... In this way, we should consider the Hungarian language, so to speak, to be the root and mother tongue of the entire Finno-Ugric group, — and this is a circumstance that we find... impossible..." (1. Vámbéry: The Origin of the Hungarians, pp. 256-257)

Us despite the fact that Vámbéry naturally rejected this otherwise completely logical conclusion in the interest of his own theory (we know that he derived the word from Turkish) (He considered Hungarian, or more precisely, our language to be a Turkish-Finno-Ugric mixed language, so he had to find such a thing a priori "impossible" if he did not want to refute his own theory himself), in the light of the new data we have learned so far, it seems very likely that he was probably touching on the essence of the relations between the Hungarian and Finno-Ugric languages in his quoted sentence. All the more so because we could also see from the above word examples that the Sumerian parallels are overwhelmingly much closer to Hungarian in both phonetic form and meaning than the Finno-Ugric correspondences listed by Finno-Ugric studies.

Theory and practice of Finno-Ugric linguistics In light of the above-mentioned connections, it is clearly methodologically unacceptable that Hungarian linguistics only takes into account the contemporary linguistic material of Finno-Ugric languages — because their linguistic monuments are practically non-existent — in order to understand the origin and history of the Hungarian language. 523.

===== PAGE 518 =====

research, but even this in such a one-sided manner that it pays little attention to the internal development of the Hungarian language itself. This one-sidedness has recently been noticed by some of our renowned Finno-Ugric linguists. For example, Loránd Benkő, in his thesis entitled "Hungarian Linguistic History - Hungarian Prehistory", clearly states: "Besides and behind the extensivestudies based on Turkology and Finno-Ugric studies, the field of research in which the questions of our prehistory can be elucidated from the perspective of Hungarian linguistics in the narrower sense has not developed sufficiently, and indeed — one might say — has lagged far behind. The weaknesses of Hungarian linguistic history in this respect have also affected the reliability of the results of the other two areas, since it was not sufficiently possible to confront and verify their propositions from the perspective of the "target language"." (Id. m. Hungarian Prehistory Studies, p. 46) This means, in the attractive packaging of scientific language, but in Hungarian terms, that Hungarian prehistory and language development have been researched primarily on the basis of Finno-Ugric and, to a lesser extent, Turkish languages, with the Hungarian language itself being virtually ignored. And this is precisely what we ourselves most object to in Hungarian linguistic and prehistory research with an exclusively Finno-Ugric perspective. However, let Professor Benkő speak for us again, who puts it more openly below: Despite the undeniable development, two fundamental weaknesses of our linguistically based prehistory research still exist and are still effective today: a) the continuation of propositions and details that are already outdated or at least debatable according to the current state of linguistics as "common knowledge" facts, and their use mainly in the social sciences; b) the incompleteness and insufficiency of the complexity within linguistics due to the gaps in the studies of Proto-Hungarian in the special Hungarian linguistics. There is also a certain degree of correlation between the two shortcomings: in one-sided approaches, the dangers of errors or at least of questionable results are already a priori given, and if the necessary multilateral linguistic study significantly lacks the specific problematic of the language whose elements are being explained, the reliability of the conclusions drawn from the linguistic explanations and the durability of the results may arouse even more doubts." (Id. m. p. 49. emphasis by Benkő)

He then continues: , The Hungarian people and the Hungarian language lived in a state of complete separation from their relatives for a good one and a half to two thousand years until the conquest of the country. This is a very considerable time in the life of a language, which provides the opportunity for the development of a linguistic system of a largely new quality and a huge number of new, own linguistic elements. Suffice it to point out here that much less time During the 19th century, many new languages emerged in Europe as a result of the differentiation of certain basic languages or the separation of related languages. The complete separation from related languages 524

===== PAGE 519 =====

After the separation, the Hungarian language, which developed independently, according to its own internal laws, outgrew the Finno-Ugric basic language states in enormous proportions, and developed into a separate, specific language system, characteristic only of it. The gradual development of the multitude of specific elements in the Hungarian language system also creates a new situation in the field of linguistic explanations; the tools, facts, and methods of Uralic-Finno-Ugric comparative linguistics are increasingly insufficient for the linguistic approach to the independent period of our prehistoric history, especially its later stages, where — moving forward in time — the role of Hungarian linguistic history is increasingly strengthened and emphasized." (Id.m. p. 49. highlights from Benkő) It seems beyond doubt that Benkő's sentences contain one of the most important keys to the truly complex method of Hungarian linguistic and pre-historical research. His reference that "... in a much shorter period of time, so many new languages have arisen in Europe" is particularly noteworthy. Well, these are indubitable facts that can be proven historically - just think of the development of the Germanic, Neo-Latin or Slavic languages, which took place almost before our eyes, in the full light of history, over the last 1000-1500 years. — which also effectively support the new recognition of Indo-Germanic linguistics and prehistory research, according to which there was no 6-8000-year-old common Indo-Germanic base language, but rather that the Indo-Germanic languages developed after the turn of the 3rd/2nd millennium BC — some earlier, some relatively late, during the 1st millennium BC — in the manner of Areal language associations.

Under such circumstances, we cannot speak of a 6-8000-year-old "Uralic" or 4-6000-year-old "Finno-Ugric" base language, since these chronological assumptions can only be evaluated in close connection with the hypothetical Indo-Germanic base language of 19th-century Indo-Germanic studies and its antiquity, and they stand or fall with it (cf. what was said on page 400). As a result, we must also thoroughly revise the assumption that "the Hungarian language lived in a state of complete separation from its relatives for a good one and a half to two thousand years until the conquest of the country." That is, it becomes very doubtful whether our language structural features, lexical and grammatical elements, suffixes, formations, and signs that cannot be found in the languages of the Ob-Ugric peoples really developed during the "separate life" of the Hungarian language,5 or whether they can be explained much more by the fact that the predecessors of the Ob-Ugric peoples simply did not adopt these features, words, and elements at the time when they were under the influence of the Proto-Hungarian language, although they already existed in Proto-Hungarian? Obviously not, because they still preserved the most specific elements of the vocabulary and structure of their original ancient language — as the Paleosiberian base layer of the Ob-Ugric languages clearly proves.

The most surprising aspect of this question, however, is that the new chronology of language development in Indo-Germanic studies — in relation to Uralic-Finno-Ugric — is

525
===== PAGE 520 =====

It is strikingly similar to the mass immigration and colonization from the Caucasus, which filled the middle Volga and Southern Ural regions with agricultural and animal husbandry settlers of Kuban-Maykop and Transcaucasian origin and with a developed Bronze Age culture in the first half of the 2nd millennium BC.
Recent archaeological research clearly indicates that the 2nd millennium BC was a time of extensive and widespread spread of metal cultures, and this circumstance satisfactorily and reasonably explains the development of the so-called Indo-Germanic, Ural-Finno-Ugric, and even Altaic languages - in the light of the then extensive expansion of the Pre-Asian cultural and linguistic koine into Western Eurasia, in the manner of areal-communicating languages, "lingua francas".
We recall here the statement of the "Handbuch der Weltgeschichte" edited by A. Randa, according to which between 2500-1600 BC the appearance of a foreign element bringing a higher culture from the south and west can be detected throughout the Eurasian steppe. These newcomers introduced the Cro-Magnon type indigenous population to animal husbandry, agriculture, and the secrets of metalworking (I. p. 31).

G. Childe's investigations led to the same result. In his work "Stufen der Kultur" he states: archaeological research has shown that Mesopotamian-Sumerian civilization permeated the northern barbarian regions in the 2nd millennium BC, both in Europe and in Western Asia.
But we are also familiar with B. Stawiski's findings that the population of the Turkmen cultures of Mesopotamian origin flooded vast areas of the Eurasian steppe, from southern Russia to Altai, between 2000 and 1500 BC (quoted in our book II, pp. 322-323).
It is in no way believable that all these enormous, south-north ethnic movements did not have linguistic consequences.
We would supplement Benkő's fundamental theoretical and methodological statements cited above with these considerations, and we do not consider it essential, not only because of archaeological facts, that our linguistic research should also take into account the language — or languages — of these southern ethnic groups, ultimately of Proto-Asian origin, when examining the formation, development, and relations of the Hungarian language. As a start, at least a large number of our words declared to be of unknown origin should definitely be compared with the known ancient Proto-Asian languages, but first and foremost with Sumerian dominating until the beginning of the 2nd millennium BC. This would be a minimal theoretical and methodological requirement even if our position were not otherwise accepted.
This is also because Finno-Ugric studies itself states: The younger a word is, the less likely its origin will remain in complete obscurity, and therefore of unknown origin.
"A significant proportion of our words can be attributed to our ancient vocabulary." (Bárczi: Szófejtő Szótár, p. XII),
526

===== PAGE 521 =====
However, the fact that up to now even these Hungarian words of unknown origin have not been compared with Sumerian - although the archaeological results outlined above have been well known for two or three decades - clearly and irrefutably shows that the exclusively Finno-Ugric
Hungarian linguistics with a .perspective does not work with a careful methodology, does not research truly scientifically supportable facts, but primarily only defends the rigid dogmas of an outdated a priori hypothesis, closing itself off from taking into account any new archaeological, prehistoric and linguistic discoveries that point in a different direction.

Let us now see how this one-sided approach manifests itself in Benkő's theoretical conclusions on these issues, or more precisely in the practical, linguistic application of the consequences that inevitably arise from them, which — as we will see — directly contradicts the most fundamental requirements of rationality,

an understandable requirement: 1. Any Therefore, in those explanations that are connected to the linguistic material of prehistoric sources related to the Hungarians, the following two-fold proposition must be considered as explanation regarding any element of the Hungarian language, approaching it from any other language, must be in harmony with the facts of the history of the Hungarian language. 2. If there are impeccable possibilities for explaining Hungarian linguistic elements from Hungarian itself, there is no doubt that those views that approach these linguistic elements from other sides have no credibility." (ibid., p. 50)

Benkő then explains that despite the brilliant results of Ural-Finno-Ugric linguistics, it is obvious, "... that linguistic reconstructions concerning the ancient Hungarian language can be carried out not only from the Finno-Ugric side, that is, looking forward in time, but also looking backward in time based on later Hungarian linguistic material, and in fact the latter are much more certain due to the fact of their own linguistic identity and the supportability of linguistic memory."
"(id.m.p.50; emphasis by Benkő)
These clear, unambiguous statements can be supplemented by the fact that the organic and massive Hungarian correspondences of the known, concrete Sumerian linguistic material make the need for theoretical reconstruction unnecessary in most cases: here, there is indeed "linguistic memory supportability", namely, going back 4-5000 years,
On the other hand, if we follow Benkő's train of thought carefully, we must notice that, although implicitly, he is ultimately saying the same thing that we ourselves established in Chapter 1 of this book: the lexical and other elements of the Hungarian language, which has relatively old linguistic monuments, a developed, rich vocabulary and a mature language structure, can be explained much more reliably from Hungarian itself.

327
===== PAGE 522 =====
starting from the current linguistic status of Finno-Ugric peoples, who are practically without linguistic heritage.
Unfortunately, with this introduction, Finno-Ugric studies were a good hundred years late. In the meantime, the dogma of "our Finno-Ugric origin" had developed and become so solidified that - as we will see below - Benkő himself could do nothing with this recognition than to try to explain even Hungarian words that have no Finno-Ugric parallels as "Finno-Ugric heritage",

The situation is similar with our loanwords, which we will deal with in more detail in the following chapters. Here and there we note that Benkő's above double theorem must of course be applied in the examination of these as well.
That is, all those words that were declared to be of one kind or another borrowing in the 19th century, which have obvious
If there are Hungarian etymologies or extensive word families in our language, we need to carefully re-examine whether they are truly foreign words.
By the way, Benkő also emphatically points out this question: how outstandingly Among researchers of our country, it is unnecessary to argue that the Hungarians' 'y'- culture
important testimony of the vocabulary is in its reconstruction. In this regard, however, it is customary to pay attention only to the Ural-Finno-Ugric, Old Turkic and a very small and largely uncertain Iranian layer of the Hungarian vocabulary. This is, however, a very narrow cross-section, in which the point is not only that a not insignificant proportion of the etymologies included here still have more or less bloody throats from the Hungarian linguistic point of view, so much so that conclusions about ancient civilization based on linguistic origin and the chronology embedded in it should not always be based on them." (ibid., p. 51; emphasis by Benkő)

Certainly, most of these etymologies are indeed "bloody throat". We recall, for example, the alleged Iranian origin of our word "vár" and in this connection that the word "vardana" in the Iranian language has the meaning of "umwallte Siedlung" (enclosed settlement), but on the other hand there is no trace of cities in either the Avesta or the Rígvēda (I. Altheim-Stiehl: Geschichte Mittelasiens im Altertum; and our book II, p. 382). The Sumerians (encircled, walled), and the Sumerians -- in the ancient Iranians -- certainly had cities. Not a few. Moreover, their 6 cities were the first cities on Earth. So it is obvious that the The word "bar" means "to enclose, to surround"
ultimate source of "vár" is the Sumerian "bar", with which it is largely identical in phonetics and meaning, while the Iranian "vardana" has already undergone a long development, that is, it has undergone a stem expansion to become
"gyar", i.e. it is the result of much later word formation.

However, does the above Benkő quote also reveal that Hungarian linguistics and prehistory research still works with the methods of "linguistic paleontology" today, considering it the ultimate scientific achievement? It is unnecessary to argue that the reconstruction of the ancient culture of the Hungarians is

===== PAGE 523 =====
"The fact that linguistic paleontology, even by Indo-Germanic studies, which has incomparably more and especially much older linguistic monuments, has long been labeled unscientific and rejected on theoretical and methodological grounds (I. pp. 417-423), does not bother our linguists and paleontologists in the least. They continue to stand by it unwaveringly, since it is possible to blow such beautiful soap bubbles with it..." (Cf. Kronasser's opinion on linguistic paleontology: 1. pp. 420-422).

However, the sourness of Finno-Ugric scholarship only follows when Benkő presents the practical application of the theoretical and methodological requirements he himself established above through a concrete example. In his opinion, its specific features provide us with a multi-level handle for distinguishing Finno-Ugric-Hungarian-type word elements..... words have structural, functional and positional meanings within the language system on the one hand and foreign-type ones on the other, regardless of their specific etymology, and they also help to chronologize their origin or at least their approximate age, essentially without regard to their specific linguistic origin and their first temporal datability. For example, csel, with an otherwise unknown etymology, means "trickery, trickery of the enemy, etc." Based on the Hungarian linguistic historical analysis of our word, it is highly probable that this part of our vocabulary — despite the lack of cognate linguistic correspondence — can be classified as a Finno-Ugric-Hungarian development..." (id.

m. 52. old.
In Hungarian, Benkő - in direct contradiction to his methodological principles clearly laid down earlier - believes that he can apply his above-quoted theoretical requirements in practice, completely detached from all reality, playing a sterile linguistic game in a vacuum: regardless of specific etymons, regardless of specific linguistic origin or linguistic memory data, and even despite the lack of cognate linguistic agreement, according to him, it is highly probable based on linguistic historical analysis that, for example, our word "csel" belongs to the Finno-Ugric developments of the Hungarian language. However, the objective observer would much rather ask, "What kind of linguistic historical analysis?" One in which neither a specific etymology, nor a specific linguistic origin, nor linguistic memory data, nor a cognate linguistic parallel is available to the analyst? What does Finno-Ugric linguistics "analyze" in this case? Obviously the great "Nothing". The whole "method" is therefore nothing more than a deception disguised as scientific phraseology.

y It is precisely this gaping chasm — which constantly appears between the highly proclaimed scientific principles of our exclusively Finno-Ugric linguistics and its everyday practice — that leads many to believe that

529
===== PAGE 524 =====
to research the questions of the origin of the Hungarian language with new methods and in other ways. In particular, we could take "cheat" (and its counterpart, "cheat") 65616008 for example, the following is really
into account the specific "clues" found in ancient Sumerian linguistic monuments, especially since the "cunning" is a word of unknown origin, and as such it is still used in Finno-Ugric

According to the methodological criteria of language, it can rightly be considered ancient (cf. Bárczi's statement on words of unknown origin, quoted on p. 526). We have seen that "analyzes" into Finno-Ugric. However, the antiquity of the word is also indicated by the fact that a) it practically forms a twin word with its deep-toned counterpart, "csal", and b) nomen-Benký is also verb-like on this basis.

According to the TESZ "... in antiquity, it most often appeared in the phrases cselť hán, cselť vet, so it was mainly a hunting and military expression." There is also an explanation that that its original meaning was "loop, net", but this is not sufficiently justified." Let's look at some Sumerian words next. zal" = "cover, cover up, secret" (cover, cover, cover, secret); cf. also "zal, zalaq" = "glänzen, leuchten" (shine, shine) and Magyar. glimmer. "Sal," = "verdunkeln" (obscure). The semantic connection is obvious: a "trick" is something secret, not something that is tied to a tree or a branch; but it is also something that is concealed, covered, covered, wrapped, obscured, that is, camouflaged, in order to mislead and deceive the enemy or the game intended to be shot. "gil" = "wickeln, verwickeln" (to twist, to coil, to tangle). Here too, the semantic parallel is close: "cunning" confuses, confuses, entangles the enemy, just as the noose wraps itself around the animal; cf. "cunning" and "breaking, throwing a noose". Here, there would be the obvious native language connection, which the TESZ does not consider sufficiently justified. "zil," = "hell, licht sein" (to be bright, shining); cf. glinlog; thus the high and alternation of deep vowels can already be detected in Sumerian (1. above , zal, zalaq" = glittering); = "gut, rein, fromm, freundlich" (j6, pure, gentle, friendly); cf. gentle. However, the wedge sign of this word can also be read with the sound value "tag", the relevant meanings of which are as follows: "tag" = "jagen" (to hunt); cf. quiver (shoots an arrow, directs it at a target) and célož; thus, the phonemes "tag" and "zil" also have their corresponding parallel synonymous versions in Hungarian, (According to our linguists, "céł" is a German loanword, but it is not found in any other Indo-Germanic language except Germanic; I, Kluge: Etymol. (Wörterbuch der deutschen Sprache, under the heading "Ziel, zielen". This circumstance, in any case, makes the German derivation of the word at least debatable.) ===== PAGE 525 ===== = "to cut down from a tree, to strike down, to throw down" (fát dönt, le-ver, leüt); vö, taglöz. = "drehen, weben" (spin, word); cf. reel, weaver (the latter is a Slavic loanword according to our linguistics). The most ancient trap was a loop and net woven from plant fibers: thus, the semantic connection, which according to TESZ is "not sufficiently explained", appears again. So, with "csel" can be applied to the basic concept of Sumerian, "fogódzók". We emphasize that we did not select the word: we only looked for possible Sumerian parallels in Deimef's "Sumerisches Lexikon" after reading Benký's above discussion. The Sumerian words presented, without any concrete etymological derivation or derivation, we merely wanted to point out, as it were, "ad hoc" what opportunities a closer examination of the Sumerian language would offer for Hungarian linguistic history research, if it were thoroughly dealt with.

This short compilation also illustrated that wherever we look into the Sumerian dictionary — looking for possible connections to a given Hungarian word — other Sumerian-Hungarian phonetic and semantic parallels, and not infrequently even complete correspondences, emerge. However, here we have only presented excerpts from other parallels that emerged while searching for possible Sumerian equivalents of the word "esel" (csinlog, szelid, tegez, 06102, taglöz, teker, takács).

However, we could also notice a part of that inestimable linguistic treasure. bottleneck, which lies for us in the Sumerian language. To prove that the above examples do not really exhaust all the possible Hungarian linguistic parallels of the five Sumerian words in question, we present further Hungarian contexts of the single word "tag": stag" = "zurücklassen" (leave behind, abandon, abandon); cf. teg-nap; "the abandoned, abandoned, abandoned day"; = "entlassen, Scheidung" (dismiss, separation); cf. ki-nagad, meg-nagad; "schmücken" (decorate, embellish); cf. "ein schmuckes Mädchen" = "a pretty girl"; "to rise, to grow" (megkel, nö); vö. dagad; "Hand" (hand); cf. limb; "pflanzen" (plant, i.e. plant); cf. dug; "errichten, zusammenbauen" (to prepare, to assemble); cf. tackle; "schlagen, stossen" (to hit, to beat, to push, to stab, to stab); cf. ýýýýý, in

Here are 12 Hungarian words that are closely phonetically and semantically parallel to the multi-meaning Sumerian word "tag". For this reason, it will be very instructive to present the derivations of TESZ:

531 ===== PAGE 526 ===== Ugric, Finno-Ugric 2 (dug, quiver) Hungarian internal derivative 1 (teg-nap) Slavic 5 (tag, deny, tag, deny, tackle, etc.) 3 (bulging, lumpy, round) unknown So, 8 words are of unknown and uncertain origin (66.7%), 2 are Ugric-Finno-Ugric (16.7%), 1 is the result of internal Hungarian development, and 1 is a Slavic loanword (8.3% per word). However, after seeing that the listed words obviously belong without exception to the semantics of Sumerian "tag", that is, they form an organic unit, the question arises: since even according to the definition of Finno-Ugric linguistics, 66.7% of them are of unknown and uncertain origin, which can be derived from the Sumerian word in question, what makes the remaining 33.3% partly Finno-Ugric, partly Slavic, or a word of internal origin? The evidence of specific Sumerian linguistic monuments — but also rationality — requires that we consider these words only in their entirety, and not in a disorganized way.

A few words about the sound development of the Hungarian language and our early linguistic memories Within the framework of the general theory of language development of "linguistic Darwinism", which has prevailed in European linguistics for more than 100 years, our linguistics also starts from the regular, temporally observed sound changes, language structure and grammatical development of the Hungarian language in its linguistic historical studies. However, to support this linguistic development, they can only refer to one or two early linguistic monuments, primarily the Hungarian fragments of the founding charter of the Tihány Abbey (1055) and the Funeral Oration (around 1200), because their phonetics and word combinations are indeed quite far from today's Hungarian speech at first glance, while in other linguistic monuments we find words with phonetic forms and grammatical structures that are largely in line with today's Hungarian language.

These two linguistic monuments are undoubtedly of paramount importance in assessing the development of the Hungarian language. It is therefore necessary to look at them a little more closely and find out whether they truly clearly reflect an earlier state of Hungarian language development, as our linguists interpret them. It will be instructive if we examine these linguistic monuments not only in isolation, but also in close connection with the interpretation of Finno-Ugric linguistics and the conclusions drawn from them. In this regard, Géza Bárczi 532 We start from the findings of his book "The Biography of the Hungarian Language".

===== PAGE 527 ===== First of all, we must emphatically point out the fact that our linguists consciously distort the words handed down to us from these linguistic monuments, obviously in order to adapt the sound characteristics of the Old Hungarian language to the sound developments they assume, especially the Vogul-Oszjak and other Finno-Ugric sounds that are missing from modern Hungarian. For example, Bárczi transliterates the expression "ohut kuta rea" in the Tihány Founding Letter — without any explanation, completely arbitrarily — with the phonetic form "oghut kutá reá", and "noqu azah fehe rea" as "nogyu aszagh feghé reá", undoubtedly in order to "prove" the assumed "gh" sound in Old Hungarian. However, since this alleged "gh" never occurs in the fragments of the Tihány Founding Letter, Bárczi only "proves" with these readings that linguistic research from a Finno-Ugric perspective works with unmethodical and impermissible means: it falsifies the concrete linguistic facts in the interest of a pre-conceived hypothesis.

In the case of the Funeral Oration, which he also repeatedly quotes to prove the development of the Hungarian language, Bárczi also deliberately distorts it, and for example, he quotes the words "vilagbele" and "nugulmabeli" written together in the original text as "vilag bele" and "nugulma beli", i.e. written separately, in order to prove that the present-day suffixes were once nominative suffixes. It should be noted, however, that the editor of the Funeral Oration also writes the nominative suffix "giggeld", which is still separate today, together with the noun preceding it, and omits the "-t" ending: "vimadsagucmia" (because of their prayers). We believe that this is a clear sign that the scribe was unable to distinguish between Hungarian suffixes and nouns, but at the same time that he did know this: what is expressed by a Latin or German preposition must be written together with the noun in Hungarian,

In addition, however, the ... into it? and ... The completely meaningless mixing of the words "beli" and "belemyg" also indicates that the author of the Funeral Oration was not a native Hungarian speaker: "belemyg" is not same as "talubeli". The scribe was obviously familiar with such word combinations, but could not distinguish them precisely from each other. This — and not the use of suffixes as nouns — is also supported by the fact that, as we have already mentioned, he writes the suffix "mi- att" with the corresponding noun. 566, the words "milostben" and "paradisumben" also clearly prove that the Articles were suffixes and not nouns even at the time of the composition of the Funeral Oration, since in the latter case we would only expect the forms "milost benne" and "paradisum benne". The same spelling runs throughout the entire Funeral Oration: "gimilsben" (and not "gimils benne"); "gimil- snec" (instead of "gimils neki"), etc. There is therefore no question that at the time of the creation of the Funeral Oration, today's suffixes were still nominatives,

It should also be noted that the subject is missing from the second sentence; "How much mercy has already been bestowed upon you, and what is your name?"

===== PAGE 528 ===== Did he save our 6th Adam from the beginning?! A Hungarian-speaking scribe could hardly have made such a cardinal intellectual error. But the structure and verb conjugation of the third sentence are also typically foreign (German or Latin): "... gimilcuctul munda neki elnie"... he told him to live from fruits). Cf. "von Obst sagte ihm zu leben". On the other hand, Hungarian means "live with fruit" and not "live from fruit", and "élnie" is a servile translation of the German "zu leben" or the Latin "accusativus cum infinitivo". Below: "God was angry and threw him into this laborious world." Compare: "God was angry and threw him into this laborious world" (hineinwerfen = to throw into; "hinein" is a participial conjunction in German).

On page 70 of his book, Bárczi again uses the Death Speech to prove the development of the Hungarian language. He writes: "Even by the end of the ... The subject and predicate must agree in number ... Old Hungarian period, it had not yet been fixed in every case (for example, in the Death Speech we find the following sentence: mend ozhuz iarov vogmuc, literally translated into modern language: we all belong to it...).".

And we again point out the obviously blatant Germanism of the sentence: "Wir sind alle 50777 (we are all full); "wir sind alle wütend" (we are all angry); — and not "satte" (full) or "wütende" (angry), because in such cases German does not pluralize the adjective or the adjectival participle! We must therefore emphasize again: the Funeral Oration — without a proper, thorough critical review — is not suitable for drawing conclusions about the development of the Hungarian language, because there are very compelling reasons to suggest that its editor was not a native Hungarian speaker.

But the conclusion that Bárczi draws from the sentence of the Funeral Oration quoted above is also very characteristic. According to him, the numerical agreement between the subject and the predicate had not yet been definitively established at that time because at that time the "expansion of the content of consciousness", the "development of thinking" had not yet reached the stage at which "... The narrow scope "...a loose, one-to-one correspondence of a small number of concepts that flashed in consciousness was replaced by a slowly expanding... content of consciousness in which a larger number of concepts were grouped into a close logical relationship..." (ibid., p. 70).

It is a pity that Bárczi's book was not translated into German. The Germans would certainly be grateful to him for this characterization, because their language has not yet reached the stage where "the few concepts that flash in the narrow consciousness... are grouped into a close logical relationship."

However, we cannot accept such nonsense neither among today's Germans nor among 12th/13th century Hungarians, because Bárczi's statement is biological absurdity.

It directly contradicts the generally accepted results of anthropological, ontogenetic and phylogenetic research, according to which the capacity of the human brain, the depth of its consciousness,

534

===== PAGE 529 =====

His abstract and logical abilities have changed practically nothing over the last 5-6000 years. According to the findings of evolution, at least 100,000 years ~ if not more ~ are needed for the development of brain function to the extent that Bárczi assumes in the 7-800 year period,

In Hungarian, all this means that, according to Géza Bárczi, the Hungarians in the 12th century AD (and the Germans even today!) were at the level of cerebral development of the Neanderthals. This is what brings our linguists to, a completely unreasonable attempt to examine and evaluate the phenomena of the Hungarian language based on Vogel.

The lesson from this is that once again, the detachment from all reality of linguistics with an exclusively Finno-Ugric perspective is undoubtedly revealed, as is the deplorable one-sidedness of its practitioners, who, in the interest of the dogma of the Finno-Ugric origin of our language, cast aside even the most basic requirements of logic. Moreover, they often express sovereign expert opinions on issues about which — to use Bárczi's own words — they clearly do not even have the few concepts that flash through their narrow consciousness. This is clearly proven not only by Bárczi's "biological" argument above, but the same shocking superficiality is also evident, e.g. Also in Róna-Tas, when he speaks of "identical, different-sex twins" (1. Róna-Tas: A nyelvrokonság, p. 405). Identical twins can never be different-sex! This fact is one of the primary fundamental laws of biology, and it would have been appropriate to hear something about it in high school.

After such experiences, we should not be surprised that the tone of the anti-Finno-Ugric publications does not correspond to the standard generally accepted in scientific debates. If a linguistic school puts forward such absurdities to prove its theses, then one can only assume that this is not an objective search for scientific truth, but merely the stubborn defense of an outdated 19th-century a priori assumption at all costs. Bárczi's above-quoted concept is exactly the same as the 19th-century Indo-Germanic conception of the conquering Hungarians as "wild barbarians". The special piquancy of the matter, however, is that Bárczi - in his eager effort to prove this typically Indo-Germanocentric principle - completely forgot that with the above reasoning he also dragged the Indo-Germanic German language into this "wild barbarian" view.

Let us now look at some of Bárczi's further arguments proving the development of Hungarian sounds. On page 130 of his book, he states bluntly that the characteristic deep, lip-rounded Hungarian a" sound only developed in our language in the 11th-12th centuries, and during the time of the conquest we only knew the open Indo-Germanic w.

533

===== PAGE 530 =====

This assumption, stated in the declarative mode, as if as a fact, cannot be proven by anything in the world, since our old texts do not know the accents. But in addition, it directly contradicts the indisputable fact that all the surrounding peoples only know this open "4" sound, and the deep Hungarian "a" is mostly not even pronounced by our neighbors, most of the time they pronounce it as "o". Consequently, since at the time of the conquest we would have only this open "a" sound, the characteristic Hungarian "a" could obviously never have developed from this in a purely Indo-Germanic environment. However, the believers in Finno-Ugric origin do not raise such considerations. For them, the only important thing is to somehow adjust the Old Hungarian sound system to Vogel. Since this open "á" sound occurs in the Obi-Ugric, "we are forced to assume" - they say - that the conquering Hungarians also had it. This "we are forced to assume" argument then implicitly becomes a tooth-and-claw reality in the literature, as we see in Bárczi. The entire evidentiary apparatus of the Finno-Ugric derivation of the Hungarian language is built on such vicious circles.

Likewise, in the case of consonants, we must accept with legitimate skepticism the assumption that at the time of the conquest, the sounds , c", "zs" , ty", "v" would not have yet appeared in Hungarian, while typically Indo-Germanic sounds, such as the English , , ch", , gh", and even "chj", were supposedly present in our language (1. Bárczi: id.m. w" felvokális, pp. 136-139).

Mar Bárczi's first example — with which he claims — He tries to prove the absence of the "c" sound — he limps very badly. Since the Slavic word "nemici" has the "c" sound. — "We adopted it in the German" sound form," he writes, "this that there was no "Slavic enemy" in Hungarian — we can provide the following additions to the question of the alleged adoption of the word:

1. In Noricum (approximately the territory of present-day Austria) before the Roman conquest, a Celtic tribe called NEMET can be dated from the 1st century BC (1. Koch-Jungwirth: 2000 Jahre Geld in Österreich).

2. Pliny mentions a Germanic tribe called "Nemetis" in the 1st century AD.

3. One of the Germanic corps of the Roman army was called "Nemetis" (1. Altheim: Geschichte der Hunnen, vol. IV, p. 106).

4. In Roman times, the city of today's Speyer was called "Nemetis". This city was the seat of the "Nemetis" tribe (1. Altheim: id.m. ibid.).

From all these sources it follows that the Hungarian language

The word "German" is obviously not a Slavic loanword, but a Hunnic-Avar word.

or Pannonian-Bulgarian mediation, and both today's

preserved the original NEMET sound form much more faithfully than the

536

===== PAGE 531 =====

distorted Slavic "nemici", i.e. it cannot serve as evidence that the Hungarian language at the time of the conquest did not have it. As for the Indo-Germanic type of double and c" hang.

even triple consonants, in the case of "gh", we have already shown above that Bárczi is demonstrably falsifying when he consistently transliterates the simple letters "h" in the original texts as "gh". Thus, in addition to the examples already mentioned, the Tihany Founding Letter also reads the word "meneh" as "menegh".

The situation is similar with the other alleged double mass vowels. These are either spelling fluctuations — in our language, similarly to other languages, there was no generally accepted spelling convention until the 19th century — or phonetic spelling, as for example the form "szoboduchja" (liberate), mentioned by Bárczi as evidence for the existence of the "chj" sound, is obviously nothing more than an attempted phonetic rendering of the "ccs" sound heard when pronouncing the word: "szabadiccsa".

Incidentally, the assumption of this "chj" sound — which Hungarian cannot even pronounce — is again urgently needed because such sounds occur frequently in Finno-Ugric languages: for example, the infinitive suffix in Vogul is "-chv".

Tórnási. Well, Bárczi is "s" became — writes Bárczi on page 139. In Bizonyítékal, Emperor Constantine was inspired by him in the form of "Almucs", "Termacsu". "cs" often, according to him, only later became Ámos and "read" names, from which the "s" (5) manipulating here too: he did not replace it with a letter in Greek, the pronunciation of which was "z" or "dz" — and not "cs", as Bárczi writes. That is, sound had to be written somehow. Emperor Constantine used this "zeta"

Constantine's "dz" or "27-[6]" can in no way prove a "cs" sound transition in Hungarian, or even "dz" > "s", just as, for example, "Gatte-hose" (gatya) that was transferred to the Austrian regional dialect cannot be evidence that "gatya" would have sounded like "gatte" — 5"

in the past. These are words written with phonetics foreign to the Hungarian phonetic order, so let us not look for the "language of Hungarian development" in such words. Our opinion is only strengthened by the fact that the text of Emperor Constantine's work in two versions: Ertim that has survived to us has been greatly corrupted. The same word, more than once occurs in two different phonetic forms. For example, when the Pecheneg tribes are repeatedly mentioned, we find the names of several tribes and Ertim, Csopon and Csozpon, Kalpeé and Koulepeé, etc. It is quite obvious that we cannot build Hungarian "language development" theories on such a corrupt text.

By studying Bárczi's book from this perspective, it can be clearly stated that the Hungarian sound development theory of Finno-Ugric Hungarian linguistics is largely based on the a priori assumption of the last century that the analogy of Hungarian word parallels found in the languages of the 19th century Voguls and Ossetians — since according to them our language is Finno-Ugric,

537

===== PAGE 532 =====

more specifically, they postulate a completely undeveloped, rudimentary, primitive Hungarian language, derived from the "Ugric" basic language, immediately before and during the conquest, and they try to present the matter as if the Hungarian language had only reached the level of development at which it was indisputably already in the 15th-16th centuries in the centuries following the conquest - under the influence of the advanced languages of the Germans, Slavs and other "educated European peoples".

However, they forget that with this assumption they themselves make their other claims very unlikely, first of all that the language of the conquerors of 895 assimilated the languages of the peoples found here. A language that was so primitive, still in a so-called liquid state, and in the process of formation, as the Hungarian language of the time of the conquest is depicted as, would have been doomed to die out in a completely different linguistic environment and as a result of the large-scale and continuous foreign admixture that occurred after the conquest of the new homeland, or at least would have lost its specific agglutinative type and would have developed a language fundamentally different from the languages of the neighboring peoples.

characteristic language and sound system. On the other hand, in 1100 years, we have not adopted a single suffix, suffix or other grammatical or structural element from the surrounding peoples, which indisputably proves that even at the time of the conquest, our language had a mature, established, developed vocabulary and language system similar to today. This is also confirmed by the fact that our language, in turn, has influenced the languages of the peoples directly neighboring the Hungarian-speaking area to a certain extent:

A so-called Danube, Carpathian Basin areal language association-like formation can be detected, which primarily covers phonology, but also some language structural elements, including, for example, the characteristically Hungarian deep "a" sound, which our linguists claim only arose in the 11th-12th centuries. Therefore, the views of Finno-Ugric linguistics on the "primitive" Hungarian language at the time of the conquest cannot be accepted for this reason alone. But not because it has been proven beyond doubt that the level of education and social development of the conquering Hungarians, who supposedly belonged to a primitive language and were also considered completely uncultured, was in no way lower than the cultural level of the surrounding Central and Eastern European peoples at that time. This has been a well-known fact that has been archaeologically and historically proven for decades, but Finno-Ugric linguistics alone does not wish to acknowledge it. Which, however, once again provides excellent proof of its objective scientificity, In general, it is also striking and characteristic that most of the phonetic, inflectional, declensional and other differences of our old texts from the later Hungarian language can almost without exception be traced back to conspicuous Germanisms or Latinisms, 538

===== PAGE 533 =====

In addition to the above, the rarity of the subject suffix can just as easily be explained as a mistake by scribes whose native language is not Hungarian, or who have used the Latin language of church institutions since childhood, and cannot therefore serve as evidence for the assumption that the subject suffix only developed later. It is well known that Germans who learn Hungarian do not use the objective case for a very long time, because their sense of language does not require it. In German, there is only a "symbolic" objective case, 62 only in the singular of masculine words, expressed by changing the article (den), while the noun remains unchanged. But in Latin, the objective case is also rudimentary, especially in neuter words, and it is completely absent in the plural.

Likewise, the frequent omission of the definite article in our linguistic monuments is also understandable without further ado, if we know that Latin does not know the definite article, and that in our codices -- as Bárczi also writes (1. id. m. p. 149) -- in many cases we can undoubtedly establish a servile, literal translation of the original Latin text, and for example, the consequence of this is the intransitive use of the object dependent on the adverbial participle in our medieval frasbeles.

However, Bárczi is not entirely right on this issue either. On page 150, he states bluntly that the Funeral Oration does not yet know the definite article. On the other hand, the definite article occurs several times in the Funeral Oration, of course not in the modern sense. For example: "ki napon emdul oz gimilstvlt" (on which day you eat from the fruit); "es in the form of "a", but was used alone until the 19th century in the "form".

evcc oz tilvlt gimilstvlt es oz gimilstben halalt evcc" (and he ate from the forbidden fruit and ate death in the fruit); "And the fruit's water was so bitter." In all these cases, our linguists explain that "that certain" fruit is to be understood as "that" fruit, and the sentences are much more meaningful when explained with a definite article than with a demonstrative article. But on the other hand, since the Hungarian definite article is ultimately nothing more than "oz", so oz" a metaphorical variant of the demonstrative pronoun "that", we can rightly reject this would be a demonstrative pronoun. However, this interpretation is not at all necessary, because as we have seen, the hair-splitting distinction!

The validity of all the arguments that we have listed so far to justify the high degree of caution that is absolutely necessary in the linguistic historical evaluation of the Hungarian language monuments of the 19th century is strongly supported by Bárczi himself when he speaks of the Latinisms that abound in our medieval codex literature (id. m. pp. 168-170). Moreover, he also admits that, in contrast to the language of the codes written in the spirit of the Latin language, our modern language preserves the ancient traditions much better. Finally, he states that the medieval written Hungarian language was almost exclusively a language of translation

539

===== PAGE 534 =====

"was" and "so many Latinisms penetrated it, and even became its rules... that the language of writing was thus separated from the living Hungarian language." (id. m. p. 189) Yet he applies this to our late medieval codex literature, when the editors and translators were already Hungarians, and lo and behold, under the influence of Latin-language schooling, even these too were separated from the spoken Hungarian language. In Hungarian, this means that even these late Hungarian codices, saturated with Latinisms, cannot serve -- without appropriate, thorough criticism -- as a solid basis for any Hungarian "language development" or "phoneme development" theory. And if this is the case even with our later codices, how much greater caution is needed in the case of the earliest linguistic monuments, which, moreover, can be assumed to be the works of scribes who were not native Hungarian speakers in all cases. However, our linguistics bases its assumptions about language and phonetic development exclusively on these linguistic relics written in fluctuating orthography, imbued with foreign sentence constructions and verb inflections.

In addition, the alternations of letters are also arbitrary in these early writings, lacking any rules. Especially in the Funeral Oration, the pronouns , bele", , bell", "ben", or the suffixes or German-style verb endings -- who sentence once "vogmuc", could distinguish them exactly -- are completely irregular, most obviously: in the mixing of a; the "u", v", "w"; the ya, 5075 the "c", "s", and the no less irregular use of the letters "u", "e", , 1 (e.g. within a another time "uogmuc"; once "gimilc", then "gimils", etc.). Similarly, the letter "z" is sometimes "Zz", another time because the Latin alphabet does not have a letter symbol for many Hungarian sounds, and so the scribes had to resort to sound substitution. "sz" denotes a sound, etc. etc. The obvious explanation for all this is that

If we examine these phonetic, sound substitution and spelling fluctuations with due criticism, and at the same time take into account the blatantly foreign verb inflections, sentence constructions, and the apparently completely hesitant, almost haphazard application and confusion of the affixes required by vowel harmony, and together with all this we exclude other Germanisms and Latinisms that can be found, then it can be clearly established from the text thus sifted that the language of the time of the Funeral Oration differed, so to speak, hardly at all from the Hungarian vernacular of the recent past. Let us think, for example, of the use of valo, which still sounds the same in the vernacular today: "mia". "because of" noun in the form "mia"

The change, the so-called "development" of our language in the last 800 years has therefore not even remotely reached the level that characterizes, for example, modern German compared to Old High German (even educated, intelligent Germans cannot understand an Old German text!), and which our linguistics -- by analogy with the German language -- believes it can forcefully deduce from the Funeral Oration.

540

===== PAGE 535 =====

Chapter 4
OUR MINOR WORDS DERIVED FROM INDOGERMAN LANGUAGES

General methodological notes

In András Róna-Tas' book "Language Affinity", we can read that approximately 68% of the root words of the Hungarian language are loanwords (ibid., p. 234).

We are astonished by this huge mass of loanwords, and we must ask ourselves the question: if this were really true, to what extent can we speak of an independent Hungarian language at all? But the question that arises with equal force is how it is possible under such circumstances that, although we borrowed more than two-thirds of the root words of our language from foreigners, we were able to preserve, in one block, the unique grammatical, structural and phonological characteristics of our language, which are almost unique in Europe, and its special features that cannot be compared to any neighboring language: its admirable conciseness, flexibility, and remarkably developed internal linguistic logic. And finally - but not least - how can we explain the surprising, almost unprecedented phenomenon in all of Europe, that the Hungarian language has managed to remain amazingly unified, without any significant dialectal differences, despite the relatively large extent of its language area (there is a distance of 1,000 km between 1936 and Székelyföld!), centuries of historical isolation (the Turkish occupation virtually cut the Hungarian language area into three parts for 150 years), and the continuous mass influx of foreign-speaking ethnic elements over the centuries?

Hungarian dialects cannot be compared with the dialects of other languages, which in many cases differ to the point of incomprehensibility, and can often be considered as small separate languages themselves. This is true not only of the Indo-Germanic languages, where, for example, the Alpine peasant from Vorarlberg no longer understands Salzbürgian and vice versa, or the Catalan from Barcelona Castilian, but also of the related Finno-Ugric languages belonging to the same language type as Hungarian, where, for example, the 20,000 Ossetians use three main dialects -- in addition to several smaller ones -- that are so different that, despite all the efforts of the Soviet official circles, it has not been possible to develop a unified literary language that all Ossetians would understand (see Antal-Csongor-Fodor: Languages of the World, p. 120).

According to this, the typological structure and agglutinative nature of our language, taken in itself, do not even remotely explain the monolithic uniformity with which Hungarian stands out among other languages of Europe.

===== PAGE 536 =====

gaslik. We must therefore look for another reason. If we take into account the conciseness, flexibility of our language, the clear, precise articulation of sounds, the brevity of the word roots, and the simplicity of their sound structure, then we must conclude that all of these testify to long-established, very ancient linguistic foundations. The same is also proven by the fact that Hungarian, even at the time of the conquest, had come very close to its present-day linguistic state and has hardly changed for 1,100 years -- or as our linguists say, "developed" -- although many new languages have arisen in Europe as a result of the differentiation of certain basic languages or surrounded by languages with the separation of related languages" (I. L. Benkő quoted his lines in a little more time, pp. 524-525), and at the same time in our Carpathian Basin country, where for 1100 years we have been completely different structural structures and internal logics, we have not taken over a single foreign formative or grammatical element from our neighbors, while they have taken over quite a few from us. However, according to the new recognition of international linguistics, languages borrow not only words -- as was previously thought -- but also grammatical and linguistic structural elements, suffixes, and formatives. As is clearly demonstrated by the Hungarian language structure features found in the languages of neighboring peoples.

All these circumstances indisputably indicate that we must accept with great skepticism the opinion of our linguistics that 68% of our root words are loanwords. Let us therefore examine more closely how this view was formed -- but let us initially remain within the framework prescribed by Finno-Ugric studies, assuming for the time being that our language derives from the Finno-Ugric basic language. For thus the one-sidedness of our linguistics becomes all the more apparent, the more clearly it becomes apparent that our linguistics does not even apply the methodological requirements it has established for the examination of our loanwords.

In the second half of the 19th century, the theory of Indo-Germanic cultural priority was in its heyday. In Book I, we have already shown that every root word found in at least 2-3 Indo-Germanic languages was declared to be of ancient Indo-Germanic origin. Loanwords -- that is, that the Indo-Germans might have borrowed a few words from other languages -- were almost out of the question. At that time, we saw that even O. Schrader -- who was one of the more reasonable researchers -- recognized only a dozen or two Semitic loanwords in the Indo-Germanic languages (Book I, p. 64). The generally accepted opinion was that the ancient Indo-Germans, who were highly educated, had no need for loanwords.

In such a scientific, methodological atmosphere, the systematic etymological examination of the words of the Hungarian language also took place, similarly in everything else.

546
===== PAGE 537 =====

To Ahlquist cljárāsā, who - as already mentioned in our Book I (p. 63)
tük — all those words of the Western Finnic languages, whose parallels he found in the Germanic or Baltic-Slavic languages, he declared a priori to be Indo-Germanic loanwords. Moreover, he did not consider the remaining Western Finnish vocabulary as an ancient heritage, but even discarded those words from it that had no equivalents in the more preserved Eastern Finno-Ugric languages — primarily Vogul and Ossetian. In other words, he worked with exactly the same method as if, for example, Indo-Germanic linguists included in the original, ancient vocabulary of the Indo-Germanic languages only those words that are also found in, say, modern Afghan or Albanian.

Let us pay close attention to this circumstance of paramount importance: not only Hungarian, but also the Western Finnic languages were examined primarily in relation to Vogul and Ossetian, i.e. the languages of the two most developed and most educated peoples of the language family, which together account for more than 80% of all Uralic peoples, and as a result, they were lumped in with the 26,000 Obi-Ugric languages living in the conditions of the Kok period.
This "method" of linguistic research was based, on the one hand, on the obvious assumption of Indo-Germanic cultural priority, and on the other hand, on the fundamentally flawed assumption that the linguistic memories of the culture of the people speaking the Finno-Ugric basic language are more reliably preserved in Vogul, spoken by primitive fragmentary peoples, and in Ossetian, than in Finnish or Hungarian.
In this regard, we recall the lines of Gábor Bálint quoted above: to explain and to lay a scientific What could be said about someone who wants to make Greek from Albanian and Sanskrit from Afghan? foundation, since we are relatives..." (1. p. 405)
László Szabédi also systematically refutes this erroneous belief with equally thorough reasoning, referring to specific linguistic parallels: "...it is obvious that the culture of the people speaking the Ugric or Finno-Ugric basic language cannot be inferred based on the original vocabulary preserved in the languages of the Voguls or the Ossetians and other fragmentary peoples, just as the culture of the Roman people speaking the Latin basic language cannot be inferred based on the original vocabulary preserved in the Megleno-Romance language." (Szabédi: The Ancient History of the Hungarian Language, p. 520)

However, the great figures of Hungarian-Finno-Ugric linguistic comparison did not waste their time on such considerations, neither then nor later. Budenz established the basic principle of Hungarian linguistic research, which is still valid today, according to which all Hungarian words that have no equivalents in the Finno-Ugric languages - with particular regard to Vogul and Ossetian - can only be loanwords.

However, in formulating this confident proposition, Budenz forgot about something else besides the aforementioned exclusionary reasons. He did not think
547
===== PAGE 538 =====

nor that "... we may have any number of Finno-Ugric words that none of the related languages have preserved." (1. Bárczi G.: Szófejtő Szótár XII. - quoted in Szabédi: id. m. p. 519.) Yet this is not a mere assumption, but a fact that can be proven many times over with concrete linguistic data: The Finno-Ugrics place the cessation of the Ugric language unit at about 3000 years ago... The community of French and Romanian languages ceased only about one and a half thousand years ago, and yet the number of French words of Latin origin whose equivalents are missing from the Romanian language is extremely large." (Szabédi: id. m. p. 519-520.)

We emphasize again: all these are indisputable, concrete linguistic facts. Despite this, Hungarian linguistics, which has an exclusively Finno-Ugric perspective, not only at that time, in the 19th century, but practically to this day, considers the assumption as the basic pillar of its comparative linguistic and linguistic historical studies that we can call "ancient heritage" only those words of ours whose parallels are also found in the Finno-Ugric languages. The only "development" in this field is that recently, through rabulistic pseudo-arguments, they have also tried to "analyze" and derive our words of unknown origin from Finno-Ugric, as we saw above in Loránd Benkő (1. pp. 529-530).

Returning to the question of loanwords in the Hungarian language, starting from the false premise described above, further logical conclusions almost came naturally: a primitive community, primarily compared to the Vogul-Ostjaks, could not have been more cultured, nor could its language be more developed, than the culture and language of the 19th-century Ob-Ugrians. This is how the image of the conquering Hungarians, fishermen-hunters, "wild barbarians" with a Stone Age level of culture, was formed, who had been taught to ride a horse and a little animal husbandry by some Turkish people shortly before, and of course the conviction that such a primitive people had to take over all words that hint at higher culture from foreign peoples, primarily — needless to say — from the Indo-Germans. As O. Schrader also stated (Book 1, p. 63).

However, no one thought that the words in question, indicating higher education, were obviously absent from the 19th-century Obi-Ugric languages because these truly backward ethnic groups, living in Stone Age conditions, simply did not need them.
Well, as a logical consequence of the a priori Indo-Germanocentric basic thesis presented above, thousands of our root words were declared to be foreign loanwords at that time -- mostly without taking into account the semantic ramifications of the individual words, their word families, or their cultural and historical background to even the slightest extent. Some words with a semi-similar meaning and parallel phonetic form

548
===== PAGE 539 =====

word in one of the Indo-Germanic languages around us was practically enough for the Hungarian words in question to be included among the loanwords. After all, the eloquent representatives of Hungarian linguistics themselves beat the drum for all our words whose "Finno-Ugric origin" they could not demonstrate; the linguists of the neighboring peoples were therefore free to choose among such ownerless Hungarian words. They did not miss the opportunity: Miklosich, for example, "showed" that about a thousand of our words were Slavic loanwords - we will see below with what "method".

We are not alone in this opinion. It has also struck many, for example Elemér Moór, who is unquestionably Finno-Ugric in his approach, that our loanwords have been examined in a completely one-sided manner. It never occurred to anyone — he writes — that loanwords could have been transferred from Hungarian to foreign languages (see The main problems of Hungarian prehistory).
This view — in essence — has not changed to this day. A good example of this is our word "szablya", which, despite all the evidence of archaeology and historical sources, and regardless of our basic verb "szab", is still stubbornly labeled by the TESZ as a "migratory word" adopted through Serbo-Croatian mediation; not paying any attention to the fact that this derivation is clearly refuted by the facts of archaeological research; moreover, even ignoring Indo-Germanic studies, which have been working with much more sensible methods in the meantime, which derives the German word "Säbel" (szablya), but also the parallel words of other European languages — including Slavic languages! — from the Hungarian verb "szablya", and ultimately from the verb "szab" (to cut, to cut) (I. II. book, p. 254, or Kluge: Etymol. Wörterbuch d. deutschen Sprache, pp. 617-618. under the heading "Säbel"). |

Yet the need for a thorough revision, as in many other areas, has been increasingly pressing in the question of our loanwords for decades. Not only because archaeology and modern historical research have long since completely dealt with the 19th-century legend of the "wild barbarian" conquering Hungarians, thereby definitively removing the ground from any reference to cultural superiority, i.e. the a priori assumption that Hungarian words declared to be of foreign origin were massively adopted by other peoples due to their higher education; but also because - as we already know - recent Indo-Germanic linguistic and pre-historical research has in the meantime recognized that the Indo-Germanic languages were not spoken until quite late, in the 10th century BC. They developed during the 2nd and 1st millennia, not from a uniform ethnic and linguistic basis, but through complicated processes of ethnic mixing and language equalization, in the manner of areal languages and language alliances. However, this recognition - which is already of fundamental importance in itself - is not relevant from the point of view of our current question, i.e. the alleged origin of various Indo-Germanic languages.

549
===== PAGE 540 =====

The statement of numerous authoritative representatives of international linguistics that the so-called "Finno-Ugric" languages played a decisive role in the development of the Indo-Germanic languages (Book I, pp. 95-101 and this book, pp. 422-427) far surpasses the significance and importance of Hungarian loanwords. The eminent importance of the question requires us to present in more detail the related discussions of some renowned foreign researchers.
J. Pokorny showed in his dissertation "Substrattheorie und die Urheimat der Indogermanen" that especially Russian, but also other Slavic languages, show many Uralic, primarily Finno-Ugric features in inflection, word formation and internal linguistic forms: accumulation of suffixes, lively verb formation, rich further formation of verbs, etc. To the north and east of Moscow, this base layer is demonstrably Finno-Ugric, writes Pokorny. But the same Finno-Ugric influence can also be observed in the Baltic languages. Moreover, he emphasizes, FN Finck already in 1899- He pointed out in (Der deutsche Sprachbau als Ausdruck deutscher Weltanschauung) that, in addition to Slavic, the Indic, Iranian, Ossetian (emphasis added), Kurdish and Armenian languages also have special features that can only be explained by language mixing. We must probably also take Finno-Ugric influences into account in these, Pokorny notes.

In connection with the characteristics of strong "Finno-Ugric" (we would rather call it pre- or proto-Hungarian) language mixing observed in the Indic, Iranian, and Ossetian languages, we recall Károly Fiók's thesis on the signs of massive influences of the proto-language group he called "Ugric-Hungarian" languages in the Finno-Ugric, Altaic, and Indo-Germanic — primarily Indo-Iranian — languages. At that time, around the turn of the century, Géza Nagy, Gyula Rohonyi, and several other researchers also claimed the same thing (for more details, see the book y. yy pp. 279-280, and below, chapter 5 of this book).
According to NS Trubetzkoy (Gedanken über das Indogermanenproblem), the most ancient Indo-Germanic dialects developed gradually, through linguistic equalization, in the contact zones of the Uralic — more specifically Finno-Ugric —, Caucasian and Semitic language families, or in the linguistically immature regions between the settlement areas of the peoples belonging to these language families. The emergence of the Indo-Germanic languages cannot therefore be considered a single event that can be defined within narrow time frames, but their development is a continuous process. According to Trubetzkoy's criteria, a certain language can become "Indo-Germanic" at any time under given circumstances, but it can also lose its Indo-Germanic character due to opposite influences. He states that the sound formation of the Indo-Germanic languages depends in each case on the other-language environment in which the peoples who later became Indo-Germanic speakers originally lived. Ossetian, Armenian, Kurdish and Indus, for example, form their final sounds in three or four ways, because there are three types of articulation in their environment. The rest

550
===== PAGE 541 =====

Indo-Germanic languages pronounce the same sounds in only two ways, since in the neighboring Uralic languages there are only two types of vowel formation. The same phenomenon can be observed in guttural sounds, as well as in several other cases (Trubetzkoy's other observations in 1. Book I, pp. 94-96 and this book, pp. 408, 418).
To avoid any misunderstandings, we emphasize once again that since modern Indo-Germanic studies have rejected the thesis of a common Indo-Germanic Proto-Language, previously thought to be at least 6-8,000 years old, the basic linguistic chronology of Finno-Ugric studies has naturally also lost all credibility and is in need of a drastic reduction. In the light of the new insights, therefore

As for The development of the "Uralic" or "Finno-Ugric" basic language — to the extent that we can even speak of such a thing — cannot be dated earlier than the 2nd millennium BC, which is the Finno-Ugric proto-linguistic influences mentioned by so-called foreign Indo-Germanic linguists in connection with the development of the Indo-Germanic languages, we must interpret them — as we have already mentioned above — as meaning that these scholars — not being experts in their field — are not familiar with the 19th-century methods that still prevail in the bosom of Finno-Ugric studies today, and they adopt the fantastic chronology of the Uralic-Finno-Ugric basic languages in good faith, without thorough verification (cf. what was said in Chapter I; 1. pp. 425-427). In other words, in the case of the "Finno-Ugric" linguistic influences on the Indo-Germanic languages, we are undoubtedly talking about the massive effects of the same ancestral language, which in the first half of the 2nd millennium BC also superimposed itself on the languages of the mixed populations of Kunda-Sigirya and Svideri, and dominantly influenced their linguistic equalization processes, we could say, turning these languages into "Finno-Ugric". This is also confirmed by Nehring's already stated conviction (I. 414-415), according to which the origin of the parallels connecting Indo-Germanic with the Semitic languages can be sought in the same unknown ancient language, which was also the source of the Finno-Ugric-Indo-Germanic linguistic correspondences. And this ancient language — in addition to our current knowledge — could only have been Sumerian, whose organic parallels can still be found in the Semitic and Indo-Germanic languages, as well as in Hungarian. I. We have already become acquainted with some of them in Chapter 5 of our book. We presented excerpts from the mass of Sumerian-Hungarian-Finno-Ugric correspondences in Chapter 3 of this book (pp. 1,513-522).

In addition to all this, we must also seriously consider that even before the 2nd millennium BC there could have been a certain areal linguistic connection between the western groups of the Swedes and Cunda, on the one hand, and the late Gravettian descendants living in their western neighbourhood, on the other, and the Maglemians (Denmark, Pomerania, southern Scandinavia), in whom we must in all probability see the ancestors of the later Germans, Balts and partly the Slavs. This assumption is in line with the opinion of those researchers who, such as B. Collinder, consider the Indo-Germanic and

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Finno-Ugric languages claim a öÖs-relatedness. However, a fundamental methodological requirement is that before we talk about such a thing, we must separate the Proto-Asiatic-Sumerian linguistic connections found in both language families, and only the remaining common linguistic elements can be considered as signs of some kind of ancient areal linguistic kinship. To our knowledge, no one has yet carried out paleolinguistic studies in this direction.

In his later works (e.g.: Die Träger der Kultur der Jungsteinzeit und die Indogermanenfrage, 1949), J. Pokorný already asserts with certainty that the Indo-Germanic languages developed from the mixing of at least two completely different languages and that one of the components that played a role in their formation was probably Finno-Ugric (cf. what was said above). The Indo-Germanic tense system of the Germans and Slavs was broken down by Finno-Ugric influence — 5208071 ie. The development of the so-called "satem" languages can also be explained by the sound system of the Finno-Ugric languages, which are characterized by hissing sounds, while these were originally absent from the ancient Proto-Indo-Germanic languages. The signs indicate — he concludes this line of thought — that Indo-Germanism arose from the fusion of the most diverse elements. The results of comparative ethnology also testify to this (1. above, p. 413 of this book).

W. Schmidt (The Origin of the Indo-Germanic Peoples and Their First Appearance in Europe) also states that the so-called East Indo-Germanic language originated from two components: 1. "A-complex", which was close to the Uralic and Altaic languages; 2. "B-complex", which can be associated with the Caucasian languages. CC Uhlenbeck also believes the same — notes Schmidt. Likewise — in full agreement with E. Forrer (1. Our book I, p. 97) — Schmidt also interprets the Nesi (Hittite), Palaian and Luwian languages that appeared in Asia Minor in the middle of the 2nd millennium BC as early stages of linguistic development moving towards Indo-Germanization, and in the Finno-Ugric languages he also sees such an early — but even more ancient — pre-Indo-Germanic (more precisely, "peri-Indo-Germanic", i.e., around-Indo-Germanic) linguistic state (see what was said in connection with the designation "Finno-Ugric" on pages 551-552). This means that these researchers also clearly date the beginnings of the development of the Indo-Germanic languages to the 2nd millennium BC. After we have become familiar with the newer views of language development and language history in Indo-Germanic studies, let us take a look at our most important Indo-Germanic loanwords, grouping them according to the languages considered to be their transmitters. Our Iranian loanwords Let us see, based on the study by János Harmatta 6165/01 entitled "Iranians and Finno-Ugrians, Iranians and Hungarians", what methodological principles under-552

===== PAGE 543 ===== Our linguistics is concerned with the words found in the Hungarian language that have strong parallels with the expression of Iranian loanwords. But let us also note how they explain the origin of Iranian word combinations that can also be detected in Finno-Ugric languages. We have already touched upon this article by Harmatta (1. Book I, pp. 80-82, Book II, pp. 368 and this book, pp. 427-428). We have shown that the author represents the anachronistic views of the so-called ahistorical trend of Indo-Germanic studies, which school — despite all new archaeological and prehistoric discoveries — still stands unchanged on the foundations of the 19th century, starting its investigations from the common Indo-Germanic proto-language, proto-people and homeland, considering all these outdated hypotheses as self-evident given facts, the proof of which is unnecessary.

Based on these hypotheses, Harmatta places the disintegration of the so-called Proto-Indo-Germanic unity around 5000 BC — after he himself stated a page earlier that modern Indo-Germanic studies no longer accept the thesis of a common ancestral language and a language family tree (1. pp. 427-428) — and, in his opinion, the Indo-Iranian unity would have disintegrated shortly thereafter, if it had ever existed at all — he adds. Going further, he divides the "Proto-Iranian" linguistic state, separate from the Indic ones — which would have lasted from about 5000 to 800 BC, i.e., according to Harmatta, would represent a period of a good 4000 years — into seven periods, based on the alleged "development" of the phoneme system. We do not need to dwell on these ideas in more detail, because anyone can determine at first glance that here — as in so many other cases — we are also dealing with just a matter of linguistic acrobatics based on sound laws pursued in a vacuum, which cannot be supported by any linguistic historical data, since the earliest records of the Orani language date from the end of the 6th century BC (the Old Persian text of the Behistun inscription of Darius I, from around 520 BC). It will therefore be much more instructive if, instead of discussing such "sound-evolutionary" philological games in detail, we look specifically at what kind of "Osirianian" loanwords Harmatta finds in Hungarian and the so-called Finno-Ugric base language. His list includes 53 words. Of these, the following have obvious Sumerian parallels that are recognizable at first glance, and we know them from the 4-5,000-year-old ancient written records that cannot be dated and are only reconstructed."

1. Ösiráni (ösir.) tag-a-; Finno-Ugric (fgr.) taja , drive, hajti". Sumerian (szum.) , ag" = "treiben, führen, machen, tun" (drive, drive, do, do). * In the case of this glossary, we also gratefully acknowledge the friendly help of Sándor Csýke.

553
===== PAGE 544 =====
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Ösir. tarbhas; Fer. torpas, torwas or "orpa, torwa ,orphan", Sumer. "ur, ur-ra" = "eins, einzeln, einzig (one, individually, separately, alone, sole, unique, solitary).

Ösir. tdharvas; fgr. ttarwas Szum. , sickle, „dar, tar" = „to cut off" (levág); „Sar", = „to cut off" (levág).

Old Norse *mrtas; Greek *martas "dead". Szum. "bar" = "corpse, body" (holttest, hulla), , . Ösir. *pardas; fgr. "mar" = "morden, vernichten, zerstören" (kill, annihilate, destroy). "porcas , malac",

Noise. "buru," = "Tierjunges, Kleinvieh" (állatkölyök, aprómrha). . Æsir. *daivas; fgr. ttaivas , I" (Himmel). Noise. "tab" = "flame, burn, shine, flash" (flames, burns, shines, flashes). . Ösir. taétrá; fgr. toctara Sumer. . whip".

"uS" = "treiben" (drive, drive); cf. it may even be a formative "tar" = "schlagen" (hit, beat); in compound form therefore: „u8"+ , tar" = Treibschläger" ("drive-beater"); -a ending -tar, -tor, however bush.

. Ösir. tancah; fgr. "ýýýý , Teil" part". Noise. „the South," = . (part); in composition: „gu,-an-Su," = "sum- ma summarum? (the sum of the parts); cf. "gu," = "Gesamtheit" (totality). Ösir. "ýýýý; fgr. *caka Szum. , goat". „Seg, sikka" = „Ziegenbock" (kecskebak). Ösir. *manuzah.; fgr. *marica man" = ember". Noise. "Bruder, Genosse" (brother, comrade). Ösir. targ"tah.; fgr. *arwa "hospitality gift". Szum. "a-ru" = "to bestow, to bestow, to give, to consecrate" (ajándékoz, ad, sacrifice, offer).

Ösir. *dhe"tnuh.; fgr. *tdjine „blade". Noise. „dig" = "to flow out, fatness" (to flow out, fatness); see also Also point 13. Ösir. *dedhi; fgr. ttüye "milk". Sum. "de," = "tropfen, ausfliessen" (drip, flow out); "dig" = "ausfliessen, Fettigkeit" (flow out, fat); cf. with the vowel "dig": „ýý whose pictographic antecedent is Deimel according to "Zitce des Zicgen- oder Kuh-Uters" (the teat of a goat or a cow); the cow is therefore a derivative of milk, 6700011 meaning: "milk-giver". Our. ýý ; lgr. *peð , head" (from Melken).

Sumerian "left" — "ausgicssen, entleeren" (pour out, empty) and "graben" (dig); cf., flows, heads, heads (stone and wine), ditches.

554

===== PAGE 545 =====

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Ösir. tsvasár; fgr. tsasar Sum. , younger sister",
"Scš" = "Bruder, Schwester, Geschwister" (brother, sister, siblings).
ýsir. tsyumé; ign. "Sum- "she".
Noise. , "bum" = "Seil, Tau" (rope),
Osir. *rsyah; fgr. *erse "him", man",
Sumerian "ur, uru., = "Mann" (man).
Ancient Greek. tdesa; for. ttese "ten".
Sum. "des, 0187 = "eins, Einheit" (one, unit), but also 60; cf. "Sar.- ýýý"y "3600x60" (sixty, mud"); just as the first larger unit of the Sumerian six-ten combined number system is 60 (418), so in the purely ten system the corresponding first round number is 10 (di8).
5,22
Osir. *Sata; fgr. *Sata Sum. . hundred
"sití" = "zählen, Zählung, Zahl" (count, counting, number); oid" = "Zahl, Summe" (number, sum); "sad., Saí" = „Gesamtheit" (total).
Osir, *snéve; fig. "sounds Jin". Noise. "gan, gana, kan" = "Band, Saí,Tau, Sehne" (szalag, kötel, ín); Jin, iní" = „Flachs" (kender, len); 1. 6 June 516. old. Grow up. *many; fig. "big Jhaz". Noise. "kud" = "Rohrwand, Zweigwerk, Käfig" (nádfal, gallyfona- dek, ketrec); "Ku, kud," = "sit, lower oneself, sit" (ul, letelepedik, lakik); yes. még kutyul, kotlik. Grow up. taxation; go. tmeke „meh".

Noise. "mus," = "Holzwespe" (fadarazs).
Osir. *kar-; fer. *kar- "as, plow".
Sum." gar," = "hacken, zerstossen, Hacke" (kapál, közzüz, porrá- tór, hoe); in compound: „en-gar," = "Bauer" (peasant); literally "a person who hoes, a hoe"; therefore, the word culturally refers back to the time of hoeing before the invention of the plow.
Grow up. available; lgr. tiwed , gabona".
Sumerian "gig, gig-ba" = "Weizen" (wheat);
"gab, gaba" = "Kleie" (bran); in composition: "gabni" = "ein Nahrungsmittel, Grütze, Treber" (a type of food, groats, porridge); , gab-na" (=„,Sehi") = "Menge von Barste, Getreide"

(the multitude of barley, grain); cf., na" = "Menge, Masse" (multitude, mass); "bi, be, ba," = "uaz",
Osir. tasurah; fgr. *asura^{trear}.
Sum, „a8" = "Mensch, Mann" (man, male);
553
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"ur, uru., = "Mensch, Mann" (man, male); "asurah (tasura) is therefore clearly a pleonasm („a8 * uru.,"), and as such a sign of pidginization. 26. Proto-Scr. traupah, *raupāsah; fgr. trepa, *repese, tropa, tropasa "rōka".

Proto-Sir. tsarah; Fer. . Fuchs" (fox); cf. 6%(61101: EF, whose pictographic proto-image is "Kopf und Hals eines Vierfüßlers" (head and neck of a four-legged animal), according to Deimel Sumerian "lub-a" = probably the jackal. 27. "sara (flood, storm, 2 spring); cf. dr, 4 28. Osir. *sarih; fgr. *säre "to "water flood". Sumerian "a-ri" = "to sweep away, to flood with water" (to wash away, to flood); suru, uru," = "Flood, Storm tide" channel, turn". Sumerian "sur," = "Wassergraben, Kanal, Furche, Rinne" (ditch, / , auspressen, keltern, Wein mischen" (to press, tread grapes, mix wine); cf. also Syr. 30. Proto-Sir. tsaituh; fgr. ý furrow, water). 29. Osir. tsura; fgr. tsura , beer, wine". Sumer. „Sur" = "bridge" Sumer. "kad, kid," = "binden, verbinden" (tie, connect); cf. also tie; "sidug" = "Bach, Wassergraben, grosser Bogen, Band, Kette" (stream, water ditch, large iv, ropes, chain). 31. Osir. *Zhasra; fgr. *Sasra "thousand". Sumer. sugar" (SuS-Sar.) = "60 x 3600" (otherwise „Sar-dis" or "diS-Sar,; 1. above).
Note; cf. the Sumerian, Hungarian and Finno-Ugric numerals:
szumer Hungarian Finno-Ugric dis" = 1 and 60 ten *tese (10) ee = 60 and 600 hundred in "5" = 360 and 3600 thousand
- ka „di\$- -Sar,"
= 60 x 3600 tsasra (1000)ten thousand
The Finno-Ugric number 100 comes from a different root; cf. Sumerian "Sat" (összesség) and Fgr. "Sata (100), and the concept of 1000 was adopted from the Sumerian 60 x 3600. In contrast, in Hungarian, a logical reevaluation of the units of the original Sumerian six-tenths number system can be detected throughout, and their rational and regular reference to the corresponding larger units of the later developed and widespread ten-base system.

556

===== PAGE 547 =====

32. Osir. *sankuh; fgr. "sepke , faék",
Sumerian "sag, sig," = "to strike, to hit" (tit, ver, bever); vő, szeg; VÖ. also Sumerian "igi" = "Auge, Vorderseite, Spitze" (eye, front side, tip, peak of something) and Hungarian ég.

33. Osir. tsukah; fgr. *Suka , beard of grain", Sumer. "sig., siki" = "Wolle, Haar, Haarfell" (wool, hair, fur). 34. Osir. *vas-; fer. *wos- "to wash".
Sum. "aš," = "to demand, to wish, to desire, to need, to take possession of, to collect duties" (to demand, to wish, to desire, to want, to need, to take, to take possession of, to collect duties); cf. to take; „a-,a8," = "to demand, to demand, to need, to take possession of, to take possession of, to collect duties" (to demand, to take possession of desire, need).
35. Osir. *vazrah; fgr. twasara , axe, hammer". Sumer. "ba-su" = "eine Art Axt, Bell" (a kind of axe, hatchet). 36. Osir. *kSarah; fer. tSüre "water". Sumer. "sur," = "Wassergraben, Kanal, Furche, Rinne" (ditch, channel, groove, water); "sur, Sur" = "regnen, tropfen, fließen" (it rains, drips, / flowing); cf. still trickling, trickling, trickling.
37. Osir. "aga; fgr. tora 38. price". Sum. "ar, ar," = "Preis, Ruhm, preisen, lobpreisen" (price, fame, / praise, exalt).
Osir. tankah; fgr. + oyke Jhorog".
Sumerian "aka" = "to be bent, crooked, hooked"; cf. akad, / hang.
39. Osir. *Sardah; fgr. tsere , clan, clan order, custom".
Sum. „Sar," = "viel sein, Gesamtheit, Unmasse" (many, multitude, mass); cf. row; "ser," = "binden, Band, Bann" (bind, bond, commanding, prohibiting power); cf. ser.
Among the 53 Finno-Ugric (-Hungarian) loanwords considered to be of 6siraní origin, 39, i.e. 73.6%, can therefore be traced back to Sumerian roots without any further investigation, even through a simple dictionary comparison.

We emphasize: these Sumerian words are not "star-shaped", that is, they are not phantom words linguistically deduced from words of languages thousands of years later, merely on a "phonetic law" basis, but otherwise cannot be given anywhere, but they can be read in specific, 4-5000-year-old written records – even if not with 100% phonetic certainty, but with sound values that are very close to the original sound forms. That this is actually the case, and that we can trust the transcriptions, is shown not only by the 39 Iranian-Finno-Ugric-Hungarian words listed above, which make up 73.6% of Harmatta's 53 words

557

===== PAGE 548 =====

This is proven by the striking Sumerian correspondences of the Hungarian language, but also by the even more massive Sumerian proto-linguistic connections that we learned above when examining the vocabulary of the Hungarian language considered to be of Finno-Ugric origin: in 50 of the 60 consecutive words of the 1616 Hungarian-Finno-Ugric etymological dictionary by Lakó-Rédei, i.e. 83.3%, we also found obvious Sumerian parallels, and even there the correspondences showed that the sound values of the Sumerian words in question — with identical or very close meanings — as determined by Sumerology (in our case, Deime's "Sumerisches Lexikon", which some consider to be completely outdated) practically hardly differed from the sound forms of the Hungarian (and Finno-Ugric) words with the corresponding meanings (1. of this book, pp. 513-522).

It is also striking and very important that, according to the above glossary, the phonetic forms of Hungarian (and Finno-Ugric) words are mostly closer to the phonetics of the corresponding Sumerian words than the sound values of the inferred ancient Iranian words.
Well, now, there are two possible cases.

1. Or are the basic languages in question really as old as the competent linguists claim them to be, and thus the basic Finno-Ugric language — according to Finno-Ugric studies — would belong to the 4th-3rd millennium BC, while Proto-Iranian — in stark contrast to Harmatta's opinion — would have developed in the 2nd millennium BC, according to recent research in Indo-Germanic studies. Therefore, accepting the current positions of Finno-Ugric and Indo-Germanic linguistics, which are authoritative for each language family, the logical question inevitably arises: who borrowed from whom? The answer, based on the language family chronologies valid today, could only be that we should consider the words in question to be loanwords of "Finno-Ugric" origin in the ancient Iranian language, which developed much later. But even this could only be made if we reduced the time of the breakup of the so-called Finno-Ugric basic language from 2000 AD to at least 1500. However, we do not want to make such a claim, because this would contradict all known linguistic and prehistoric data. So the following possibility remains.

2. It is much more reasonable, obvious and probable to conclude that the so-called Finno-Ugric languages are not older than the Iranian ones, but that both language groups developed sometime during the 2nd millennium BC, under the unifying influence of the same ancient, developed cultural language, through its dominant superstratification, or as a result of its "lingua franca"-like spread: on the one hand, in the Oka, Volga, Delural, Ob region, Finno-Ugric, the basic language", and on the other hand, in a currently undefined area, somewhere in Western Asia, between the Caspian Sea and the Pamirs, Indo-Iranian, or Proto-Indo-Sindian and Proto-Sirani, the basic language". As we saw in Chapter 1 of our book I, recent archaeological investigations have revealed a very massive Pre-Asian -- more specifically, the one in question

558
===== PAGE 549 =====

In the period, Mesopotamian-Sumerian cultural influences and colonial settlers who had immigrated from East Asia were already clearly shown; in the vicinity of Turkmenistan around 00652 BC during the 3rd millennium, and in the Ural region around the turn of the 3rd/2nd millennium and after (Book II, pp. 27 and 46-49). The Sumerian-Hungarian-Ancient-Finno-Ugric lineages presented above are therefore supported and explained by clearly palpable archaeological cultural-historical and paleoethnogenetic data.

It is also worth mentioning that, according to Harmatta, in the Dirdni period - that is, roughly in the middle of the 1st millennium BC, during the time of the Ananjino civilization - the phonemic system of the Finno-Ugric languages did not yet differ significantly from each other. In the Old Irish loanwords that were transferred to the individual Finno-Ugric languages, he writes, we cannot yet detect any significant phonetic differences.

This linguistic fact, however, can be most obviously explained in our opinion by the fact that in the 1st millennium BC the Finno-Ugric languages were still quite young, and the areal linguistic equalization outlined earlier — which can be dated to the 2nd millennium — and the unifying effect of the dominant cultural language were still strongly present at that time, while the Finno-Ugric languages that later became independent were still barely separated from each other. In other words, the chronological order of Finno-Ugric studies, the age of the "Finno-Ugric" basic language believed to be 4-6000 years old, is clearly refuted by the linguistic forms inferred by phonological linguistics.

At the same time, however, the agglutinative language type and language structure of the Finno-Ugric languages, which are identical to Sumerian, indicate that the ancestors of the Finno-Ugric languages were much more intensely influenced by Proto-Asiatic-Sumerian languages than the ancestors of the Iranian groups with inflected languages. This different language development can most obviously be explained by the very large number of colonizing populations of Caucasian-Transcaucasian origin that appeared in several waves in the middle Volga and Ural regions, and consequently by their particularly strong linguistic influences, but it is also possible that the Indo-Iranian and Proto-Siranian language communities only later, during the dominance of the Proto-Asiatic inflected Semitic languages, emerged and consolidated in their vicinity, and developed their inflected language system under their influence. In this case, the Sumerian-Iranian word correspondences could have entered the ancient Iranian vocabulary — at least in part — through the mediation of the Akkadian-Babylonian language: it is known that Akkadian-Babylonian is full of Sumerian loanwords, and in all likelihood the Semitic language family itself was unified under the influence of the Proto-Asiatic linguistic koine, only in much earlier times.

What Harmatta writes about the loanwords of the Oirani period — between about 800 BC and 300/200 AD — is also very instructive. He states that while in the Volga- and Permian-Finnic languages there are 33 (in Zűrjén), 25 (in Votjak), 19 (in 559

===== PAGE 550 =====
Mordvin) and 12 (in Chereemis) 6-letter loanwords can be identified, and even in Vogul there are 9, while in Hungarian there are only 6.

Even Finno-Ugric linguists and historians agree that the ancestors of the Hungarians occupied the southernmost place in the Finno-Ugric chain, namely on the eastern side of the assumed territory, roughly in the Volga-Ural region. Reasonableness would therefore require that we should find the most Iranian immigrants in Hungarian, since the 16th-century linguistic influences undoubtedly reached the Finno-Ugric peoples from the south or southeast.

It does not help here that Harmatta — sensing the glaring contradiction in the distribution of loanwords he considers to be Oirani — is forced to place the szdlldstenilet of the ancient Hungarians in the 1st millennium BC to the north or far east of the Permians. He does not consider that with this typically "Finno-Ugric method", starting from a single "sound law" linguistic idea, or more precisely, trying to prove this linguistic assumption, but otherwise completely unfounded, "ad hoc" idea, he only ends up in a heap: he comes into irresolvable contradiction with the other, not hypothetical, but truly indisputable linguistic fact that the number of Lapp and Samoyedic similarities is the least in the Hungarian language on average among Finno-Ugric languages, but the Lapp and Samoyedic linguistic affinity expressed in percentage terms is also much greater in the other Finno-Ugric languages than in Hungarian (L. present book, pp. 498-502). Now, the languages lying north or east of the Permian

In the case of the ancient Hungarian settlement areas — up to the end of the 1st millennium BC! — all these correlations should appear in the linguistic material in the opposite direction: we should find the most Lappish and Samoyedic matches in Hungarian, and the percentage of linguistic affinity should also be the highest between Lappish and Samoyedic, as well as Hungarian.

On the other hand, placing the ancient Hungarians near Pechora or in the northwestern part of Siberia — in the 1st millennium BC — cannot explain the numerous, undeniable Scythian-Sarmatian parallels that we can find in the lifestyle, art, cultural image and spiritual treasures, and legendary traditions of the conquering Hungarians (L. Book II 331. yy 334-337. pp.), and which are prevalent among the other Finno-Ugric peoples. Such a large amount of material and spiritual tradition, preserved in a closed unit for more than 1000 years, indisputably proves that the ancestors of the conquering Hungarians in the 1st millennium BC definitely lived in the Scythian-Sarmatian region, or at least in its immediate vicinity, and not far in the high north, even further north than the Volga and Permian Finns.

Finally -- but not least -- we would like to remind you of the results of Tibor Tórt's anthropological studies, according to which the conquering Hungarians 560

===== PAGE 551 =====
His remains are among the Sarmatian finds, in Sarmatian graves excavated in the lower Volga region (see pp. 457-458 of this book).
Given our current knowledge, the idea that the ancestors of the Hungarians lived north or east of the Permian in the Oirani period - that is, precisely in the Scythian-Sarmatian period - is downright absurd. All the more so because these results of Tibor Tóth were well known several years before the publication of Harmatta's present article (1977).

Such trifles, however, obviously do not disturb Finno-Ugric linguistics in its toy-making with sound laws. We cannot expect anything else: after all, what do these few years do compared to the fact that our linguistics ignores the other anthropological fact, well-known for almost a century now, that the Ob-Ugrians have nothing to do with the Hungarians anthropologically, and despite this, it categorically claims to this day that our language and ethnicity are a derivative of the "Ugric basic language" and the "Ugric ancestral people" shared with the Ob-Ugrians.

The aforementioned contradictions therefore show more clearly than ever that, despite all his self-confidence, Harmatta did not succeed in temporally separating the Iranian loanwords of the Finno-Ugric languages. In other words, the "phonetic" linguistics was thoroughly mistaken this time too, and ended up in a hopeless dead end, as it has in so many other cases. Whether we accept the loanwords that Harmatta considered Iranian as being of Iranian origin or not, the fact remains that the Proto-Hungarians, who must undoubtedly be placed somewhere between the proper Finno-Ugric and Iranian language areas, played a key role in the issue of Iranian-Finno-Ugric word parallels.

In the Middle Iranian period (2nd century BC - 9th century AD) — Harmatta continues — the distribution of Iranian loanwords in Finno-Ugric languages is quite different. Hungarian clearly leads with 45 words, followed by Zűrjén (39 words), Votyak (31) and Mordvin (28), from which Vogul (24) and Ossetian (17) are not far behind. That is, the situation with the Volga and Permian peoples is approximately the same as in the Oirani period, but Hungarian has moved from last place to the forefront, following to some extent the Ob-Ugric peoples.

We have already stated above that these inexplicable fluctuations in the Iranian loanwords of certain Finno-Ugric languages clearly show that something is wrong with the methods of linguistics working with sound laws, especially in the question of the relations between Hungarian and other Finno-Ugric languages. Harmatta's explanations, although unspoken, also imply that the Hungarians, Voguls and Ossetians, who in his opinion were still living far north in the Ananjino period, would have broken through the settlements of the Permians and Volga peoples around 82 AD and moved far south, right to the border of the steppe. However, no trace of such large-scale population movements can be found in archaeological finds, and as a result, such a purely linguistic, individual-361

===== PAGE 552 =====

We must refer assumptions built on "sound-history" ideas to where they come from: among the improbable and unprovable ideas of 19th-century linguistics.

This hypothesis of Harmatta is made even more absurd by the fact that, speaking of the ancient Iranian period, he himself states: the greatest influence of ancient Iranian culture fell on the Finno-Ugric tribe in which we can see the ancestors of the Hungarians, and this is also reflected, for example, in the adoption of large animal husbandry. "There is no doubt that the most advanced animal husbandry (multi-purpose large animal husbandry) is reflected in the ancient Iranian terminology adopted into Hungarian" — he notes (id. m. p. 175).

Therefore, according to Harmatta, the ancestors of the Hungarians in the ancient Iranian period must be sought in the southernmost band of the Finno-Ugric peoples, since the ancient Iranian cultural influences coming from the south are most strongly manifested among them.

If we now put the prehistoric ideas that appear here and there, scattered and unsystematically, in Harmatta's article into a logical order, then we would have to forcefully assume that the ancestors of the Hungarians originally lived on the southernmost edge of the Finno-Ugric territories, then towards the end of the Ősirán period, after the turn of the 2nd/1st millennium BC, they moved up somewhere to the region of the Pechora or the lower Ob, to the northern reaches of the Ural Mountains, even further north than the Voguls and Otyaks (since, according to him, Hungarian has far fewer Ősirán loanwords than Vogul), and then 8-900 years later, around AD, at the beginning of the Central Iranian period, they migrated back to their original places of residence, to the fringes of the Eastern European or Western Asian steppes. And all this was done solely to justify Harmatta's assumptions about the chronology of Iranian loanwords based on phonetic evolution.

These prehistoric leaps of Harmatta — inferred by linguistic paleontological methods — are eerily reminiscent of the Ob-Ugric "swing" assumed by Fodor between the mouth of the Ob and the area of the Andronovo culture (Volume 1, Book 11, pp. 327-330). In other words: all these helpless experiments clearly testify that the Hungarian language was not yet developed by the language development and regular sound change methods developed by Finno-Ugric studies precisely for this purpose.

"We cannot even manage to fit it into the scheme of the Finno-Ugric language and people family. Our language somehow always throws us off our guard, and it constantly throws the most careful calculations of the school of our exclusively Finno-Ugric origin across the board. For example, we are like this with the Persian layer of our Central Iranian loanwords. According to Harmatta, judging by their phonetic structure, these Virtually none of the 6 borrowings come from Persian proper. can only be northwestern Iranian ("Med" and "Parthian") loanwords, that is, they come from the area of Iran that borders the Caucasus region," (id. m. pp. 181-182)

562
===== PAGE 553 =====

On the other hand, Oszvöld Szemerényi, in his excursus to F. Altheim's "Geschichte der lateinischen Sprache" (id. m. 66-84, p.), states that a number of Central Iranian 6700016 Hungarian words can only be derived from the sound system of Southwest Iranian, proper Persian nyclv. However, since he cannot imagine direct contacts between the ancient Hungarians and Southwest Iran - under the spell of Finno-Ugric dogmas - he cannot specifically explain this striking circumstance, apart from various vague, unstable assumptions. Finally, he is forced to assume an imaginary North Iranian dialect that cannot be given data anywhere, which, however, should have almost completely coincided with the proper Middle Persian dialect, and from this he derives the Hungarian words with the same sound structure as Middle Persian.

Discussing Szemerényi's excursion, Altheim in his aforementioned work thinks that the old Khovarezmi language could have been the North Iranian dialect very close to Persian, from which the Hungarians adopted their Persian-sounding words. He concludes this from the remark of the 11th-century Khovarezmi-Arab historian Beérüni that "... the Khovarezmi are shoots of the Persian tree". He adds, however, that this cannot be proven, because the relics of the Khovarezmi language, which are known from the 13th-14th centuries AD, "... show no affinity with the southwestern Iranian language of Persia" (ibid., p. 85). In Hungarian: "... they show no affinity with the southwestern Iranian language of Persia."

Much more remarkable, however, are the conclusions that Altheim draws on the basis of these linguistic phenomena: he considers it beyond doubt that even before the Ossetian contacts — which the research places around 600-800 AD — the ancestors of the Hungarians must have been in close contact with a northern or northeastern Iranian people who used a more closely related Central Persian dialect, most likely the Chovarezmi (ibid., p. 186). This statement stands in sharp contrast to all the assumptions that our Finno-Ugric-oriented prehistory research has made over the past 100 years regarding the residences of the ancient Hungarians before 600 AD, and at the same time seems to confirm the Chovarezmi traditions preserved by Kézai.

From this alone, it is clear that the issue of our Iranian loanwords is not as simple and clear as our linguistics in general — and Harmatta in his present paper — tries to portray this complex of issues as being straightforward and problem-free. However, when considering these questions further, we must also take into account the very important aspect that the thesis of the Iranian-ness of the various northern and eastern (Khorazmi, Bactrian, Sogdian, Kotani-Saka, etc.) itself stands on very weak foundations (I. Jettmar et alii: Geschichte Mittelasiens; and also Book I, pp. 83-84). But "Iranian" dialects

563
===== PAGE 554 =====

Even WB Henning and Altheim - who do not doubt the Iranian character of these languages - state that Sogdian, Khovarezmi and other northeastern Iranian dialects are so far removed from the truly Iranian Persian language that they can only be related to it "in the misty antiquity" ("im grauen Altertum") (IWB Henning: Mitteleiranisch, Handbuch der Orientalistik 4. 1958. pp. 109 and 113; Altheim: Geschichte der Hunnen Vol. I. p. 427).

It should also be noted that Harmatta's only example, which he cites to prove that our loanwords with Persian phonetic forms from the Middle Iranian period do not originate from the Middle Persian language proper, is linguistically very lame: we saw earlier (cf. p. 557) that Harmatta derived the Finno-Ugric word "vásárol" (Hungarian vesz) from the Old Iranian "vasty", and now, at the same time, with a surprising twist, he mentions an alleged Old Iranian word "vahálara- (market, fair) that is also "starry" — i.e., again not datable, only inferred — which, according to him, would have developed into "vázár" in Middle Persian, and into "vacâr" and váSar" in the northwestern dialects, and this latter phonetic form is the Iranian source of the word "vásár".

would be the Hungarian. However, since Harmatta himself first established that the verb root of the word "vásá- (more correctly, vesz", dialectal, vösz") in Old Iranian is "vas-", and the datable Middle Iranian developments (with the meaning "vásár") are "vázár", "vacar" and "vásár", we can rightly ask: on what basis does he now assume at once that the Middle Iranian words in question would have originated from an Old Iranian phantom word form like "vahacara-? What is the need to insert the syllable "-hà-? Presumably it is only to be "sound-developed". If the Old Iranian verb root really sounded like "vas-", which is likely to be the case, apart from the known regular Middle Iranian further formations (vázár, vacâr and vásár), the Sumerian proto-language parallels ("aS., a-a8," = "verlangen, begehren, wünschen, bedürfen, nehmen, sich bemächtigen, Zoll erheben; verlangen, Bedarf") are also obvious. supported in such a way, then under such circumstances there is no justification for the existence of any word form in the Oirani language such as "starry", "vahálara-". However, the most important thing in the whole issue is that since the Hungarian verb "vesz" in its high-tone version still faithfully preserves the sound form of the "vas-" root word derived from the former, ancient Iranian base language", our word "vásár" cannot be viewed independently of this and simply declared to be a Middle Iranian loanword.

Finally, we must also point out the harmonious agreement that prevails in Hungarian linguistics in the field of deducing the phonetic forms of ancient basic language words. A very good example of this is our word "vesz", whose "basic forms" look like this in the projection of representative linguistic publications of only three years:

564
===== PAGE 555 =====

1, according to TESZ (Vol. III. 1976): in the "vesz" and its counterpart, "visz" are the common Finno-Ugric base form "fike- , could have been"; 2nd article cited by Harmatta (1977): the Finno-Ugric basic form of "buy" (buy) is "wos-; 3. in the Lakó-Rédei etymological dictionary (Volume III, 1978): "vesz" is of Ugric origin, its basic form "wey- ; that is, one member "wike" is a Finno-Ugric heritage, its basic form is "wike- of the word pair would belong to the "Ugric" and the other to the "Finno-Ugric" basic language, which, although - precisely from the perspective of Finno-Ugric linguistics - means a difference of more than 1000 years, apparently such "small things" cannot deter our Finno-Ugricists from continuing to deal only with the supposed "laws" of sound changes that are thought to occur regularly in time, as if by a higher command, without regard to all other linguistic circumstances. The necessary consequence of this one-sided approach to sound laws will then be the confusion that we could observe earlier in its basic forms. This also shows the fundamental methodological error of sound law linguistics. The problem is: if the "sound laws" were truly legal, they would not result in different basic language sound forms Our word "takes" is an inferred proto-linguistic for almost every researcher.

We cannot agree with Harmatta either when he mentions the peculiar sound changes ("ã">"i, "a > „ë") of our so-called "Cho-Varesmi-Alán" loanwords — which he himself writes cannot be detected anywhere in the known Alanian linguistic monuments — explains that according to Berüni's report, even in his time (i.e. in the 10th-11th century AD) Alans and Ases migrated from Khovarezm to the shores of the Khazar Sea (Káspi-tó), and their language "was from Xvarizmi and Pecheneg". One of the characteristic features of Xvarizmi was precisely the a > i sound change. It is obvious to think that in the Alan dialect which was under strong Xvarizmi influence, the same sound development tendency prevailed, and thus in it the i corresponded to the ü of the other Alan dialects" — notes Harmatta (id. m. p. 182).

Here is a textbook example of the aforementioned lack of methodology: although the sound changes in question cannot be detected in any Alanian linguistic record, the Hungarian words in question are nevertheless a priori "Chovarezmi-Alani" they become loanwords, This would be our preliminary, theoretical-methodological objection to the above quote, As a next step, let us accept Harmatta's argument conditionally for the time being, and first take a closer look at the historical data of the time. In the light of these, it soon becomes clear that Harmatta's reasoning contains a fundamental error; namely, the people of Berüni, whom he mentions along the Amu Darya in the company of the Pechenegs, even have their language similar to the Pechenegs.

===== PAGE 556 =====

Vertnck says that in Berüni's time, i.e. in the 10th-11th centuries AD, the name could hardly have been Alan or As in the earlier sense, but at that time it could obviously only have meant the Uz, a tribal group of the Oghuz Turks, to which the Pechenegs also belonged. It is well known that around the 9th-11th centuries, first the Pechenegs, and then the Uz in their wake, migrated from the shores of the Aral Sea to the vicinity of Lake Caspian, and then to the territory of present-day Southern Russia. Moreover, since we also learn from Berüni that the language of these As was strongly mixed with the language of the Khovarezmi and Pechenegs, which practically means that some kind of language alliance had developed between the three peoples, when examining the sound changes in question we must also take into account that the so-called One of the characteristic features of our Old Turkic loanwords is precisely this "a"—"i" (y) sound transition. Harmatta's argument would therefore not be convincing even if we were to believe in good faith everything he claims, citing Berüni. However, the question takes a completely different form when viewed more closely. Since we already know from experience that we should not accept any Finno-Ugric source data without checking, we looked up Harmatta's reference in this case as well, to the best of our ability. This time, our effort was not in vain, because the text in question reveals that Berüni does not mention two peoples, but only one: the Alans or the Ases; — at least this is what Zeki Validi, who processed the text, says. And this is a very significant difference.

But we learn something else from Berüni. Here is Bérüni's report, interpreted by Zeki Validi: "In the introduction to his 'tahdid nihayat al-amâkin', he reports that the Alans or As, together with the Pechenegs, inhabited the lower reaches of the Amu Darya (the Özboy) in antiquity and then, as a result of the river's course changing, migrated to the shores of the Sea of Khazars. He explains that the language of these Alans was composed of Khwarezmian and Turkish-Pecheneg." (I. Zeki Validi: On the Language and Culture of the Ancient Khwarezmians; Journal of the German Oriental Society, 90, 1936, Appendix 28. old.)

In Hungarian: "In the introduction to his work Tahdid nîhâyat al-amâkin, he tells about the Alans or âsök that they once lived with the Pechenegs in the lower reaches of the Amu Darya (at the Özboy) and then, due to the change in the riverbed, migrated to the shores of the Khazar Sea. He says that the language of these Alans was a mixture of the Chova-rezmi and the Turkish language of the Pechenegs."

Altheim also writes the same about this report by Bérüni: "He speaks of the dialect of the As or Alans..." (1. Altheim-Stiehl: Geschichte Mittelasi-566

===== PAGE 557 =====

ens im Altertum, pp. 178-179) In Hungarian: "The Ások or Alän dialect speaks..."

In addition, we would like to mention that although the change in the Amu Darya bed cannot be precisely dated, it is certain that it did not happen in the 10th century AD, but many centuries before, as Bérūni writes: "im Altertum".

Harmatta therefore completely arbitrarily changes the text of Bérüni's original report twice in order to use it to support his thesis:

1. writes about Alans and Āsok, whereas Bértini mentions Alans or Āsok;
2. He claims migrations of the 10th-11th centuries, although Berúni explicitly and unambiguously refers these people's movements to the distant past: "sometime ago", when the bed of the Amu Darya changed - he writes about them.

Here, the well-known "objectivity" and characteristic "source criticism" of Finno-Ugric scholarship reappear.

If we have already reached this point, we can go one step further. Just because Bérúni reports on the ancient settlements of the Pechenegs and As in the Aral Sea region, we must not forget that already in the 2nd century BC, in the area of the Aral Sea, Khovarezm and Sogdia, we have shown very close phonetic parallels between the names of all the later significant tribal groups of the Oghuz Turks among the northeastern Iranian folk names published in Chinese annals and in the works of classical Greco-Roman authors: asi-uz; pasiani-Pesheneg; Kang-kü-kangar; kun-bok (the name or title of the prince of the U-sun or Wu-sun people) - kun-beg or kun-beke (1. Book II. pp. 270, 289-292, and 353-354).

All these ethnonym correspondences that we assume are largely supported and made probable by Berüni's report: he explicitly states that the language of the Alans or Ayöks was a mixed Khovarezmi-Bechghian language.

However, since the Iranianness of these Turken-northern Iranian peoples mentioned in Strabo, Justin and other ancient authors — apart from the correctness or incorrectness of the above name parallels — is also open to much doubt (Book I, pp. 83-84 and this book, p. 564), the question of the entire Hungarian loanword complex, which is claimed to be of Central Iranian origin, remains open. All the more so because in Book II we also pointed out that, according to the results of recent source research, the Avars, Sabirs, Bulgars, Khazars (and with them probably significant groups of the predecessors of the later Hungarian tribal confederation) migrated from the region of Khovarezm, Sogdia, Bactria and Southern Turkmenistan, from the territory of the Hephthalite-Hun empire, in the 1st century AD. In the middle of the 1st millennium BC, through Dagestan and the Caucasus to the Kuban-Pontus tributary (I, pp. 265-267; II).

567

===== PAGE 558 =====

Altheim: *History of the Huns* I. köt.; Haussig: *Theophylact's Excursion on the Scythian Peoples*).

Last but not least, the Avesta question is closely related to the objective assessment of our loanwords, which are considered to be of Iranian origin. We have already mentioned that the oldest Gathas of the Avesta are still not understood by Iranologists today (Book 1, p. 236). In Altheim's words: "Sentence structure and expression, word order and word choice, a high antiquity of language and thought place barriers to understanding." (Altheim-Stiehl: id. m. p. 63) In Hungarian: "The sentence structure and expression, word order and word choice, language and the high antiquity of thought place barriers to understanding." That is: nicely described, but ultimately, *ignoramus*.

Well, first of all, this Avesta serves — mostly with the help of linguistic sophisms that can be called embarrassingly primitive — not only to Iranianize the various ancient languages of the peripheral regions around Persia (Khovarezsm, Sogdia, Bactria, Fergana, Kotan, etc.), but also to support the Iranian nature of the Scythians and Sarmatians. And if we also mention that primarily the Central Iranian language, but also partly the Avesta, forms an almost inextinguishable linguistic web with the ancient Turks, then we have before us the real situation of Iranology in its entirety, in spite of all the hype to the contrary.

The massively emerging Iranian-Avestan and Old Turkish parallels — it almost goes without saying — have of course been dealt with summarily so far by strictly adapting to the 19th century Indo-Germanic culture-priority theory's "method" of examining loanwords, and by simply declaring all Turkish word equivalents found in Iranian languages to be loanwords of Iranian origin in Turkish — in every way similar to the procedure used at that time to determine loanwords in the Hungarian language.

The situation has changed somewhat, however, after the large-scale, planned Soviet excavations in Central Asia of the last two decades uncovered the tomb of a 5th-century BC Sakian prince or prominent warrior, filled with precious metal objects, east of Alma-Ata, near Issyk-Kul. On one of the Eziist cups lying next to the skeleton, which was almost covered with silver plates, were found symbols very similar to the Orkhon-Yenisei five-beaded runic script. The body of the warrior, buried in splendid decoration, lay in a side grave of the excavated kurgan. Unfortunately, nothing remained of the main burial, since "according to the old good custom" it had already been robbed in ancient times.

Soviet researchers associate the tomb with the "sai-wang" people of Chinese sources, the "saka-uraka" people of ancient authors, i.e. the royal tribe of the Sakas, since the Chinese annals mention this ethnic group directly in the vicinity of Issyk-Kul (1. B. Brentjes: *Mittelasien*, p. 45). Brentjes trace-568

===== PAGE 559 =====

It also highlights - with an obvious side-stepping towards the "päiniräni" cultural heritage images common in previous research - that these treasures, just like the

Hermitage hires because of the gold of Siberia, the Persian treasure of the Sakas and the high standard of their metalwork, "...

sondern lokale Produktionen." (id.m. p. 45) In Hungarian: ".....because the objects are not "Persian" exports, but local products."

As can be seen from this quote — Brentjes makes a sharp distinction between Sakas and Persians! — the Sakas can hardly be considered Iranians in the light of recent research.

Here we recall the data disproving the Iranianness of the Scythians and Sakas (1. 57-59.; pp. 236-246. and 270-271.), especially that the brother of the king of the Scythians living north "Scythians" — i.e. Sakas of Jaxartes already bore an Altaic name in the time of Alexander the Great: "Charthasis" (Kara-Tas), in Hungarian, "Black Stone" (1. p. 240.).

However, these Central Asian Saka writing symbols from the 5th century BC clearly refute the cultural historical views held so far about the spread of writing.

Until now, the generally accepted idea was that the Phoenician-origin, so-called West Semitic script spread to the East along with the Aramaic administrative language ("Reichsaramäisch") used in the Achaemenid, Old Persian Empire, first in the Old Persian Empire, and then from about the 3rd century BC in India, where the so-called Kharosthi script developed from the Aramaic alphabet. However, the nature of the writing found along the Issyk-Kul suggests that the script reached Central Asia by a different route, and that its basis was not the Aramaic alphabet, but a version of the so-called West Semitic, Syro-Phoenician script, much older than Aramaic. It can therefore be assumed with great probability that the script of Pre-Asian origin was adopted by certain northern and northeastern ethnic groups that had particularly strong ties with Pre-Asian cultures already in the Neo-Assyrian period. The Scythians and Medes are primarily considered as such, who demonstrably played a prominent role in Assyrian-Babylonian-Syrian history in the 7th-6th centuries BC. The Scythians are actually identical with the Sakas, whose legacy is the Issyk-Kul inscription.

It is also necessary to mention that the generally accepted thesis of the "West Semitic" origin of writing was already seriously questioned by the discoveries of inscriptions from Ugarit (Ras Shamra) in the 1930s. The cuneiform alphabet used there around 1500 BC was determined by many researchers to be of non-Semitic origin (1. e.g. H. Bauer: *Die alphabetischen Keilschrift-texte von Ras Schamra*). In addition, another script similar to the signs of the later Phoenician alphabet was also discovered in Ugarit from the middle of the 2nd millennium BC, which was used not only to record texts in the West Semitic but also in the Hurrian language.

569

===== PAGE 560 =====

However, the complete change in the picture of written history is linked to the name of Anton Moortgat. During his excavations carried out over many years in the ruins of Tell Chuera near the Chabur River, in the area of the former homeland of the Subartui-Hurrian peoples, a fragment of a vessel from the Mesilim period (ca. 2700 BC) was discovered in 1974, on which five letters — very similar to the signs of the later Phoenician writing — were visible. Professor W. Röllig, the Semitic writing expert who assessed the find, acknowledged the "Phoenician" nature of the signs, but was unable to bring them into closer contact with any known West Semitic alphabet. At the same time, he considered it completely impossible that the signs could originate from the 3rd millennium. Moortgat notes that, although incomprehensible and unexplained phenomena often occur during excavations, he emphasizes that the 3rd millennium BC age of the vessel fragment in question cannot be doubted due to the precise observation of the excavation layers (1. Moortgat: Tell Chuera in Nordost-Syrien. Preliminary Report on the Seventh Excavation Campaign 1974. pp. 30-33).

This opinion of Moortgat was clearly confirmed by the excavations carried out in 1976. In an undisturbed layer undoubtedly belonging to the period of the early Sumerian dynasties (3000-2500 BC), observed "in situ", directly in the vicinity of the site of the pottery fragment with inscriptions found in 1974, only slightly deeper, several pieces of pottery were again found, with incised numerals and on one with "Semitic" letters. Moortgat states that after this new find, "... the existence of a Semitic alphabetic writing in the 3rd millennium, in the Mesilim period, can hardly be doubted." (1. Moortgat: Tell Chuera in Nordost-Syrien. Vorl. Bericht über die achte Grabungskampagne 1976, p. 39.)

It should be noted, however, that during the early Sumerian dynasties, we only have data on the Subartu-Hurrian population in the area of the Chabur River, i.e. in Northern Mesopotamia (for example, the first Subartu-Hurrian personal names in Northern Mesopotamia were found in the Akkadian layer of Tell Brak, located barely 60 km from Tell Chuera), but we do not find any traces of Semites from these early times. Therefore, from the fact that the writing signs found in Tell ChuEra resemble the signs of the Western Semitic script that appeared one and a half millennia later, we can conclude much more, contrary to Moortgat, that the Semites later adopted the script from the Hurrians. Incidentally, the Hurrian-language script texts found in Ras Shamra also testify to this: the research considers them all to be of non-Semitic origin.

"Phoenician" in nature were all written in the cuneiform alphabet, which latter script - as we saw above -

We must also briefly recall our Ossetian loanwords, which we have already become acquainted with in connection with Bernát Munkácsi's original article (*Alanische Sprachdenkmäler im ungarischen Wortschatze*) (I. IL könyv, pp. 352-353). In addition to what was said there, we will also mention the following:

===== PAGE 561 =====

We should also mention that the Ossetian language contains several grammatical elements that are characteristically Hungarian and are absent from other Iranian dialects. For example, the present tense plural 3rd person ending is "-ncá" (cf. Hungarian "-nak, -nck"); the subjunctive mood plural 3rd person ending is "ejoncá" (cf. Hungarian "-janak, -jenck"); among the Central Iranian languages

only Ossetian has a definite article; "i" (between i and 8; cf. 6, az). These unique features in Iranian languages clearly indicate that Hungarian once strongly influenced the development of the Ossetian language,

The unclear ethnic and linguistic affiliation of the ancestors of the Ossetians, the Dsoks (or the Alans), also contributes to this. We have already mentioned that research derives the Asoks (and the Alans) from the Turkmen tribes mentioned by Strabo and other ancient authors under the names Asi, Asiani. The ethnic name Asi, on the other hand, is strikingly similar to the name of the Uz, and the name parallels of other peoples of the Uz (Oguz) ethnic group also appear in the same ancient sources that mention the names Asi, Asiani (Book 1, p. 567). We saw above that Berün's report also very likely supports the evidence of these ethnic name parallels; in ancient times ("im Altertum") the Pechenegs living in the company of the

He informs us about the Asans or Alans in the area of Lake Aral, in the immediate vicinity of Chovarezem, whose language was strongly mixed with the language of the Pechenegs (Book 1, p. 566). At the same time, the ethnic name Asiani is strikingly similar to the name of the people mentioned in Chinese sources as "a-Si-na", from which people the Chinese believed the Turks originated (1. Liu Mau-Tsai: Die chinesischen Nachrichten zur Geschichte der Ostürken).

This also includes the research results of László Bendefy, according to which the groups of Hungarians who remained in the east, living in the foreland of the Caucasus, along the Kuma River, largely fled to the more protected valleys of the Caucasus after 1380 to escape the devastation of Timur Lenk, and there they mixed with the Ossetians, Abkhazians, Circassians and Kabardians (Bendefy: Kunmgyaria). This mixing is also proven by the fact that at the end of the 19th century, Zichy's Caucasian ethnographic expedition still found strong Hungarian origin and kinship traditions among these peoples. Therefore, when examining Ossetian-Hungarian linguistic parallels, we should not ignore these very important data. Not even if our academic historical research sweeps these traces aside with the superficial reference that they "probably" originate from the Hungarians who were deported by the Tatars.

Summarizing what has been said, we can see that the most important historical and linguistic issues related to the Iranians are still largely unclear, the research is actually still in a fluid state, and in fact the only thing certain is that at the end of the 9th century BC a people appeared in Western Iran - called "Parshua" by the Assyrians - who, after three 586 6j years, created the Old Persian empire. Their language

571
===== PAGE 562 =====

was the 6irani, whose 1626156 linguistic monuments appear around 520 BC. Virtually all other ideas in common use about the ancient culture, ancient language, origin, etc. of the Iranian peoples are merely unproven assumptions, and the appearance of great certainty is only created by the circumstance - for outsiders, but not for specialists! - that most handbooks or summary historical works still present the 19th-century view of Indo-Germanic studies, the various unproven, a priori basic theses on Pan-Indo-Germanic - in this case Pan-Indo-Iranian - assumptions, historical and cultural-historical ideas that have grown out of theses, as decided facts and historical realities. However, if one looks behind the loud voices behind the scenes, one soon notices that Iranian studies research is still groping in a state of considerable uncertainty on most fundamental issues even today.

The fact is that in this area too, the Iranian language (but also Old Indus and Sanskrit) is full of Sumerian (and Akkadian) roots, which are of course not taken into account just out of convenience, because they would irreparably tear to pieces the chains of language development hypotheses built on sound laws and regular sound changes. It would not be possible to continue to dominate so nicely with the high-flying etymology of Avestan words, first broken down into their syllables and then -- in a different order -- put together again in different ways, such as "owner of the desert horses" (the "Iranian" name of the Animasap people) or "month with united horses" etc. etc. (1. Book I, 58-59 and Book I, p. 238). But we should also get off the shiny cane horses of the Grimm-like sound carousel for 130 years, whose special appeal lies in the fact that with their help it can be shown that almost every sound can be passed into one another.

If anyone has any doubts in this regard, we recommend that they carefully study, for example, Altheim-Stiehl, in whom the above-mentioned person undoubtedly belongs to "History of Central Asia in Antiquity" című könyét, the more sober, more thorough -- you can find a mass of the most absurd etymologies and hypothetical sound changes invented using the "method". Yet Altheim researchers, and to whom we can thank not one truly important, significant discovery. Unfortunately, however, he was not able to shake off the shackles of the laws of sound either. It is clearly evident from this work how much Iranological research stands on feet of clay in reality. Even from this cursory overview, we can conclude that we are still only at the beginning of truly thorough, comprehensive research on the issue of our loanwords considered to be of Iranian origin. However, future studies in this direction can only lead to reliable, objective results if they are freed from the trappings of the 19th-century Indo-Germanic cultural priority ideas.

572
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- Our German loanwords
- In the almost 1100 years since the conquest, the logic of historical relations dictates that, naturally, many German -- mainly Bavarian-Austrian -- words have entered our language. We do not want to argue about this. We must only point out the one-sided methods of our linguistics, according to which almost every word that we have in common with German, with the same or similar sound and meaning, would be a German, or more precisely a Bavarian-Austrian loanword. Let us first examine what traces we can find of more than a thousand years of coexistence in the Bavarian-Austrian regional language. In terms of vocabulary, several Hungarian words have been adopted. Let us see some examples of these: 1. quality goods"; a regional language word widely used in Lower Austria. 2. "Gatte-Hose" = , underpants"; a vernacular word widespread throughout Austria. "Törek-Ware" = . bad
3. "Maschik-Seite" = 4. . other side"; a commonly known expression in Eastern Austria. "Pussta" = , desert, steppe"; used throughout the German-speaking area. "Ischikosch" = "chickenpox"; also common throughout the German-speaking area. 6. "Gulasch, Golasch" = "gulyás/soup/, goulás/-meat/"; a wandering word known throughout Europe. 7. "Säbel" (Svab-Alemannic "Sabel") = "sable"; a wandering word widespread in almost all of Europe. According to Kluge's etymological dictionary, it is of Hungarian origin (1. more details in Book II, pp. 254-255 and this book, p. 549).

However, in addition to word borrowings, we also find some grammatical, structural, and phonological parallels in the Bavarian-Austrian dialect, which is directly adjacent to the Hungarian language area.

For example, the use of the deep Hungarian "a" sound in the Bavarian-Austrian vernacular, which can be observed widely, from Eastern Austria to Bavaria, is one example. This sound is otherwise unknown in Indo-Germanic languages, so we can only evaluate it as an areal influence of the Hungarian language in Bavarian-Austrian. An equally striking phenomenon is the formation of the property relationship on the Hungarian model. The Austrian colloquial language does not use the general German word order, but Hungarian, and at the same time -- also similar to Hungarian -- "zöen" -- the possessive pronoun also appears in the possession. For example, "die" Instead of "my father's sister", they say "my father's sister" Schwester"; that is, "my father's daughter" and not "my father's daughter".

573
===== PAGE 564 =====

This is also related to the uncertainties in the use of case-related prepositions. In Vienna, for example, during the summer holidays, signs can be seen on the doors of shops saying "closed due to holidays", in many cases as follows: "wegen Urlaubs geschlossen". The correct spelling would be "wegen Urlaubs geschlossen", since "wegen" is a preposition with the genitive. This error indicates that the Austrian sense of language does not perceive the word "wegen" (because of) as a preposition, but, like Hungarian, as some kind of noun suffix, which has no case-related. The same situation is true when we hear "während der Krieg" instead of "Krieges" (during the war). also with the preposition "während" (during, during): in everyday speech the grammatically correct "während des

The question of a late Hunnic, Avar, and early Hungarian linguistic base layer in the present-day Bavarian-Austrian region, which can be repeatedly mentioned in contemporary sources, also arises here. The already mentioned Unterdorf" (Lower Village) correspondences ~~places called Obereindorf (Upper Village) and Unterdorf~~ which we often do not find i.e. more than likely Oborendorf" (Avar village) (1. Ertl: Topographia Norici); the settlements called "Edling/Kazaze" in Carinthia-Stäjerország, which point to the Avar origin of the Carinthian "Edlings" (see Lessiak: Edling -- Kazaze); the possible connection of the name of the Bavarian "Chiemsee" with the Hungarian bird name "gém" (1. Klebel: Langobarden, Bajuwaren, Slaven); as well as numerous place names of Hungarian origin or of former Hungarian (Avar?) population in Austria (1. Vilmos Györi: Die magyarischen Ortsnamen auf dem Boden Österreichs, 1962; doctoral dissertation). We have already discussed all these questions in more detail in our II. volume (1. II. book, pp. 283-284.)

In light of the above data, it would be absolutely necessary to thoroughly re-examine the words we consider to be Bavarian-Austrian loanwords and to explore their possible more distant connections in a complex manner.

Let's look at a concrete example using this multi-faceted approach. TESZ leaves a line-like mark with a pointed . He considers our word "karcol" to be of German origin. According to the TESZ, the meanings of the word are: 1. , sharp, instrument, nail, etc.; ritzen, kratzen"; 2. , draws, scribbles; zeichnen, kritzeln"; 3. "scratches (throat), causes a sharp sensation of pain (in the stomach, etc.); kratzen im Hals, Magen usw.:" The derivative of karcol is karcos = , belligerent, quarrelsome; kampflustig, streitsüchtig". Then the TESZ writes the following: "The basis of the word family, the verb karcol, is of German origin; cf. German. kratzen "to scratch, to scratch (also in connection with fighting, hostility, also as a word for goldsmiths); to scratch (also with a bad pen), to scribble, to write badly", In Hungarian, the more original form krácol kracol can be confirmed by numerous old data. From this, karcol became karcol, to which perhaps the morphological influence of the verbs harcol, karmol, kapar lele may have contributed... No

574
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"It is likely that karcol is related to karmol or korhol, or is of Hungarian onomatopoeic origin." Let us note here that in the case of karcol, which is quite obviously an onomatopoeic word (also in German), the TESZ does not apply the fundamental principles of onomatopoeic linguistics. methodological rule that onomatopoeic words are not cognate, and that they are highly unlikely to be loanwords, since such words developed separately in each language; but simply states, without any evidence, that onomatopoeic origin is "unlikely",

But let's go further. According to the TESZ, our word Karmol belongs to the family of the onomatopoeic verb kar-, to which karistol (to scratch, scratch), and perhaps korhol also belong. The inferred relationship of kar- to the root har- hor- of the words harcol, harizsál, horzsol, etc. requires further investigation - states the TESZ (under the heading karmol).

For the heading harcol we find this: "The basis of the word family, the verb harcol, is a derivative word related to harzsol, horhol, horzsol. It may also be related to the harsan family."

At the same time, the formation of horh, horhos (waterlogged ditch, deep road; Hohlweg) is not sufficiently clarified according to the TESZ, and the only thing certain is that it belongs to the family of verbs horzsol, harzsol, harcol, horhol, etc.

However, the TESZ writes about the words horzsol, horol, horhol: The word family hortöve is an ancient onomatopoeic word from the Finno-Ugric period..."

The word family harsan, harsog, harsány, etc. is also onomatopoeic, a low-pitched version of the family herseg, hersen, hersent — notes TESZ.

In short, it can be clearly stated that the TESZ is rather groping in the dark regarding the connections between all these words, i.e. the similar meaning of the har-, hor- (her-); kar-, kor- stems and their derivatives, and cannot clearly state their position regarding the origin of these words. Where there is some Finno-Ugric parallel, they seize it with both hands, and some words, forcibly torn from their environment, suddenly become Finno-Ugric in origin (e.g. hor-), while where there is none, they remain ordinary onomatopoeic words, among which, despite their indisputably onomatopoeic nature, German loanwords can also be found (karcol).

But we can go even further in examining Hungarian words with a related meaning, stemming from the "hr-". This time, we will also present the word explanations of the TESZ for the first time.

Anger (Zorn): derivative word; the root word har- is an ancient legacy from the Finno-Ugric period.. The explanation of the verbs horzsol, harzsol, harcol from the root hor- har- is unlikely.

Hergel (to irritate, annoy, anger): onomatopoeic, related to the verbs herreg (to grumble) and hörög. (Note: although every native Hungarian reader will immediately think that hergel is probably

375
===== PAGE 566 =====

(It is just a high-pitched version of harag, haragft, this possibility is not even mentioned in the TESZ.)

Bite (beissen): probably an ancient legacy from the Finno-Ugric era

Harap (debris, debris of fallen twigs, dry grass, harasz): probably a derivative word, the presumed root har- is of disputed origin; or ; an ancient legacy from the Finno-Ugric period or its root is identical to the root of the verb harap (beissen).

Haraszt (Chopped dry foliage, litter; litter-grass pasture; green foliage): of Slavic origin. (Note: cf. harap = "uaz")

Snore, snore, snort, snort, etc.: a family of words of onomatopoeic origin..., related in terms of onomatopoeic expression to the family of hurit, hurrog, hörög, etc.

Groove (engraving; kerben), groove (groove; Kerbe): word family of uncertain origin.

Hook (hook, deep path between mountains, hills, strait; Engpass): of unknown origin. Its possible connection with the word horh requires further investigation.

Loop: of Turkish origin. (Note: the obvious semantic connection with the word hook is not even mentioned in the TESZ.)

Finally, we will also mention a wandering word, we will see why below.

Haramia: of Serbo-Croatian origin... In Serbo-Croatian, the Ottoman harami, "roadside, robber"? (+ Arabic "harámi" = "ua."s. Arabic "harám" = "that which is forbidden"). Etymologically related to harem 5260 1 (Note: it will

soon become clear that this derivation is fundamentally wrong.)

So, among the words with the "hr-" root with parallel meanings listed later, we find almost every variant, from Finno-Ugric heritage, through onomatopoeic words of uncertain and unknown origin, to Slavic and Turkish loanwords, and even to migratory words of Arabic origin adopted through Serbo-Croatian mediation.

Let us now look at the polysemantic Sumerian "bar, bur, bir-, urs, ir,," = "einritzen, bar" some meanings of the root word, whose cuneiform is sometimes read as bur" s sir)", yýyy and ýý " with the phonetic forms i. kerben" (to scratch, groove); cf. kar-col, kar-mol, kar-istol, hor-nyol, fr (schreiben; cf. 82 with "ir," phonetic forms); "zeichnen" (to draw); cf. ir (zeichnen; yý, "ir,");

"Krümmung, Ring" (curvature, ring); cf. hor-og (hook); "schreien, brüllen, dröhnen, Lärm, Geklirr, Getöse" (shout, clamor, jingle, noise, clang, zenebona); cf. har-sog, har san, hur-it, hurr-ant, her-scg, kur-jant;

"zcrribcn, zermahlen, zerkaucn" (to grind, grind, chew); yý, hor zsol, hor ol, hor hol, har-ap (to bite), ór-ól (compare with the sounds "ur");

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576
===== PAGE 567 =====

"Futter" (feed); cf. har-ap (avar, dry grass), har-aszt (uaz);

"Schlinge, Netz" (noose, net); cf. hur-ok (and perhaps hál-6? > PT

"Berg" (mountain); cf. hór-i-(hor-gas), or-om (cf., "ur, ý7

"Bachtal, Schlucht" (stream valley, narrow mountain pass, ravine);

hor-h, hor-hos, hor-og (deep road, mountain pass);

"ersehen" (to see, notice, discern); cf. gór-é, In compound: "har-ra-an" = "Weg, Strasse, Zug, Reise, Karawane"

(beat, street, line, procession, march, journey, caravan).

Furthermore, Sumerian "bara" = "Bösewicht" (rogue, villain); cf. har- ag, gar-ázda, gor-omba; but apparently the root of the word hara-mia also belongs here, which in all probability is a fusion of the meanings of the words "bara" (rogue) and "bar-ra-an" (road, journey, caravan): haramia = A "roadside scoundrel, caravan robber".

So it can hardly have much to do with the word "harem", especially since the The original Semitic meaning of "harem" is not "forbidden", but the clear evidence of the Akkadian words "haramu" separate), "harem, harmu" (Geliebter, Gemahl; lover, spouse), according to, meant the (absondern; harimtu" (Hure, Buhlerin. Hierodule; whore, concubine, temple prostitute)

separated, enclosed, fenced-in residence of concubines, wives. Cf. also Sumerian, gar? = "rings umschliessen, umzáunen, Umzáunung" (körülzár, körülkerít, fence) and "kar," = "Einschliessung" (körülzárás, separation); Hungarian kerít, fence, kárárn. This latter word is therefore a relative of "harem", not of the We cannot now deal exhaustively with further Hungarian parallels of these Sumerian words, because this would lead too far haramia".

from our topic this time. However, we would like to show through an example how close the connections are between the Sumerian and Hungarian languages.

We have seen that one of the readings of the cuneiform sign "har" is "ir," 5 which exactly corresponds to both meanings of the Hungarian word "ir-": "einritzen; karcol" (to write with a pen) and "zeichnen; zejsal" (to write an egg).

Now, the full semantics of this Sumerian word looks like this: sir" = "einritzen, zeichnen" (to scratch, draw); cf. Magyar. ir

5 ("write" 68 "draw");

"near, approach, come here" (near, approach, come here); cf. Magyar. el-ér, er-kezik, er-kezik;

"antasten" (touch); cf. Magyar. ér-int;

"Verstand, Gemüt, Gesinnung" (intellect, sentiment); cf. Magyar. ért-t, er-ez;

"Konto, Vermögen, Guthaben" (account, wealth, claim); cf. Magyar. worth something; worth,

It is essential to mention that this picture hardly changes if we include the ancient Greek language in our investigations. The above observations are

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We can find a good part of the phonetic and semantic parallels there as well. Let's look, for example, at the meanings of the ancient Greek verb "air" ("haire-"): air raita ér;

"pea tap" (rajtakap)

"to win" (to attain);

"hochschätzen" (to value more) means to value more; "überkommen, überraschen" (to overtake, to surprise) means to be in trouble; "einholen" (to catch up) means to catch up;

"seize" (megfog) Er-int;

"to join, to attach" (to join, to attach) to, to touch;

"to convince oneself" (meggyőződik) er-t;

"to grasp mentally" (to grasp spiritually) to understand with the mind;

What "coincidences" can explain these organic phonetic and semantic parallels between Sumerian, Ancient Greek and Hungarian?

These excerpts clearly show that the phonological linguistic methods used so far are not suitable for recognizing real archaic, linguistic-historical and semantic connections, but on the contrary, they lead to the unsystematic fragmentation of semantically organically related word stems and word families.

Let's go back a few pages and observe, for example, the relevant etymologies of the TESZ for Hungarian words that are semantically indisputably of Sumerian origin.

"har" stem has a different suffix "hr-"

Finally, among our loanwords attributed to Bavarian-Austrians, we will examine the more distant connections of our words kanna and kanta, because from a general linguistic historical perspective, these also provide a lot of lessons.

Let us first look again at what the TESZ knows about these words. Kanna (1621: kán) "Häferl, Töpfchen, Eimer, Kanne, Hohlmassenheit, Trinkbecher": of German origin, with multiple borrowings. Its ultimate source is the Sumerian-Akkadian gin, reed". The latter reached the English language through the Babylonian-Assyrian ganu, reed", the Greek channa, reed, reed stalk, reed mat, mat".

lat. canna, it continued to be , reed, reed stalk". This Latin word, as an ancient migratory word, "nădszăr"> . pipe", was transferred to Germanic with a change in meaning, and to Latin sa , vessel with a spout; through the meaning of v. used as the name of various liquid-holding vessels in the various Germanic languages.
Kanta (pot, jug, measure of space, female genitalia): of Bavarian-Austrian origin; see Bavarian-Austrian Kante "pot, kanta", its ultimate source is the Latin canna, just like kunná. The circumstances of its adoption are unclear, especially in connection with the fact that it initially appears as a more Eastern dialect word; the meaning "female genitalia" is a Hungarian development.

In connection with the latter opinion of the TESZ, we recall our already obsolete word kantair (medicine, magic potion, magic potion),
578
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which was undoubtedly some kind of love charm, a magic ointment; 62 however, he argues that the word kanta should be considered a The meaning of "female genitalia" cannot in any way be a late Hungarian development.
Starting from this semantic parallel, let us also take the word mare, which is strikingly similar in its phonetic form, and whose meaning also shows a close connection: "female, female animal", "Mare" (Stute, weibliches Tier, Weib – sexually) and "kancds" (lunatic); of Slavic origin; cf. Czech konice "mare". It is striking that no corresponding formation can be demonstrated from other Slavic languages, although the continuation of the base word, the Proto-Slavic "konj, horse" is everywhere
— writes TESZ about this word.
At the same time, the jug (Krug, Kanne), which also obviously belongs to the words kanna, kanta, is of unknown origin in the view of TESZ, and its explanation as a derivative of kanna and kán (pot) is unlikely.

If we look up the corresponding German words (Kanne, Kante, Kandel, etc.) in Kluge's etymological dictionary, we will also find the source of the science of TESZ.
Our linguistics — in accordance with its habit that has been ingrained for a century now — has uncritically adopted the wise conclusions of Indo-Germanic studies, which are considered infallible, without having originally verified their reliability with the help of Sumerian-Akkadian linguistic monuments.
In Sumerian, "nád" is not , gi" = Akkadian "kanu" (~~qanab~~; ~~Thes~~ ~~Ufretian~~ , gin" (gen, gi-in, gi-en) means, on the one hand, "gehen" (compare Hungarian yón és — , gin", but horrible dictu — German gehen, pronounced géen), and on the sein" (hard, solid), which is Akkadian "kanu", i.e. without a dot, "y", i.e. not "q", but an ordinary "k"
(1. Deimel: SAG and ASG, and Soden: Akkadisches Handwörterbuch). It is therefore obvious that Kluge and the later revisers of his dictionary mixed up the two Akkadian words, did not pay attention to the dot under the "k", and instead of "kanu" used the Sumerian sound value of "kanu", for "reed". Their Hungarian copyists naturally also fell into the same mistake. The word "gin" was interpreted

Below we present the kanna, kanta (also as in some ancient languages. female genitalia") mare and jug have real phonetic and semantic correspondences in Sumerian, Akkadian and even

Sumerian "gan", "kan" and "kunin" = "Kanne" (jug); from which: Akkadian "gannu" and Kanne" (jug). jug" =
Sumerian "gannum" = "eine Art Krug" (a kind of jug, pitcher); from which: Akkadian "kankannu" = "eine Art Krug" (a kind of jug, pitcher).
As we can see, not counting the above-mentioned mellőfogást, there is no need for any "nădszálak" and transfers of meaning, because the above words clearly meant kanna, jug in both Sumerian and Akkadian.

579
===== PAGE 570 =====
But we can go even further: Egyptian "hanu" = "ein Gefäß" (a vessel); Aramaic "kan-kan-la" = "Krug" (jug, pitcher); Ugaritic "knkn" = "Krug" (jug, pitcher); Hebrew "kan-kan" = "Krug" (jug, pitcher).

However, all these words are of course out of the question in the view of "tone-law" linguistics when deriving the words in question, because there is no sound development": kanna, kanta, kancsó still sound almost the same as they did 3-4-5000 years ago. In the case of "gin", however, there is both sound development and meaning development, so this is a much more "scientific" derivation. Its only flaw is that it is in direct contradiction to linguistic facts, to specific ancient linguistic monuments. However, for tone-law linguistics, this should not be an obstacle: if linguistic facts contradict the theory, well, then The special flavor of the matter, however, is that the editors of the TESZ did not even conscientiously look into the origin of the word Kanne in the Kluge dictionary. The transfer of meaning in question (reed pipe) also the worse for the facts."
took place in Latin, according to the Kluge-szótár, and not only in German, as the TESZ states; the meaning of the word canna "Kanne" can be traced back to the 3rd-4th century AD in Latin (1. Kluge: id. m. 21st edition, such a meaning existed in Latin much earlier.

"Kanne" under the heading). Knowing the above ancient words, we strongly suspect that it is already

However, the other meanings of the Sumerian word "gan" (gana, kan) are no less interesting for us. Its basic meaning is, on the one hand, "Gestell, Rohrgerät zum Einstellen von Spitzkrügen", i.e., "stand, structure made of reeds for placing pointed-bottomed jugs" (from which the figurative meaning "canna, jug, pitcher" comes), and, on the other Band, Binde, Seil" (strap, twine, rope), and on the other hand "trächtig, gebärend" (pregnant, bearing, giving birth woman or female). Thus, we can conclude that one of the primary meanings of the Sumerian "gan" has been faithfully preserved to this day by the Hungarian word kanta meaning "female genitals" (which TESZ typically considers to be a late development), kanta-ir (charming, magical ointment — apparently to facilitate conception, pregnancy or childbirth), and kante (female; in a sexual sense). The other basic meaning is most likely carried by our word kantar: , a net made of strap, twine, in which objects are carried" (cf. "gan, kan" = "Band, Binde, Seil", i.e. „521[, twine" and "Rohrgerät zum Einstellen von Spitzkrügen"). From this latter basic meaning, the meanings of kanna, kanta, and jug arose secondarily — but still in Sumerian — because the net made of plant fibers, or the stand woven from reeds and sedges, were primarily used to suspend and support vessels in prehistoric times.

Accordingly, one of the basic meanings of the Sumerian word is also a regular 6201068 parallel to channa (reed, mat), and át-580

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in the first sense kantharos (handled jug), and in the second gyne (woman, married fact) and genne- (to give birth, to be born); cf. "porphyrogenetos" = From the Sumerian "gan" born in purple".
(male) and its phonetic version, "gam," we briefly mention 3 2 lat. genus "Sippe, Geburt, Geschlecht" (genus, birth, sex); old root gamos "geschlechtliche Vereinigung") sexual union, intercourse); Old Indic kama kamatylol and gim (female deer, female animal). "Liebe" (love — in a sexual sense); Hungarian

According to these, there is little doubt that our words kancsó, kanta (kan- na), kantair, kanca (mare), kantár, as well as kamatylol and gim belong semantically to the same family. At the same time, the TESZ divides these Hungarian words as follows: kanna, kanta: German loanwords of the same origin; kantair: of uncertain origin; kancsó: of unknown origin; kanca (mare): Slavic loanword; kantár: probably a Turkish loanword; kamatylot: probably of onomatopoeic-onomatopoeic origin; gim: of unknown origin.
Illustratively summarized (by word family): German Slavic Turkish (?) onomatopoeia-onomatopoeia uncertain unknown
The picture is not much different from the one we have already seen in connection with the Sumerian root word "tag" (Book I, p. 531). The majority of these words are of undetermined origin: 57.1% of them are unknown, uncertain and onomatopoeic; the remaining three words are equally distributed between German, Slavic and Turkish languages (14.3 - 14.3%), where, however, the Turkish correspondence is only probable, and therefore ultimately also of obscure origin. In other words, the proportion of words with an origin that is not completely clearly clarified actually amounts to 71.4%,

Under these circumstances, we should formulate the question as follows: if linguistics could show the Sumerian equivalents of the other six Hungarian words in addition to the II word in each language in German, Slavic or Turkish - and with close phonetic and semantic parallels comparable to Hungarian! + then we could argue about who first took these words from Sumerian, who passed them on to the others and where they came from in Hungarian. However, until such a thing

581
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===== PAGE 572 =====

is not proven, until then we can only assume that only in Hungarian do we find close Syntingual parallels covering all seven words, i.e. our language has preserved the entire semantics of the Sumerian "gan" (kan) in one block to this day. And from this fact it follows compellingly that we can say with a very high degree of probability: the unique words in question reached German, Slavic, Turkish from the language in which each and every meaning variant of the proto-language base word can be detected. -- And not vice versa.

Our Slavic loanwords
We have already mentioned (1. above, pp. 546-549) that the 19th century Indo-Germanic linguistic and historical science saw in the conquering Hungarians — like our closest linguistic relatives, the Voguls and Ostyaks — a fishing-hunting population that had just emerged from the primitive conditions, and who, having learned some animal husbandry from the Turks, roamed aimlessly and purposelessly across the Eurasian steppe like wandering gypsies. In accordance with this assumed low cultural level, it was also considered obvious that the language of such a barbaric nomadic horde was just as undeveloped as its culture. The research into our loanwords also began from this theoretical starting point, after it was agreed long ago that all words of the undeveloped Hungarians at the time of the conquest, which indicated a higher level of culture than the fishing-hunting lifestyle, were taken over from the surrounding, much more developed and cultured Indo-Germanic peoples (1. Book I, p. 63 and this book, p. 548). According to this "method", in most cases it was practically enough to find a parallel for a Hungarian word in the language of a neighboring Indo-Germanic people to declare the words in question as loanwords.

A very good example of this is the His work "Die slavischen Elemente im Magyarischen", in which the author claims the Slavic origin of almost a thousand Hungarian words. Miklosich's work -- methodically proven fundamental refutation of F. Miklosich Vámbéry (l. below), and then in the middle of our century, despite Knieza's sober criticism and in many cases his very skeptical position - it is still largely the Bible of our Slavic loanwords. For example, the latest TESZ refers to it at every turn, and at the same time, Miklosich's idea of 956 Hungarian words of Slavic origin still dominates our linguistics: our linguists unanimously speak of the Hungarian language as having "almost a thousand" Slavic loanwords.
In his book, written with deep scholarship, Miklosich in several cases derives modern words from Proto-Slavic, "phonologically" as well as Hungarian words that have extensive vocabulary in our language.

582

===== PAGE 573 =====

They have a number of derivatives formed from a single root, while in the Slavic languages, only one or two derived words have a corresponding meaning, while the root meaning itself is missing, and they are successively declared to be Slavic loanwords. All this is not said by us, but by the renowned Hungarian linguist József Balassa, who is also recognized by Finno-Ugric studies (1. Balassa: Die slavischen Elemente in Magyarischen; - Review of Miklosich's book in the Ungarische Revue in 1885).
The latter include, for example, shovel, scapula and lapu(leaf), which are still considered to be indisputable Slavic loanwords (TESZ 1), although in Slavic languages, apart from the words lopata (shovel), lopatka (scapula) and lopuch (scapula), we do not find any similar phonetic or semantically related root words, while in Hungarian the root lap has a clearly understandable primary meaning and numerous derivatives: lap, lapos, lapul, lapít, lappad, lappan, lappang, lapogat, lapály, lapótya (lepény), laponya (lapály) and — in addition to these — lapu, lapát, scapula.

linguistics: they simply state that the words listed above have nothing to do with each other, because the root word "lap" belongs to a different word family. What does it matter "shovel", "lapocká" and "lapu", but this does not disturb our that "shovel" is nothing but "flat tool", "scapula" is "flat bone", and "lapu" is also "flat leaf"; all these undeniable semantic connections are pushed aside, obviously only because even today they essentially use Miklosich's methods.

Yet Miklosich has done little to the language, 956 of which he declared to be Slavic loanwords — apparently with the help of a Hungarian-German dictionary. In his book, he repeatedly exposes the pitiful state of his Hungarian language knowledge. His following definitions of words, for example, testify to his truly thorough knowledge of the Hungarian language: kosz = "Haar" (hair, coat); tilos = "ein geschlossener Platz, wo das Vieh weidet" (a closed place where cattle graze)
cattle grazing).

It is clear from these translations that Miklosich had no idea of the real meanings of the Hungarian words he derived from Slavic languages. The basic meaning of kosz is known to be "dirt", but it is also used to name scabies (ótvar; "Grind, Krätze"), which often - but by no means always! - occur on the hairy scalp. The German name for this form on the scalp is "Kopfg grind, Kopfk rätze" (fejkosz. [jy]yyyyy). We can therefore follow Miklosich's etymological wisdom exactly, which is obviously a mere Hungarian-German

583

===== PAGE 574 =====

He drew from a dictionary. Since he discovered the German word "Grind" as one of the meanings of dirt, and since this skin disease often occurs on the head — "Kopfg grind" — he immediately fell for it, since Slavic also has a word with a similar sound: kosa (hair) and its derivative, kosmaty (struppig, haarig, behaart; shaggy, hairy). After this discovery — at least for Miklosich — it was child's play to "reconstruct" the missing semantic connection: kosz = "Grind", which is often "Kopfg grind", but there is hair on the head; why shouldn't the actual meaning of dirt be hair? All the more so, since it can be so nicely matched with the Slavic word kosa. However, since the Hungarian kosz can never mean "head", much less "hair", but only a scum (Grind, Krätze, Schmutzflechte), Miklosich's science had not spread until then. The relevant article in the TESZ (under the heading 1. kosz) is also particularly characteristic in this regard, which, although it already rejects the Slavic derivation of the word, still gives the German meaning of kosz as "Kopfg grind" - unmistakably under the suggestive influence of Miklosich and his similar followers - even though the exact translation simply did not bother to look up the exact meaning of the word in a Hungarian-German dictionary; it simply deleted the incorrect translation of the earlier literature following Miklosich's lead.

Grind". So the TESZ article writer still

In Hungarian, we are once again faced with a case of the kind we have already encountered in the "sound history" derivation of the German word "Schraube" (screw) from the Latin expression "sus scrofa" (sow) (book 1.1, pp. 127-128). As we saw there, this absurd etymology has already affected the "newer" Latin vocabulary, since in the Latin dictionaries published in recent times we find the meaning of the word "scrofa" simply as "sow", while at the same time the actual basic meaning - tumor, lump, nodule - is not even mentioned; well, in the same way, Miklosich's etymology of "haj" - at least in the view of TESZ - also changes the exact meaning of the Hungarian word "kosz" to "fejkosz" (Kopfg grind), despite the fact that TESZ itself qualifies this interpretation of Miklosich as erroneous. basket =

Let us note, by the way, that the filamentous fungus According to the TESZ, one of the Hungarian meanings of kosz is "skin disease caused by a filamentous fungus". Well, we believe that our people always called it "kosz". The would have been diagnosed with a microscope before a verrucous skin disease, even a specialist cannot recognize filamentous fungus can only be detected with a microscope, so with certainty with the naked eye. But anyway, in Transdanubia, for example, scabies is also mostly called scurvy, so there is not such a sharp distinction as the TESZ believes.

In the second case, Miklosich's shocking lack of knowledge in the field of the Hungarian language is even more blatant. Not only does the 584

forbidden"

===== PAGE 575 =====

He does not know the basic meaning of the adjective, but in addition, he explains the metaphorical meaning of "tilos hely" (forbidden place) that can also be used as a noun, 18 in the opposite sense. Here again, the "source material" and "work method" are clearly visible; in the Hungarian-German dictionary, he saw that one of the meanings of "tilos" is "Gehege", a word that in German primarily means a game garden, but also a fenced alpine cattle pasture 1% 1 However, he did not know that the translation "Gehege" only gives a very rough approximation of "geschütztes Gebiet". The alpine "Gehege" is indeed a cattle pasture, but in our case it is not to herd, because they would trample and graze the sowing, the meadow intended The word "tilos" has a figurative meaning, and in our language it can only be used in the sense of "tilos" as "forbidden, protected area", "verbotenes, for mowing, the freshly planted saplings or the like.

"forbidden" place where cattle graze, but precisely where animals are not allowed to go

Miklosich probably had the Slavic word "tok" (Gedränge; crowding, bustle) floating in his mind's eye, and that is why he wanted to connect "tilos" at all costs to "tilos", where of course "crowding, with the concept of "confined space" crowding" prevails.

These examples clearly prove that the methods used in the study of loanwords in the Hungarian language were exactly those described above: a single phonetically similar Slavic word was enough to "adjust" the meanings of the corresponding phonetic Hungarian words to them — even in the most convoluted and hair-raising way. It is also obvious that what Miklosich did did not even meet the elementary requirements of science, even in the methodological context of 19th-century linguistics. The least we can expect from a linguist studying loanwords is to be at least aware of the correct use of the dictionaries of the languages he compares and to be able to separate the individual meanings of the words from each other. However, anyone who translates "tilos" as "cargo pasture" is clearly unable to meet even this primary requirement.

"dirt" for "hair", the

It should also be noted that Ármin Vámbéry held a similar opinion in almost everything, and already in 1882 he emphatically pointed out that Miklosich's work is based on a faulty understanding in its basic idea and, on the other hand, reveals errors and mistakes in its elaboration that result from the author not taking the material of the sources into due consideration." (I. Vámbéry: The Origin of the Hungarians, p. 401)

By the erroneous understanding of the basic idea, Vámbéry means -- as we will see below: with all right -- that Miklosich did not make a distinction between loanwords in the real sense and foreign words, and this included the latter among loanwords: "...for example, foreign words

===== PAGE 576 =====

I am looking at the following: abanajcz, angor, beläka, bervény, bobujics+ ka, ekebabala, gornyik, haräk, izlot, izsgäncz, etc., which are perhaps exceptionally understood by some in the Hungarian region in the immediate vicinity of the Slavic language area, but which appear to be completely foreign to the greater part of the Hungarian nation, "which is why one third, if not half, of the words listed in his list must be omitted, that is, he considers them to be: ni, which occurs partly as a foreign word in Hungarian, and partly as something completely unknown today" — Vámbéry emphasizes (id. m. 402, p. - emphasis by Vámbéry).

Now, of the words mentioned above by Vámbéry -- presumably as an implicit consequence of this very criticism (I would not openly admit for the world that Vámbéry might have been right about something, because the "official" thesis is that Budenzék "really" overthrew Vámbéry) -- in the latest TESZ, angor, beläka, bobujicska, ekebabala, izlot and izsgäncz are no longer found, but abanajcz or abajdok (mixed, mixed, waste grain), bervény or berveng (winter green; --plant name; -- said to be of "most likely German origin"), gornyik (forest-skipper, village servant) and haräk (revalued as onomatopoeia). At the same time, however, the TESZ also mentions a large number of "Slavic loanwords", which are absent from Vámbéry's list above, but their marginal, regional language character, limited to certain narrow areas of the Hungarian language, is completely obvious. For example, the following - to mention only a few words beginning with "a" and "b" - are: abronica (pot-carrying net, water-carrying rod), azsag (fire-starting rod, index finger, policeman), badär (beautiful, mischievous), bagazia (painted canvas, skirt), bobajka (Christmas cake made from yeast dough), bobolnyik or bolonyik (frog jug; - plant name), bojnyik (thief, road-dweller), etc. etc.

From these few examples alone, we can undoubtedly conclude that Vámbéry's objection was indeed well-founded, and that even today our linguistics is filling the number of our Slavic loanwords to almost a thousand with a series of words (more sensible criticism, mainly under the influence of István Knieszsa, has since sifted out quite a few of Miklosich's original 956 words), which are completely unknown in most of the Hungarian-speaking area, and are understood only by an insignificant proportion of our people in the contact zone of the Hungarian-Slavic settlement area. Moreover, not one such word occurs only in some older documents, and - as Vámbéry 15 notes - was no longer in use anywhere in the 19th century.

An integral part of all this is the fact that the editors of the TESZ clearly state in the introductory part of volume I: due to the nature of the work, the colloquial material "... can only be avoided with strong selection"-586

===== PAGE 577 =====

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"put into the dictionary", namely only the most important of the tölj words (vol. 1.1, pp. 8-9.). However, according to the title words of the TESZ, the editorial staff only met this objective in the case of specifically Hungarian words, since Hungarian regional dialect words are largely absent, but at the same time, we find not only a mass of Slavic words similar to Fentick, which are indisputably regional in nature, in the dictionary, but also most regional words of German origin, which are only used in the immediate vicinity of the Swabian settlement areas, and even modern international words of Greek, Latin and other origins that are explicitly scientific technical terms, which obviously have no place in a scientific work such as "The Historical-Etymological Dictionary of the Hungarian Language"

(emphasis added), since these words have only become more or less known to the wider strata of our people in the last decade or two, and therefore have no significant impact on the Hungarian language. As for the insufficient consideration They don't have a "story".

of the source material, Vámbéry means by this Miklosich those Slavic loanwords which are also found in the Turkish languages, and therefore cannot be considered simply of Slavic origin, but which must either be considered "an ancient property of the Hungarian language, or which, as later borrowed words of Turkish origin, came to Hungarian either directly from Turkish or indirectly from South Slavic. So far, we have selected more than fifty such Turkish-Hungarian words of culture from Miklosich's list, which are mistakenly considered Slavic or came to us through Slavic means; these words are listed in Appendix V." (Vámbéry: id. m. p. 402.)

Vámbéry finally summarizes his criticism as follows: "... of the 956 examples mentioned by Miklosich... almost half are not even Hungarian, more than 50 can be classified as Turkish and perhaps the same number as Finno-Ugric vocabulary, so that at most ONE THIRD of the 956 words mentioned can be said to be Hungarian words borrowed from Slavic." (id. m. p. 405 — emphasis by Vámbéry)

Among Miklosich's other fantastic word explanations are very good examples in Vámbéry (1. id. m. 403-405. and 656-661). For example: kender-kadr (rotates, revolves); bojtár ~ bajta (house, hut); szín (car color) ~ sjen (shades), etc.

We would also like to briefly mention that in order to properly explain Miklosich's scientific role, we must also take into account, in addition to the above, that he was among those sent to Pest by the Bachs after 1849 to "gleichschalt" Hungarian linguistic and historical research, to direct it exclusively in an "Indo-Germanic" direction. However, this aspect of Miklosich's work is a strictly cultural-political issue, and we cannot deal with it in more detail within the framework of this volume.

287
===== PAGE 578 =====

Let us look at how our linguistics works in this field today. Above, in connection with the words lapát, lapocka, lapu, we have already mentioned that Miklosich first introduced

"His methods" are still alive today. In many cases, our linguistics still insists, without criticism, on Miklosich's once completely one-sided

"to szószármaztatásai-, and as a result, our loanwords, which we considered Slavic, have remained almost unchanged since then. A very good example of this is our German word. TESZ methods".

writes about it as: a derivative, or perhaps a takeover of a Celtic ,..... Slavic origin; vi.... syn. (Slovenian) German; slk. (Slovak) German ... the word or gender & adjective "unable to speak" tribal name."

Well, as we have already mentioned, this Celtic tribal name sounded like NEMET — however, TESZ carefully and "expediently" omits this — but "Nemetis" was also the name of a) a Germanic tribe in Roman times; b) the city of today's Speyer under the Romans; and c) a Germanic troop unit of the Roman army (Book 1, pp. 536-537). The TESZ article writer has obviously not heard of all these things, although the tribal name "Nemetis" is also found in Pliny — unfortunately not in Miklosich...

In short: these data clearly indicate that the German folk name has a very specific ethno-onomastic basis, and can hardly be derived — even among the Slavs themselves! — from the word "néma". The NEMET tribe, originally a Noricum Celtic tribe, later Germanized according to the evidence of the source data, is a historical reality that can be dated in multiple ways, and the former phonetic form of the name — miraculously — is not the Slavic némec, but the Hungarian word "néma" identical letter by letter. It certainly reached the Slavs in the East through the Huns and the Avars, who adopted it distortedly into némec (here perhaps the similar phonetics of the word "nemel néma" also acted as a folk etymology), while its original phonetic form has remained with us. This is also confirmed by the fact that in the Slovenian and Slovak languages, which were exposed to strong Avar-Hungarian influences, "némec" still sounds like "nemec", and in other Slavic languages it sounds like "nyemec", "nyemci".

In this regard, however, it is essential to point out that our German word, which has preserved its original sound unchanged, is also significant evidence to support the thesis of a double conquest. It makes it very likely that the word was directly incorporated into Hungarian. This is only possible for the Hungarian-speaking population of the Avar period.

groups; a significant number of peoples, according to them, truly understood the conquest and passed on the correct German pronunciation to the conquerors without interruption,

We can also experience a superficiality similar to the previous one in the Slavic derivation of our word néma. The TESZ firmly states that it is a Slavic loanword (vy, 66. szl. német ; szb.-hv. nem, néma, némo; 588

===== PAGE 579 =====

etc. unable to speak, mute"), although at the same time he notes: "The origin of the Slavic word is uncertain... The Slavic adjective was transferred to Hungarian in the feminine form. Its direct source however, cannot be determined."

In Hungarian: a) from a Slavic word of uncertain etymology; b) from its feminine form they derive the adjective "néma" in the Hungarian language, which does not know grammatical genders; c) and moreover, they cannot even determine the direct source of the word. And they do all this despite the fact that the excellent István Kniezsa repeatedly emphasized: until the etymology of a Slavic word is clarified, we cannot say for sure that Hungarian words with the corresponding phonetic form and meaning have a Slavic origin.

We can also add to this that in the Sumerian language, "nu" = "Negation" (negation); "nu,,. ne,," = speech, speech sound); in compound not" (nem); "me, ma," = "speak, language, voice" (beszél, form therefore: "nu,-me" or ".,ne,-ma," = "does not speak".

However, the most important circumstance in the question of our alleged Slavic loanwords is that the generally accepted historical and cultural-historical ideas of the 19th century have fundamentally changed in the light of recent archaeological and source research. Not only in the sense that it has been proven beyond doubt about the conquering Hungarians that their culture was in no way inferior to the cultural development of the surrounding peoples; but at the same time, the other side of the question, the historical and cultural relations of the ancient Slavs, have also been put into a completely different light.

First of all, the 19th-century linguistic and historical research methods of Slavic studies have been sharply criticized by many in international scholarship over the last 50-60 years.

We have already presented Miklosich's unqualified methods ourselves, and we also mentioned the legend of "Great Moravia" in Chapter 1 of our Volume II (Book II, 1, pp. 218-219).

Let us now look at the opinions of some 20th-century foreign researchers on these issues.

In his treatise "Die Ortsnamen der Sudetenländer als Geschichtsquelle", E. Schwarz states that the sound changes and kinship relations of the Slavic languages were examined very superficially at that time, in the 19th century, and in many cases they arrived at very hasty "assumptions". For example, several alleged sound changes of the supposed Proto-Slavic language were treated as facts without any evidence, and even despite such evidence, tacitly, because this was the only way to bring the hypothetical common ancient basic language into line with the laws of sound.

In his work "Byzantinische Geschichte", AD Zakynthinos repeatedly refutes several previously generally accepted and, in older literature, almost commonplace false beliefs about the early history of the Slavs. For example, he writes, until recently historians believed that the Byzantine emperors Justin (518-527) and his nephew Justinian (527-565) were of Slavic origin. The

589
===== PAGE 580 =====

However, modern research — primarily by J. Bryce — has proven beyond doubt that the sources on which Justinian's Slavic origin was based are "... falsified texts from the 16th and 17th centuries, originating from the Pan-Slavic school of Dalmatian intellectuals from Ragusa." (Zakynthinos: id. m. p. 36)

Later he states that "... when the southern Slavs came into contact with Byzantium, their social organization and culture were still in a rudimentary stage." (id. m. p. 54) Larger-scale Slavic incursions and invasions only took place after the appearance of the Avars, accompanied and under their leadership, they first attacked the Byzantine Empire in an organized manner: Gebiete an und verursachten grosse Zerstörungen." (id. m. p. 54-55) In Hungarian: "As executors of the Avars" Under the leadership of this Asian people, the Slavic tribes, acting as executors of the Avars' war plans, repeatedly attacked Byzantine territory.

war plans, under the leadership of this Asian people, the Slavic tribes repeatedly attacked Byzantine territories and carried out great destruction."

He then explains that the Slavs appeared in the Balkans in the 6th century AD, but we only have data on larger-scale immigration from the time of Emperor Heraclius, at the beginning of the 7th century (ibid., p. 73).

Zakynthinos then refutes the idea of an alleged mass influx of Slavs into Greece. This thesis was spread by Ph. Fallmerayer in his book "Geschichte der Halbinsel Morea während des Mittelalters", in which Fallmerayer claimed that the inhabitants of present-day Greece are of Slavic origin, and "... not a drop of true, unmixed Hellenic blood flows in their veins". However, recent research and modern anthropological, ethnographic, linguistic and folkloristic studies have made the errors — if not the falsifications — of Fallmerayer and his followers obvious" - he notes (ibid., p. 74).

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After the turn of the 7th/8th century, Zakynthinos continues, the Bulgarians in the Balkans increasingly extended their influence over the leaderless Slavic world, taking over the declining

"The legacy of the Avar Empire." (id. m. p. 88)

In this regard, we should point out that at the same time the Onogur-Bulgarians also played a very significant role in the Avar Empire itself. Around 670-680, the "griffes indäs" appeared in the Carpathian Basin. After the collapse of the Onogur-Bulgarian empire of Kovrát, they arrived in "Pannonia" under the leadership of one of the Kovrát sons, presumably Kuber, at about the same time as the Aldanubian Bulgars of the other Kovrát son, Asparuch, and the tribe of the Kovrát son. We recall Gyula László's study "On the History of the Sons of Kagan Kovrát", in which a third, unknown name, settled in the territory of the "Exarchate of Ravenna" the presence of the Avars and later the Onogur Bulgarians in the northwestern Balkans and their

590
===== PAGE 581 =====

We can find evidence of its important role in 162652011, onomastic and source-based 15, with the more recent relevant literature 08014. Of particular importance is the statement that at that time both Illyria and Dalmatia belonged to the Ravenna exarchate, and it is much more likely that the Onogur tribe in question settled in the Western Balkans than in Italy.

However, we must not forget that in the second half of the 6th century, the Avar Khagan Bajan settled 10,000 Kutrigur warriors in Dalmatia, mainly in the Salona region, who, according to Emperor Constantine the Great (De administrando imperio, chap. 31), remained there and formed a significant ethnic component of the Croatian people.

As Gyula László also states in his aforementioned article, based on our current knowledge, it seems certain that "... in the last quarter of the 7th century, a real Onogur settlement belt separated Byzantium and Avar from the Adriatic to the Black Sea." (l. Gyula László: id. m. Hungarian prehistoric studies, p. 229)

By considering these sources, H. Gregoire came to the very reasonable conclusion that the name of the Croats (kroat, chrovat) comes from the name of the Onogur-Bulgarian khagan Kovrát (Kobratos) (L' origine et le nom des Croates et des Serbes; Byzantion 17. 1944-45.). Although this assumption of Gregoire has recently been rejected by several people, these researchers have not been able to find a better etymology for the Croatian national name either; each of them suggests a different interpretation — notes Zakynthinos (id. m. p. 73).

We are convinced that these primary sources best capture the most important ethnogenetic and historical components of the development of the later South Slavic peoples. Especially when we take into account that J. Mikkola, the renowned Finnish Slavic linguist, explains the names of the Croatian leaders who moved south, preserved by Constantine the Purple-born, from the Turkish language (I. Mikkola: *Avarica; Archiv für slavische Philologie*, 41. 1927.). However, 19th-century Slavic research did not take into account the fact that the Slavs were continuously ruled by steppe peoples of "Altaic character" (but not exclusively Altaic-speaking — 1. later, when discussing our Turkic loanwords) for almost 600 years from the beginning of the Hunnic conquest of Eastern Europe, i.e. from about 375 AD, and that they were organized socially, militarily and politically by them (1. above, the findings of Zakyntinos). As a result, their languages are full of Hunnic, Avar, Onogur-Bulgarian with loanwords. For example, their titles are mostly "Altaic" borrowings: ban, pan, zhupan, boyar, etc. (I. Randa; *Handbuch der Weltgeschichte*; Jetmar et alii: *Geschichte Mittel-asiens*; and our book II, pp. 227-228). According to Theophylaktos Simokatta, they even adopted the songs and chants of the Avars (see Haussig: *Theophylakt's Exkurs über die skythischen Völker*). With this information, we can

===== PAGE 582 =====

In this connection, we mention that the TESZ naturally says that our word "igric" is of Slavic origin, although the corresponding Proto-Slavic word in this case is only "star", an undatable linguistic reconstruction, and the transmitting Slavic language cannot be determined in more detail. In particular, we find a striking number of loanwords of Altaic origin in Russian. But this is natural, because on the one hand, as a result of the rule of the Pechenegs, Uz, and Cumans in southern Russia, and then the Mongol-Tatar hegemony of more than two centuries, the steppe-Altaic linguistic influences persisted among the Russians for about 500 years longer than among the other Slavic peoples, and the primary borrowings that occurred in the 1st millennium were greatly strengthened and expanded, as if layered on top of them. This fact has also prompted some to make serious scientific attempts to relate the Russian language to Turkic-Mongolian. For example, P. Lukasewitsch (Lukasevics), in his book "angebliche indogermanische Welt" (1873), showed well over a thousand word similarities between Russian and Turko-Mongolian languages, and claimed that Russian was much closer to the Altaic languages than to the Indo-Germanic languages. This was of course an exaggeration in such a categorical form, but the very fact that such a thing could be thought of at all sheds light very well on the mass of Altaic words found in Russian. The book was of course considered unscientific at the time and was not taken into account, but 80 years later, international Indo-Germanic studies itself partially proved Lukasevich right when he pointed out emphatically the numerous Sub-Uralic characteristics found in Slavic languages — primarily Russian — and concluded that the Sub-Uralic languages played a very significant role in the development of Indo-Germanic languages (1. above, pp. 550-552). (page)

Despite all this, Slavic studies generally does not deal with the question of possible loanwords in Slavic languages. In this regard, they still start from the basic premise of 19th-century Indo-Germanic linguistics that developed Indo-Germanic languages did not need loanwords. This situation is clearly reflected in the cautious formulation of István Kniezsa, when — speaking about the importance of loanword research in Hungarian linguistic history studies — he states that "... loanword research plays an incomparably greater role in Hungarian linguistic literature than in any Indo-European language. This difference is particularly striking, e.g. between Hungarian and Slavic languages. Here, with the exception of the only Germanic elements (which, however, are not

"(Slavic researchers have conducted) the question of loanwords is almost completely unknown." (Slavic Loanwords of the Hungarian Language, p. 3) Kniezsa explains this difference by saying that in our country -- in the absence of old linguistic monuments -- the research can only start from the horizon of loanwords when deducing the sound system of the Ancient Hungarian language. However, this interpretation

592
===== PAGE 583 =====

It only illuminates one side of the question, and not even the most essential one; it explains why the careful examination of loanwords is important in Hungarian linguistics -- we do not want to argue about this, we only demand the application of the "equal measure" principle, but it does not even hint at the methods used to determine "our loanwords" at the time, and what is the methodological justification for the fact that Slavic studies, on the other hand, does not deal with the issue of loanwords in Slavic languages? They belong closely here, and therefore we must definitely mention the notorious Königinhof (Kralova Dvur) and Grünberg (Zelena Hora) manuscripts, which were already proven beyond doubt in the 1880s to have been forged by V. Hanka, an employee of the Prague Central Library, at the beginning of the 19th century. 7. G. Masaryk, the later President of the Czechoslovak Republic, and J. Gebauer definitively demonstrated their forgery (1886 and 1888). Hanka "... created a whole 15-century with his forgeries, and the misconception he spread about the past and ancient cultural life of the Slavic peoples has not been completely dispelled even today," writes the Révai Lexikon (under the heading "Hanka") in 1913, i.e. 25 years after the final exposure of the forgeries.

It is a fact, for example, that Palacky, the father of Czech historiography and still the greatest recognized authority, used Hanka's forgeries extensively in his works, and not least with them he supported — among others — the so-called Szatoplukian "Great Moravian Empire" (this name itself reveals its Königinhof origin) also the alleged extension to almost the entire Carpathian Basin; which was then taken over uncritically by Hungarian historical research - paradoxically, however, only at the end of the 19th century, when there was no longer any doubt that the texts in question were forgeries. We recall that in 1861 László Szalay flatly rejected these things", which according to him only "Svatopluk's country extended to the Szerémség" (1. p. 218). Moreover, a significant number of our historians -- cipher even then promoted this view for many decades, products of the "creative fantasy" of Czech historical detectives; however, at the end of the century, Gyula Pauler firmly stated that

But in the same way, the main arguments of foreign and domestic linguists in Slavic studies were these forgeries, referring to the high ancient Slavic culture that could be read in them, when they claimed that the most diverse Hungarian cultural words were loanwords and of Slavic origin. Only in such an atmosphere can it be understood how researchers working with Miklosich's methods could become experts. Moreover, as we saw from the above Kniezsa quote, Slavic linguistics even in 1955 assumed from the outset - obviously as a result of the still vivid psychological reminiscences of Hanka's forgeries - that people with high culture, developed literary

593
===== PAGE 584 =====

The ancient Slavs, who had a common language, did not borrow words from foreign languages, and therefore did not consider it necessary to examine possible loanwords in Slavic languages. The situation in this area has essentially not changed since then. It would be very instructive, however, if we thought a little about the paradox of the history of science outlined above: given a forgery of prehistoric and cultural history, born in a national-romantic atmosphere, with a decidedly chauvinistic outlook, which ignores the surrounding peoples and presents Central Europe of the 9th-10th centuries AD as if no other peoples existed there at that time besides the Slavs; after a certain time, however, objective research exposes the forgery — and by an outstanding scientist of the very people whose heroism it was made to glorify — while researchers of a neighboring people, decades after the exposure, base their investigation of the prehistoric history and linguistic conditions of their own people on the outlook of this forgery...

After the above general objections, let us look at a specific example from the broader context of our words considered to be of Slavic origin. It is known that our linguistics explains our word "gát" as a loanword of Slavic origin. So does the latest TESZ. However, again, the TESZ's remark that "... the etymology of the Old Slavic "gatj is not sufficiently clarified" is very characteristic. In other words, this "word gatj is again not datable, it is merely an inferred, star-shaped" phantom word form, and moreover it has no meaning in Slavic languages. Under such circumstances -- apart from everything else -- we consider it a rather unreasonable procedure to speak of our word "gát" simply as a loanword of Slavic origin (cf. Kniezsa's methodological statement; 1. p. 588).

Apart from this principled consideration, however, there are much more serious reasons 16 that prove that "gát" cannot in any case be a loanword of Slavic origin in Hungarian. If we include the memories of the Sumerian language in our etymological studies, we will come across extremely surprising and no less instructive proto-linguistic connections. We have already mentioned that many Sumerian cuneiform signs have two or more sound values. For example, the pictographic tar the cuneiform sign of polysemantic roots is also the same: tra. The antecedent of "kud" and "ékjd" is, according to Deimel, "der Scheideweg" (the fork in the road); from this image arise the basic meanings: "Weg, scheiden, richten, spalten, öffnen, zerschlagen, schneiden usw." (way, separates, judges, decides, splits, opens, shatters, cuts quickly.). Of the many semantically related meanings of these words, only the complete or almost complete synonyms are of interest to us now,

"kud" sz "to inhibit, tame, block off the water" (vizet ý 94 to stop, to obstruct, to block); cf., dam, "===== PAGE 585 ===== 10. star" = "to stop, restrain, shut off the water" (to stop, restrain, shut off the water); cf. tor-lik, tor lad, tor lastzt, tor-ódik. "kud" = "water breaks through" (water breaks out); cf. well; "tar" = "breaking through water" (kitör a víz); ýý, tor-l, tor ol (hány, okádik). "kud" = "abschneiden" (lcvag); cf. kuty-ol (to prune grapes; Balaton-vidéki tájszó); "tar" = "to cut off, to break off, to pluck off" (to cut off, to break off, to pick off, to tear off, to pluck off); cf. tör, tar-ol, tar; "kud" = "Abschnitt, Schar, Stamm" (section, troop, tribe); cf. had (troop, clan); "tar" = "Abschnitt" (section); v6. stem-zs. "kud" = "to cut wood/herb; bundle of reed grass" (to cut wood, grass, garden plants; to cut off); cf. kot-ú (cut off reed stump), csut-a, csut-ak, csut-ka, csád-é; "tar" = "abschneiden von Holz/Kraut" (cuts wood, grass, garden plants); cf. tor-zsol, tor-zsa, tor-zs, tar-ló (harvested grain field). "kud" = "graben, baggern" (ditch, digger); cf. kot-or, kuty-ul (vájkal), göd-ör, kot-ú (pit), katy-ü; "tar" = "graben" (ditch); cf. túr. "kud" = "a container made of wood/leather" (wooden or leather container); cf. tub; "tar" = "bewahren, hüten, halten, a container" (preserve, protect, hold, container); cf. tar-t, tár-ol, tar. "kud" = "Latentür, Rohrwand" (slatted wall, nád-, mat wall); cf. kot-éc, kat-roc, ket-rec (one meaning "Latentür"); star" = "Leiste, Gestell, Tragbaum" (slat, beam, stand, frame, side beam); cf. tar-gonca, tar-ginca (slatted beam), a corner on a cart). "kud" = "binden, bannen" (bind, bewitch, command a spirit, bind a harmful power, bind), detain, neutralize a destructive force); cf. köt; star" = "binden, bannen" (bind, bewitch, command a spirit, bind a harmful power, bind), detain, neutralize a destructive force, eliminate its effect); v6. tor, tor-ol (original meaning: "an atoning sacrifice brought to the spirit of the dead").

"kud" = "suchen, nachforschen, richten, Richter" (search, investigate, judge); cf. kut-at, kat-at, kot-onoz; but cf. also Kád-ár (the chief judge of the Huns and Hungarians in Kézainál);
"tar" = "to hear, to decide in court, to bring to account, to pass judgment" (to question, to interrogate, to judge, to hold accountable, to hold accountable, to pass judgment); cf. to break (to punish), to law, to break a stick (to condemn); but cf. also Tur-da, Tor-da (in Kézainál Kádár comes from the Turda tribe).
595
===== PAGE 586 =====

It is obvious that all these Hungarian words with the same or largely parallel meanings, stemming from kd- (kt-) and tr-, cannot be separated from each other or from their Sumerian equivalents. In other words, this word family formed an organic unit in ancient times, and later, through the change of the root vowel, suffixes, and root extensions, it branched out from the Sumerian roots kud and tar, which have partly synonymous meanings; to such an extent that the belonging of the word family would not even be recognizable today if we did not have the ancient memories of the Sumerian language at our disposal. Accordingly, according to TESZ, the etymologies of the mentioned words are also completely irregularly distributed.

- 1. dam: Slavic loanword.
- Well 2: of unknown origin.
- 3. kutyol (pruning grapes): this regional word is not included in the TESZ.
- 4. had (troop, clan): of Finno-Ugric origin; the possible basic form is tkunta.
- 5. kotú (nádtorzs): of disputed origin; it belongs either to the family of kotyog (this word is onomatopoeic) or kunkorodik (probably onomatopoeic). It is unlikely that it is classified in the family of csuta. Note: i.e. either way, it is onomatopoeic-onomatopoeic.
- 6. csuta, csutak, csutka: word family of unknown origin.
- 7. csádé, tsáté (sás, giz-gaz, fallen tree branch): of unknown origin.
- 8. kotor: probably of onomatopoeic origin. More about kotonoz and
- The word "kutat" is related to verbs.
- 9. kutyul (digger): of onomatopoeic origin; may be related to the family of kutat.
- Pit 10: of unknown origin.
- 11. kotú (pit): the TESZ takes the word well as one, with the word "kotú" meaning "reed trunk", which is of onomatopoeic or onomatopoeic origin (1. above), and notes that its supposed relationship is with which is unlikely.
- 12. kátyú: of uncertain origin; it may be a variant of kotú, but the connection is unclear.
- 13. kat: of Slavic origin; its ultimate source is the Hebrew kad, "wooden bucket", which is transmitted to the Slavic languages by Greek; cf. the ultimate source of small vessel, bucket, jug" and kados , vessel, bucket, jug". Note: here the TESZ also comes closer to the real the Greek kadión, the Sumerian "kud" (wooden or leather vessel), which was adopted in the Akkadian-Babylonian "kuddu" (guddu) in phonetic form (with the same meaning), and from there it was passed on to the Hebrew bc. -- But even if it really originated from Hebrew, it would still be much more likely that the Hungarians adopted it directly from the large number of Jews living in Khazaria, rather than from the Slavs,

"14 bundles" is a Romanian borrowing; in Romanian it is a South Slavic loanword.
15 cage, katroc: Slovak borrowing; in Slovak it is a Bavarian-Austrian loanword.
596

a see y3 HE wer @ EZ Wr Se "a "ee Tl) Pac k. Tr ee <
wa cur Bt"shsh
yy y-yyy < yy yy — y nr sg y, and
yyy Ee Tr ges yy
===== PAGE 587 =====

- 16. 17.
- 18.
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- 28.
- 29, 30.

Note: the TESZ does not take into account the obvious semantic and phonetic connection between yyyyy and katroc, cage,
kit: Finno-Ugric origin; possible base form *kitke- or *kitke-,
dig, dig: onomatopoeic 6100616 word family; closely related to the verbs katat and kotor.
Kádár: a legendary name; the etymology of this personal name, which has no connection with the Huns, is unclear.
Note: on the basis of what methodological criteria can one decide a priori that a word of unknown etymology is "not in any way related to the Huns"?
torlik, torlad, etc.: the members of the word family are derivatives, but their root word is disputed: either the verb tol (of unknown origin) or the verb torol, torl, hány, okádik (of uncertain origin; 1. below); the latter is much less likely.

Note: i.e. probably of unknown, possibly uncertain origin. torol, torl (vomit, vomit): word family of uncertain origin; probably derivatives of the root word tor- for throat, which is an ancient legacy from the Ugric period.

broken: of unknown origin.
tarol, tar: Old Turkic loanword with Chuvash characteristics; cf. Uyghur taz "Bald" etc.
trunk: of uncertain origin; perhaps a high-pitched version of torzs.
. torzs, torzsa: word family of Italian origin.
stubble: of Old Turkic origin; cf. Tatar tarlau, "arable land".
Note: the "tar", "tarló", therefore, in the view of TESZ, comes from a Turkish word with a completely different meaning than tarol, tar. túr: of uncertain origin; perhaps a legacy of the Finno-Ugric period.

tart: of uncertain origin; perhaps an ancient legacy from the Finno-Ugric period. Based on the richness of its meaning and usage, it can certainly be assumed that the word tart is a very old element of our vocabulary.

tár, tárház (storehouse): the prefix of words compounded with tár is a migratory word; these words spread from Turkic languages.
forklift, forklift: of unknown origin.
torol (ráchen, vergelten, für die Seele eines Toten der Kirche Ablass-dienste leisten; to avenge, to pay for, to do services to the Church for the repose of the soul of a dead person): derivative; probably derived from the noun tor (Mahl, Leichenschmaus), of uncertain origin; perhaps identical with the root word tor- for throat,
Note: the obligatory church services of the funeral pyre and the medieval torlós are demonstrably the "baptized" descendants of the atoning funeral sacrifice of the primitive religion, with which the "return" of the dead was intended to be prevented, that is, to "bind" the dead with magical power, to "enchant" them,
397
===== PAGE 588 =====

31. lawl of uncertain origin; may be somehow related to the Turkish words "törö, töre, etc.", "law, law, custom". 32. Turda, Torda: not listed in the TESZ.
So this time we experience the same thing as with any previous example: these words, which show tenfold similarities and very close parallels with the synonymous meanings and phonemes of the Sumerian roots "kud" and "tar" and extend to both phonemes, i.e. they are undoubtedly in an organic semantic connection with each other, are dissected by the editors of the TESZ in a completely unsystematic manner, do not even suspect their connection, and define them either as unknown, uncertain, onomatopoeic-phonetic origin, or as loanwords originating from the most diverse foreign languages. Moreover, they do not even notice the most striking semantic similarities: cf. e.g. the independent derivations of the words ketrec (katroc) and kotéc, as well as tarol and tarló.

To summarize the etymologies of the TESZ, the derivations of the aforementioned 32 words (more precisely, word families) are as follows: Uralic, Finno-Ugric, Ugric Turkic 3 (tarló, tarol, társ) 2 (army, contract)

Slavic 2 (dam, pen)
German 1 (cage)
Italian 1 (distorted)
Hebrew 1 (tub)
onomatopoeia-onomatopoeia 5 (kotor, 2 x kotú, kutyul, kutat)
uncertain 7 (pothole, tribe, túr, tart, torol, torl, law)
unknown 8 (csádé, csuta, pit, well, Kádár, torlik, tór, forklift)
not listed in the TESZ 2 (kutyol, Turda)

Of the 30 words (word families) found in the TESZ, 20 are unknown, uncertain or onomatopoeic, i.e. ultimately of undetermined, unclear origin. (66.7%), 3 words are Turkish (10%), 2-2 are Finno-Ugric and Slavic (6.7-6.7%), and 1-1 is of Italian, German and Hebrew origin (3.3% each). The fundamental methodological question that we have already encountered when presenting the serial Hungarian parallels of the Sumerian roots "tag" and "gan, kan" (pp. 531 and 581) is raised again: since Finno-Ugric linguistics cannot unambiguously determine the origin of 66.7% of the interrogative words, according to the evidence of Sumerian linguistic monuments, all these Hungarian words belong with a probability bordering on certainty to the Sumerian "kud" and are obviously closely related semantically; on the basis of what methodological criteria can 10% of 6 words "tar" derivatives of stems, and be declared to be of Turkish origin, 6.7-6.7% of Finno-Ugric and Slavic origin, and 3.3-3.3%-4 of Hebrew, Italian and German origin?

598
===== PAGE 589 =====

Of the many meanings of the words kud and tar, we have only highlighted the consonants, we have already met with the twenty Hungarian equivalents of tar above (1, p. 448). These two Sumerian words have a complete semantics, with more than 20 Hungarian equivalents each with identical or largely parallel phonetic forms and meanings, as follows we present in our volume. Summarizing what has been said so far, we can conclude that our Slavic loanwords were defined using very superficial methods in the second half of the 19th century: in the spirit of the Indo-Germanic cultural priority ideas prevailing at that time;

2. starting from the perspective of the a priori assumed uncultured, "wild barbarian" conquering Hungarians;
3. not least under the spell of the "Old Slavonic" texts from Königinhof and Grünberg;
4. moreover, in the knowledge of the historical events and ethnic relations and migrations that took place in Eastern and Central Europe in the second half of the 1st millennium AD, which was still very incomplete at that time. However, it has since been proven that: the thesis of Indo-Germanic cultural priority can no longer be maintained: the cultural state of the conquering Hungarians was in no way inferior to the cultural development of the surrounding peoples; the Königinhof and Grünberg texts are forgeries; the historical, political, cultural and linguistic influences of the Avars, Bulgars and the steppe peoples in general - especially in the field of ethnogenetics and cultural development - were much more significant than those of the Slavs, as these things were thought in the 19th century. We now know that the Slavs lived for more than half a millennium under continuous Hunnic, Avar and Bulgarian rule, as their auxiliary peoples, and consolidated ethnically, linguistically, socially, politically and culturally during this period. In the meantime, it has also become clear that the first political formations that could truly be called Slavic in terms of people and language appeared on the stage of history only after the fall of the Avar empire (early 9th century), in the south and in the Balkans after the decline of the Danubian Bulgarian power (mid-10th century), but even then they were mostly not completely independent, but in the west within the framework of the Carolingian Empire, as its border provinces, largely under the control of leading classes of Avar origin, in the south under Byzantine dependence, and in the north and east under the leadership of conquering Norman-Varangian ruling classes; in Novgorod and later - after the Hungarians withdrew - in Kiev under the rule of the Ruriks, in Poland under the rule of the Piast families (also probably of Norman origin) and their Norman military retinue.

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599
===== PAGE 590 =====

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These recently discovered cultural and historical facts therefore directly require a thorough review of our loanwords considered Slavic, with particular attention to the detailed exploration of the strong Avar and Bulgarian influences on Slavic languages. The need for this revision is particularly supported by four other very important circumstances. Modern Indo-Germanic studies have established that the so-called Finno-Ugric languages played a very significant role in the development of the Indo-Germanic languages. played a role, and these ancient linguistic influences appear most strongly and clearly in the vocabulary, structural elements and grammatical structure of the Slavic languages (1. above, pp. 550-552). The separation of this layer is therefore fundamental would be a methodological requirement. A considerable proportion of our words considered to be loanwords of Slavic origin cannot be found in any other Indo-Germanic language apart from the Slavic languages, and this very likely indicates that these words are mostly reverse borrowings. Many Hungarian words said to be of Slavic origin have obvious parallels in Turkic languages, and even often in Mongolian, meaning that the Slavic origin of these words is not clear either.

Our so-called Slavic loanwords are often incomprehensible from Slavic languages, and occur uniquely in them without an appropriate base word, while in Hungarian they have impeccable etymologies and extensive word families, which, moreover, are often found in the 4-5000-year-old memories of the ancient Sumerian language in detail. has multiple correspondences (1. our above-mentioned examples: sabre, haraszt, takács, mare, néma, gát, kád, kotéc). CAN)

===== PAGE 591 =====
Chapter 5. OUR ALTAI (TURKISH) FUTURE WORDS Scientific history and methodological overview
We have already touched on the issue of our Turkish loanwords repeatedly (1. pp. 234-236, 276-277 and 500, 512-513). We have shown that there is considerable uncertainty in our linguistics regarding the exact derivation of these words, because they show the phonetic peculiarities of various Turkic languages. We have also dealt with J. Benzing's recognition that, according to Kasgári Mahmud, the sound change "z > gr and the "a" +1" only occurred in the second half of the 11th century in some western Turkic languages, such as Chuvash (1. Benzing: Die angeblichen bolgar-türkischen Lehnwörter im Ungarischen). However, this article by Benzing has not been taken into account by Hungarian linguistics to this day. A logical consequence of this uncertainty is that they cannot agree on the time and place of the adoption of our Turkish loanwords, which are considered to be from before the conquest. The hypotheses of the individual researchers extend in time from the 10th century BC to the 10th century AD, and in space from Mongolia to the Kärpät Basin. Although attempts are made to separate them in time based on "sound development" criteria, these speculations are without any concrete basis in the complete absence of appropriate linguistic records. The real situation is very aptly described by Dénes Sinor, who, in the In his article "Historical Hypotheses and Linguistics": Finno-Ugric linguistics and its meaning — my opinion very closely related field of Finno-Ugric prehistory, unfortunately often does not have a satisfactory coordination of historical and linguistic hypotheses... The words calf, ox and bull can be derived equally - or perhaps even more - from both the Mongolian and the Turkic languages. The scholars who deal with this question are consciously or subconsciously influenced by the historical - and not linguistic - hypothesis that the Hungarians lived together with the Turks; consequently, the Hungarian loanwords must be derived from the Turkic language. If we now go a step further and ask the question, how do we know that the Hungarians lived together with the Turks, the main argument in response is that there are many Turkic loanwords in the Hungarian language, such as calf, ox, bi-

"Here is a beautiful school example of the circulus vitiosus." (1st ed. m., Gulya J.: The people of waterfowl, p. 329)
Seeing all this fluctuation, hesitation, and even, as Sinor openly states, straight-up turning in circles, "idem per idem" reasoning, and the unreasonable reversal of the order of cause and effect, one involuntarily raises the question:

===== PAGE 592 =====
des: wouldn't it be high time to break out of this dead end — which obviously leads nowhere — that is, to emphatically, with a theoretical and methodological edge, raise the question of the reality value of the basic thesis that triggered the unproductive circulus vitiosus, and finally to thoroughly examine the possibility that the words in question did not pass from Hungarian at all — both into the various Turkic languages and into Mongolian? Or — much more likely — did they not reach all of them from some common ancient source, together with Hungarian? Perhaps from the language of certain groups of the Huns and Avars? Or even earlier, from the Scythian-Sarmatian dialects? Perhaps from the language of the Pre-Scythian population of the Andronovo culture — or more precisely from the idioms of those settler groups of Caucasian-Transcaucasian, Kuban-Maykop origin, who in the first half of the 2nd millennium BC Did they arrive in the Volga-Oka-Ural-Ob-Irtysh region in the mid-19th century, shortly after the appearance of the Afanasyevo population, who were anthropologically completely identical to them, but also culturally very similar, and who also came from the south (Southern Turkmenistan), in the Altai-Yenisei region, and later, together with the latter, did they develop the Andronovo culture? In any case, the results of recent archaeological research effectively support, and one could even say methodologically require, the consideration and thorough, careful examination of these lines of thought when researching the formation, development, and relationships of the Ural-Finno-Ugric and Altaic languages and peoples. The studies of Gulya Mészáros were of fundamental importance in this regard, but they have been completely ignored by the Finno-Ugric trend of our linguistic and prehistoric research until now. Eastern European Folk History II. In his fundamental work "Chattiak és szkýthák" (Chattiak és szkýthák) in 1938, he included in a broad, all-encompassing prehistoric synthesis the archaeological complexes of Pre-Asia, Asia Minor and Eastern Europe that had been discovered up to that point. He showed that starting around 1300 BC, significant migrations began from Asia Minor and Northern Mesopotamia through the Caucasus to the Kuban-Káspi-Pontus region. In the following centuries, this migration continued and reached its peak in the 7th-6th centuries. During this period, huge Pre-Asiatic masses flocked to the Kuban-Don region and the northern coast of the Black Sea. Contemporary sources clearly prove that these peoples were the Scythians, which in turn implies that the As/Kytták had settled south of the Caucasus in the times before the 7th century BC. They lived in East Asia, He emphasizes that the Ananino civilization was also developed by these southern peoples (more on all this in 1. pp. 242-245). At that time, Mészáros was branded a fantasist by the exclusively Finno-Ugric-oriented representatives of Hungarian prehistory research because of this book. This opinion has since changed. However, we must emphatically emphasize that Mészáros' research results have been

602
===== PAGE 593 =====

The archaeological discoveries of the past four decades have largely confirmed it. Moreover, recent excavations have even proven that not only since 1300 BC, but also starting from the turn of the 3rd/2nd millennium BC, in the first half of the 2nd millennium BC, huge migrations from south to north took place in the region of interest to us. The over-expanded population of the advanced agricultural, animal-breeding, and metalworking cultures of the Kuban Caucasus, Transcaucasia, Northwestern Iran, and Southern Turkmenistan probably flooded the Eurasian steppe and forest-steppe regions in the first half of the 2nd millennium BC.

all the regions of the steppe that were suitable for agriculture or animal husbandry, and he widely spread the knowledge of agriculture, animal husbandry and bronze work in these vast areas (1. pp. 29-31, 47-51, 323-324, 425, 435-437, 511-512, 526; and the works of G. Childe, F. Hanĉar, A. Randa, K. Jettmar et alii, Burney-Lang, B. Stawiski et al.).

We can read very little about all this, about the global-scale population movements that undoubtedly played a primary role in the ethnogenesis of the later steppe and northern peoples, the development of their languages, and the consolidation of larger language groups, and which can be traced archaeologically with considerable accuracy, in the recent publications of Hungarian prehistory research with an exclusively Finno-Ugric perspective -- at most, something like, "... The processing of these discoveries and the achievement of somewhat solid results will keep many branches of science busy for years, even decades." (1. Bartha A.: What is not according to reason is ugly; partially described on page 380.) At the same time, however, our Finno-Ugric-oriented prehistorians already know precisely that this is the prehistory of the Middle Eastern countries, but in our judgment with scientific method, all these paleoethnogenetic and paleolinguistic questions do not even arise in the professional literature of Finno-Ugric linguistics in the narrower sense, as a result "has no direct connection with Hungarian prehistory." (1. Bartha: ibid.) A rather convenient position: "has no direct connection...".

of the approach outlined above, neither in terms of the general Finno-Ugric language development, nor in the field of individual detailed problems. Thus, in the matter of our words derived from Turkic languages, the vicious circle excellently characterized by Sinor still prevails: it is still assumed from the outset that the words in question are only Turkic. There may be loanwords in Hungarian, most of which were adopted sometime in the second half of the 1st millennium AD,

All these one-sidednesses are organically related to the research methods used from the earliest beginnings of Finno-Ugric studies, which

603

===== PAGE 594 =====

Its introduction is primarily linked to Budenz. He laid down the basic theoretical working method of Hungarian-Finno-Ugric language comparison, he took | introduced and made dominant the so-called "restricted" language study method, as well as the organically related "second-degree" language comparison, which still form the backbone of comparative language research in Hungarian Finno-Ugric studies today. According to these methods, first only the Finno-Ugric languages must be compared, based on the comparison the Finno-Ugric basic language must be deduced, and only then this reconstructed basic language may be compared with the basic language of the more distantly related -- e.g., the subaltern languages, which must also be reconstructed beforehand (second-degree" language comparison). As for the comparison of the Finno-Ugric languages, 111011, in the sense of this basic thesis, it must be carried out without any regard to the more distantly related languages (restricted "method").

This fundamental theoretical position, which has accompanied Finno-Ugric studies from its very beginnings, as a sort of scientific axiom, with a categorical demand for exclusivity, has branded any other language comparison as unscientific from the outset, and thus made it impossible and still makes it impossible today. That is why our linguistics declares every Hungarian word that has no parallel in the Finno-Ugric languages to be a foreign loanword almost a priori, without taking other circumstances into account. However, there is a great deal of doubt about the theoretical soundness of these methods, especially in the case of languages that have practically no older linguistic records -- just as the Finno-Ugric languages do not. We rely on the "misofokuk" and the "with the criticism of the "restricted" language comparison method, I will focus primarily on the argument of László Szabédi below.

Szabédi sharply points out the untenability of 6 methods. He states that second-degree linguistic analogy is nothing more than a completely erroneous generalization of the obviously misunderstood practice of the Romance language. Since the basic language of the Neo-Latin languages (Latin) is abundantly available to the researcher in masterpieces and thousands of simple inscriptions, the Romance language never compares Neo-Latin to a more distantly related language, such as Greek or German. But it is not because it does not resemble, as if the comparison is boring, it is not logical, it is just unnecessary, since for the purposes of comparison with the ýýý g language, Latin, which preserves a much earlier linguistic state, is much more suitable than any Neo-Latin language spoken today. Even if Latin had not survived, just as, for example, the Finno-Ugric basic letter lv had not survived, the Romanian linguist could only prove the Latin origin of eae juh by testifying to the Greek asz "sheep English ewe vanyajuh", since this word was not preserved in other neo-Latin languages." (5 abedh The Prehistoric History of the Hungarian Language, p. 52)

The conclusion is that. The theoretical basis of the narrow comparative method is the erroneous assumption that more distantly related languages

604

===== PAGE 595 =====

Taking into account the data of the 4 ý ýý cannot contribute anything to clarifying the relationships between closely related languages, and in fact it rather interferes with this work. This is of course mistake." (Szabédi; id.m. p. 20) In addition, however, the narrowed comparative method has at least two very harmful consequences -- emphasizes Szabédi: "1. it slows down linguistic research, because it "for the time being" narrows the field of language comparison; 2. by "for the time being" excluding more distant relatives from the comparison, it contrasts the language under study with more distantly related languages as a relatively unrelated language, and this relative non-relatedness may soon degenerate into categorical non-relatedness." (Szabédi: id. m. 20-21, pp.) Szabédi's insight is fully confirmed by the scientific history of Hungarian Finno-Ugric and Altaic studies. This slant of our linguistic research has already reached the point as a result of a "narrowed" view, where a considerable proportion of authoritative experts actually categorically deny the relationship between the Uralic and Altaic languages, as we have seen, for example, in the work described by Róna-Tas (1. pp. 449-451). It is true that they are quite alone in international scholarship with this denial: let us remember the quoted statement by Collinder, in which he clearly exposes the falsehood of the hype in this direction that has "recently gained ground" in Hungarian linguistic research (1. ibid.). However, already at the turn of the century, several of our researchers -- such as Géza Nagy, Károly Fiók, Gyula Rohonyi and others -- put forward very thoughtful arguments regarding the fact that the languages that are now commonly called "Bulgarian-Turkic"; "Chuvash-like" or "Western (Oghur) Turkic" in scientific terminology, but whose origin is in fact unclear -- because they are practically unknown -- can actually be considered the connecting link between, on the one hand, the later developed Rök languages, and, on the other hand, the Finno-Ugric languages, all of which were strongly influenced by them in ancient times. "Common Turk" (Eastern or Oghuz-Turkic)

These researchers classified the Onogur-Bulgarians, Hungarians, Sabirs, partly the Avars, Huns, and, with certain restrictions, the 1st century BC. the language (or languages) of the Scythian-Sarmatian peoples of the 1st millennium BC -- which was indeed a reasonable and very obvious conclusion in the framework of the broad-sighted, global prehistory and linguistic history approach they represented and in the light of the Scythian-Hun-Avar-Hungarian identifications of contemporary sources. These languages - in distinction from the true, Altaic-speaking Eastern Turks - were not called Turkic, but rather the "Ugric" or "Ugric-Hungarian" language group in the true ethnic sense of the word, and at the same time they were considered much older than the common Turkic and Finno-Ugric languages, and were interpreted as their common ancestral linguistic base: the parallels of the Finno-Ugric and Altaic languages, the Uralic-605

===== PAGE 596 =====

The common linguistic elements of the regional languages were explained by the strong linguistic influences of this Syntlingual group in the north and east. According to them, it was from this "Ugric-Hungarian" language group that the grammatical and structural features common to Hungarian were transferred to the Turkic languages and Mongolian, as well as those words that our linguistics considers to be Turkic loanwords in Hungarian (1. pp. 276-277). In other words, to use the words of modern linguistics, they actually viewed the common features of the Finno-Ugric, Turkic and Mongolian languages as the effects of this "Ugric-Hungarian" language group that caused an areal linguistic equalization -- more than four decades ahead of Trubetsky and modern Indo-Germanism.

The leading representatives of Finno-Ugric linguistics and ancient history research at the time rejected this far-sighted concept, which was vastly ahead of the scientific standards of the turn of the century, without any closer examination and branded it as dilettante, because it contradicted the dogma of our exclusively Finno-Ugric origin. One of the main arguments of the Finno-Ugricists in this criticism was what we already know well from the previous discussions: in the current vocabulary of the Hungarian language, any word, expression, or concept that is not found among the Ob-Ugric and other Finno-Ugric peoples cannot be considered a foreign language. "ancient heritage", but also a loanword. As we saw above, this view is based on the Budenz- This was a position that necessarily followed from the essence of the "restricted" method of language comparison. The other most important objection referred to the fact that the Scythians and Sarmatians spoke an Iranian language, according to the findings of Indo-Germanic linguistics. We admit that these arguments seemed well-founded at the time, around the turn of the century, since both were based on the almost uncontroversial convictions of international linguistic and prehistory research at the time. Therefore, the thoughts of Fiók, Nagy and their colleagues in this direction were shrouded in a veil of oblivion for several decades. However, if we carefully consider the new insights of modern Indo-Germanic linguistics and prehistory, which we presented in Chapter 1 of this volume (see pp. 407-428), then Géza Nagy and his colleagues' theory can certainly no longer be called fantastic or amateurish. In fact! The definite rejection of the 19th-century ideas of the language family tree, the newer views on the time (2nd millennium BC), the way (Area language alliances, 6765, Finno-Ugric, with Caucasian and Semitic influences) and the place (Eastern Europe, Caspian region) of the formation of the Indo-Germanic languages, as well as the increasingly strong doubts about the Iranianess of the Scythians, correspond point by point to the concept of prehistory and language development of Nagy Gé-7a, Fiók and his colleagues (cf. Fiók: Ostorénét és kritika; Rohonyi: A honfoglalásállás történet; Nagy: Ethnology and linguistics; The prehistory of the Hungarians; On the Scythians; etc.).

606

===== PAGE 597 =====

According to their views, the "Ugric-Hungarian" Osnyelvi block was located in the southern areas of Eastern Europe and the connecting strip of Western Asia in the 2nd millennium BC, roughly between the Southern Urals and the Caucasus in the north-south direction, and between Eastern Ukraine and the Aral Sea from west to east. 16 Well, this area exactly corresponds to the area of the Eastern European and Western Jia Early Bronze Age archaeological civilizations that have been archaeologically identified since then (Kuban-Maykop, catacombs culture, Balanovo, Abasevo, etc. in Europe; Andronovo in Western Asia), whose ethnic groups mostly migrated from the Caucasus, Transcaucasia and southern Turkmenistan to the Eurasian steppe in the times following the turn of the 3rd/2nd millennium BC.

On the other hand, according to Géza Nagy and his colleagues, the "Ugric-Hungarian" proto-language had a very strong influence on the languages of the neighboring ethnic groups: on the Finno-Ugric in the north, on the Altaic in the east, and on the Indic and Iranian in the southeast. This thesis is also in full accordance with our current knowledge. Today, a significant proportion of the authoritative representatives of Indo-Germanic studies believe that the "Finno-Ugric" languages had a very significant influence on the Indo-Germanic languages that began to develop in the 2nd millennium BC. However, the above-mentioned south-north migrations reached the Altai-Yenisei region around 2200 BC (Afanashevo culture), and the anthropological composition of the population there matched the racial characteristics of the bearers of the contemporary Eastern European Kuban-Maykop and late pit-grave cultures. Both groups are descendants of the Pre-Asian cultures.

came from the area: the Afanasyevites from southern Turkmenistan, and the Kuban-Pit Sirois from Transcaucasia. At the beginning of the 2nd millennium BC, these "plump Europid" ethnic groups mostly migrated further to the middle Volga, the southern Urals and the Ob-Irtysh region, as well as to the entire steppe and forest-steppe region of Western Eurasia, forming the Andronovo civilization with a unified anthropological and cultural profile.

As an illustrative comparison, we briefly mention that, in addition to the evidence of contemporary archaeological finds, these migrations, which eventually led to the settlement of the Eurasian steppe within about 1000-1200 years, are also known from the past. In exactly this way, almost in the full light of history, the European colonization of vast areas of the North American continent took place over a period of about 200-250 years. The faster pace can be attributed to more advanced technology and larger human populations.

In the 17th century, European settlers were found only in small pockets on the eastern coast; in the 18th century, these pockets had grown into a continuous coastal strip. First, traveling traders and fur trappers penetrated the interior, soon followed by stockbreeders, and these were followed by farmers.

settlers, 19th century v6-607

===== PAGE 598 =====

They gradually, step by step, conquer the entire continent, while the indigenous population, who are backward in civilization and technology, are either exterminated or forced into reservations.

It is obvious that the settlement of the Eurasian steppe at that time also took place in the same way by the southern colonists, who had an incomparably higher culture than the indigenous population: step by step they took over the economically viable areas, at first mainly the food sources and the river valleys suitable for farming, the forested steppe areas, and then, with the spread of extensive animal husbandry, the grassy steppes, while the sparse fishing-hunting population of these areas was partly exterminated, partly subjugated, and pushed further and further north. We can follow this process particularly well in the light of the results of anthropological studies. Although there is still a lot to be done in this area, it is already certain that in the 1st century BC. Starting from the end of the 3rd millennium BC, the southern origin population gradually spread and eventually became dominant throughout the Eurasian steppe and forest-steppe — from Eastern Europe to the Altai. At the same time, the earlier Paleo-European (Cro-Magnoid) and Paleosiberian populations gradually disappeared (1. Jettmar et alii, Childe, Hanéar, Grjasnow et al. works, László

broad-fronted and rather short-headed plump Europid type, while

Gyula id. m. 1961.).

In Chapters 1 and 3 of this book, we have repeatedly shown that these enormous processes of cultural expansion and migration in the Early Bronze Age cannot be separated from the question of the development of the Finno-Ugric language family. Now, however, we can see that we cannot examine the circumstances of the unification of the Altaic languages without taking these paleoethnogenetic data into account. In Hungarian, this means that in the light of the results of recent archaeological research, this southern origin

We must attribute primary importance to the languages of the colonists, in line with the linguistic family-like balance of both the Ural-Finno-Ugric and the Altaic-Turkic languages. Moreover, we must very likely take into account in this regard the strong Finno-Ugric and Altaic parallels of the eastern groups of Indo-Germanic languages, primarily the Slavic and Iranian languages, since these latter languages - especially Finno-Ugric - played a primary role in the formation of the vocabulary and grammatical and structural features of the individual Indo-Germanic language communities, according to modern Indo-Germanic linguistics (1. pp. 549-552).

Due to the eminent importance of the question, we emphasize once again that the term "Finno-Ugric" in this context does not mean the Finno-Ugric basic language in the modern sense, but rather the southern proto-linguistic community, which, in addition to its significant role in the development of the Indo-Germanic languages, also catalyzed the areal equalization of the present-day Finno-Ugric language family.

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It also brought about the consolidation of these languages as a language family (1. 425, 428 and 550-551).

From all of this, it follows that the fundamental changes that have occurred in international scholarship since the turn of the century, the recently developed and increasingly general approach to prehistory and linguistic history, make the theory of Géza Nagy and his colleagues very relevant, and almost require that we revisit their theses, thoroughly examine their reality in the changed scientific atmosphere, in the world of new archaeological and prehistoric discoveries, and finally embed them in our current body of knowledge.

At the same time, however, all these considerations effectively support our conviction that our words considered as loanwords of Turkic origin cannot be left out of these newly conducted paleolinguistic studies: the question of their real origin also needs a thorough and objective review. Especially because when defining our Turkic loanwords, the image of the "wild barbarian" Hungarians at the time of the conquest also provided the data that supports the paleolinguistic approach of Géza Nagy and his colleagues.

"methodological" basis for declaring practically every Hungarian-Turkish word correspondence a priori to be a Turkic loanword.

→. Do we find any specific linguistic, historical, archaeological or other considerations in addition to the above general considerations?

Notes on the linguistic, historical and ethnic background of the so-called "Chuvash criteria"

Let us start from that hopeless situation, from that linguistic and historical vicious circle that we have already become acquainted with in Sinor's apt formulation (see pp. 601-602), and first of all let us examine in more detail the insoluble contradictions that phonological linguistics is involved in regarding the closer derivation of the "Chuvash" layer of our loanwords, which are considered to be of Turkic origin. In our next review, we will examine the argument of Géza Bárczi's study entitled "Loanwords".

The Hungarian language is similar to Bulgarian and Common Turkish.

The aforementioned vicious circle already appears in the introductory lines of the paper: Bárczi simply accepts as fact the hypothesis of Hóman — Moravcsik, which was written a good fifty years ago, but was already very unstable at the time of its formulation, according to which the ancient Hungarians migrated from the Ural Mountains to the Kuban-Meotis region around 460 AD under pressure from the Sabirs and Avars, accompanied by the Onogurs (1. Bárczi: id. m. Tanulmányok a bulgár-magyar körebyl, pp. 31-32).

609

===== PAGE 600 =====

In this regard, we recall the research results of Haussig and Altheim, who, using numerous contemporary primary sources, clearly proved that the Sabirs and Avars migrated to the Kuban region along the southern shore of Lake Caspian, through Dagestan and the Caucasus (1. pp. 265-270). In the same place, we also showed that the names of the Onogurs, Ogurs, Sabirs and Bulgars can be dated to the Caucasus region in times before the Hunnic era.

Bárczi therefore works with one-sided and unmethodical means when outlining the historical background, insofar as he wants to explain the historical circumstances of the adoption of our Turkish loanwords with a hypothesis that was initially extremely improbable, and in fact, has proven to be downright false in the light of recent decades of research.

He then explains that determining the transmission language is extremely difficult because a) the Common Turkic languages are still quite close to each other today; b) in the case of the Chuvash languages, it is also disputed whether there were more than one such language — today there is only one: Chuvash (1. id. m. p. 40). Let us note these two statements of Bárczi, because we will have to refer to them again — in sharp contrast to Bárczi, who in the following pages of his study consistently ignores these facts and postulates various hypothetical "proto-Chuvash" language variants on a conveyor belt.

The most important part of Bárczi's thesis from the point of view of a more objective assessment of our Turkish loanwords is the part discussing the so-called Chuvash criteria. By this, our Chuvash criteria" linguistics understands the phonetic peculiarities of our Turkish loanwords that differ from common Turkish and are close to the phonetic order of the Chuvash language. From this, it is concluded that the words in question originate from a "Chuvash-like" Turkish language(s).

In the field of vowels - writes Bárczi - the Common Turkic "a" in Chuvash corresponds in some cases to "i". This sound, it seems, corresponds in many cases to the Proto-Turkic "a". However, this cannot be justified in all cases. "It can be thought" that (1. id. m. p. 41).

a" > . 1" an old change in Chuvash, precisely because in Hungarian we find 17 at this time — he notes

Compare this with the article presented by Benzing (1. pp. 235-238): The testimony of Mahmud of Kasgári scrunches the sound "i" ("y" ("instead of "7a") only in his time, i.e. the second half of the 11th century. Turks living in the west began to use it in the middle of this century. According to these, it is therefore not possible to think about what Bárczi is asking. He himself feels this, because he continues: , But some Hungarian "i" instead of Turkish "a" occurs in Turkish loanwords in which Chuvash does not know either "Pt, e.g. "gyertya-gyirtya-gyortya" etc. ~ Chuvash "surDa", Altai "jarta" , (1. id. m. p. 41.)

V6. Sumerian "gir," — "leuchten, blitzen, Blitz" (lights up, flashes, lightning); Akkadian Assyrian "kir ru" "Ofen, Brand, Hitze, Feuer" (stove, burning, hot- 610

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fire); ancient Greek "ker-aunos" = "blitzstrahlend" (shining like lightning); la

===== cre-mo" = "verbrennen" (to burn); German "Ker-ze" (candle), We believe that the thousands of years old Sumerian and Akkadian "i", as well as the Old Norse and Latin According to these "sounds", the high-pitched variants of the root are in any case much more ancient than the low-pitched Turkish word forms; which, according to these, could most likely have been transferred to the Turkish languages from Hungarian - where high, mixed and low-pitched variations of the root can still be found. - And this is even if - as can be seen from the above - we strictly adapt to the methodological requirement of "regular sound changes" observed over time in phonetic linguistics.

Bárczi then discusses the supposed Chuvash-Central Turkic consonant changes, but of course in each case he has to work only with hypothetical, "star-shaped" Proto-Turkic phonemes, because there are simply no ancient Turkic language monuments in which we could record the sound changes in question.

It is characteristic, for example, how he wants to "prove" the originality of the Chuvash "r" sound in contrast to the Common Turkic "z": since, in addition to the Mongolian equivalents, the Chuvash "r" is also found in our loanwords adopted during the time of the Uralic homeland, thus claiming a very ancient language variety. That is: "r", this shows that "r" is the more original Turkish sound - he writes.

a) not only does he treat as a fact the unprovable, a priori assumption that the Hungarian-Turkish parallels in question can only be Hungarian adoptions, but b) he also assumes for certain that the two or three words that our linguistics considers to be ancient adoptions were indeed transferred from the Old Turks to Old Hungarian in the 1st millennium BC - which, however, is also just an assumption without any concrete evidence.

In other words: he "proves" the antiquity of the "r" sound in Turkic with two hypotheses, which themselves were originally based on the a priori assumption that "r" was the original sound in the ancient Turkic languages.

— So Sinor's vicious circle appears again...

The methodlessness of this one-sided approach is topped by the fact that even Turkology is not sure whether the antecedent of the common Turkic "z" in the ancient Turkic was really "r". András Róna-Tas writes about this issue in his article "The Nature of Hungarian-Bulgarian-Turkic Contact": "According to some views (Ramstedt, Poppe, Räsänen) both Chuvash and Mongolian have preserved the ancient Altaic "r" and "l". According to the newer and increasingly general view, the Mongolian words in question are ancient Bulgarian-Turkic loanwords," (see Róna-Tas: id. m. Magyar ýstört. tanlúník, p. 271, note 14)

In other words, this "r" sound is not a proto-Altaic or proto-Turkic, but a "Bulgarian-Turkic" characteristic, which, however, -- in sharp contrast to the Róna-Tas word-1

===== PAGE 602 =====

mazdsával + According to the testimony of Kasgári Mahmud, you cannot call it ancient in any case (1. p. 601). Neither among the Chuvash nor among the "Bulgarian-Turks". Let us be content with stating this for now, postponing our other considerations regarding the so-called "Bulgarian-Turk" languages and ethnicities until later. Bárctzi sees the situation as "more complicated" in the case of the Proto-Turkic word-initial "j". He is certainly right in this. So let's take a closer look at the complications that arise here.

According to Bárctzi, the word-initial Proto-Turkic "4j" corresponds to "s" and "š" in Chuvash, and to "j" in Common Turkic. He then notes that the criterion value of this correspondence was questioned by Ramstedt, on the grounds that Chuvash yy > yy is not older than the 13th century. In passing, we would like to note: Benzing was not wrong when he discovered in his work by Mahmud Kasgári that it was in the so-called "a" > "4" (y) and 8 22 gr changes are not new in Chuvash, and "Bulgarian-Turkic" languages. Added to these is the also late occurrence of the sound change "t" > "š" in Chuvash, and all of this really turns the theory of Bulgarian-Turkic — Hungarian sound correspondences, which our linguistics has held since Gombocz, upside down. However, Bárctzi flatly rejects Ramstedt's criticism, saying: this objection is unfounded, because in this case there is also an "s" in Yakut, which points to an old change. Now let us pay close attention, because here begins the dead end into which the phonetic forms of our words, which are a priori considered to be Turkish borrowings, practically force phonological linguistics into. Bárctzi further reflects: "The Chuvash loanwords of the Hungarian language cannot go back to a single Chuvash language; there was a Chuvash language where the change "j" > "š" really appeared late, but there was also one where it was very old." (ibid., p. 43) In other words, for Bárctzi, the language of the Yakuts of Eastern Siberia — which, moreover, is not even "Chuvash", but a common Turkic dialect! — "proves" that there were at least two Chuvash languages in the past — along the Volga. It could hardly have been anywhere else, because three pages earlier Bárctzi himself had stated that the ancient Hungarians had contact with the Turks between the Urals and the Don, and in the same place he also stated that consequently the Yakut language "... due to its geographical position cannot be taken into account in terms of the Turkic loanwords of the Hungarian branch." (id. m. p. 40) So even if we were to accept the former existence of such an ancient Chuvash language, we could only look for it near the Yakuts, somewhere in Eastern Siberia, since only Yakut 611216 and one of its phonetic features, namely the sound change a.j" -"a", "š" are known. According to Bárctzi's own methodological definition, nothing is so distant. from the "Chuvash" language

612

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We should not under any circumstances explain the initial "s" sound of our Turkish loanwords. However, this is just the beginning. The thicker part will come later; "However, there are words that are undoubtedly Chuvash in nature, in which the initial "j" in Hungarian is not "sz", but "o". We are forced to assume that these words were transferred from a different Chuvash language or dialect than those beginning with "sz"." (id. m. p. 43) Let's count carefully: this is the third supposed -- but practically treated as a fact by Bárctzi -- Chuvash language or dialect, although we only know of one, modern Chuvash, of which he considered the most characteristic to be several: Bárctzi himself first admitted that "... it is also disputed whether there We can prove that the "Chuvash" phonetic features only occurred from the 11th to the 13th centuries; what were several such languages". But let us continue Bárctzi's helpless struggle: "However, the common Turkic word-initial "j" has a third Hungarian equivalent: "gy", and this occurs in words that show an undoubted Chuvash criterion" — he continues. "... The question is, what could have been the Turkish antecedent of "gy"..." (id. m. p. 43). His solution, of course, is this time too, "... that these words of ours came from a language (language variant) in which the word-initial "t" developed into a "d" or "j"-like sound. Since there are several Chuvash types among these words, we must therefore assume that not only a "š"-ing and a "j" -ing or "t" -[61 losing, but also a third type of "j"-ing language or dialect came into contact with the Hungarian language." (id. m. p. 44)

We can see that Bárctzi has miscalculated, because this is the fourth Chuvash language that he has to posit, in order to explain the different phonemes of our Chuvash-like loanwords (the first one is Chuvash itself). He obviously no longer knows himself in his own chain of hypotheses, or perhaps he himself feels that perhaps there will be a bit too many of these Chuvash phantom languages....,

But actually a fifth such phantom Chuvash must have existed, because we also have words in which the Turkish initial "j" corresponds to ny (e.g. nyak and nyár). Well, Bárctzi naturally also explained this above by saying, "... We must admit that there was an ancient Turkish language variant in which "j" corresponded to 'n'." (id. m.

36, p.)

This would have given us at least four — or rather five — "Chuvash" languages or dialects, although we know only one. But what is even more important: the Turkic languages are still much closer to each other phonologically than these hypothetical "Chuvash" language variants would have been, according to our linguistic assumptions.

613

===== PAGE 604 =====

1500, and in some cases (ancient borrowings from the Ural region) 3000 years ago. We have seen: it is taken for granted that in these "Chuvash" languages the Old Turkic "j" would have been equivalent to "sz", "gy", "o" or "ny". These are very considerable differences — and moreover, only for a single sound! So this is where phonological linguistics comes to in its ideas about the ancient times of the Turkic languages, which are still practically mutually intelligible to each other today. The unspoken basis of all these irrationalities is the forced idea that our words parallel to Turkish can only be loanwords in Hungarian taken from Turkish.

It is obvious that under such circumstances, in the realistic interpretation of our "four- and five-fold "We must put it up" is the way to go, we can never get ahead 05078505 type Turkish adverbial words" of Bárctzi. With this "method" everything in the world can be "proved", only a sufficient number of undatable phantom languages need to be posited. Let us therefore examine whether the so-called "Chuvash criteria" of our questionable words cannot be explained in a different way, with greater probability, and especially in a much more obvious way than Bárctzi's - and Finno-Ugric linguistics - unreasonable argument.

We must begin by saying that Chuvash is the only Turkic language that differs from the others to a greater extent. While the common Turks can understand each other relatively easily when necessary — the linguistic differences are smaller for them than, for example, between Slavic languages — Chuvash is a completely separate language, incomprehensible to other Turks. This undoubtedly indicates that a non-Turkic linguistic component played a very significant role in its development. We must therefore determine this linguistic influence in order to explain the phonological differences between Chuvash and other Turkic languages.

As is known, the Chuvash living along the middle Volga are found precisely in the region where the Volga Bulgarians were located in the 8th-13th centuries. Accordingly, according to the general conviction of the researchers, we must see them as descendants of the Volga Bulgarians. These former Bulgarians, however, seem to have migrated to the middle Volga region after the collapse of the Pontic Onogur-Bulgarian empire of Kovrat at the end of the 7th century, accompanied by significant Sabir and Hungarian groups. The latter were found by Julianus in the first half of the 13th century in 06/6056 Volgánál, in the immediate vicinity of the Volga Bulgarians, barely two days' walk from one of the major Bulgarian cities.

The descendants of these Hungarians who remained in the east, according to Russian chronicles, were still living in the middle Volga region in the 16th century, mentioned under the names moZar, madzar, molar, maéarin, and mescer, yy mizdr. Most recently, István Vásáry has dealt with these Hungarian remnants in a thorough study (The Hungarian fragments of the Volga region after the Mongol period). The 614

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Below we discuss this issue based on his most important findings, occasionally adding our own conclusions, The MoZar (Madzar, Macarin) people of the Russian chronicles originally constituted a considerable ethnic group. place names: ,.... We find the mortar-containing elements scattered over a vast area. MoZary, Mozarovo, Mozarovka, Mozarovskaja, Mozarik, etc. These names occur in the Ryazan, Tambov, Penza, Nizhny Novgorod, Simbirsk, Kazan, Ufa, Vyatka governorates of the former Russian Empire. If we project the Mozar-element place names onto a map, two major settlement centers clearly emerge, excluding several scattered localities: one is located on the right bank of the great bend of the Volga, mainly in the present-day Chuvash territory (emphasis added), and the other is in the central part of the Ryazan region. and in the northern part of the Tambov region." (Vásáry: id. m. Hungarian prehistoric studies, p. 286) We can therefore state in advance that, according to the clear evidence of place names, the descendants of the Volga Hungarians named MoZar lived mostly among the Chuvash, mixed with them, until the 16th century, and later merged with the Chuvash. The name Mescher (mescer, mizár, misár) is closely related to these MoZars, not only phonetically (v6. Hungarian-Magyar), but also territorially. Vászary writes about them as follows: Two ethnically distinct groups are known. The smaller western group of Mescheryak is described as Russian in Russian literature of the last century, but it is clear from the descriptions that they are some kind of ethnic group that became Russified over time. They lived mainly in the northern districts of the Ryazan region, and the area north of the middle Oka is still called Mescherskaya storona. Mescheryak still lived in the former Tambov, Penza and Saratov governorates, in most places mixed with Russians, Mordvins and Tatars.

On the one hand, in the places mentioned above, and on the other hand, further east of them, in the former Nizhny Novgorod, Simbirsk, Kazan, Ufa, and Orenburg governorates, there lives an ethnic group much larger in number than the Russian Mescheryaks, which the Russians also call Mescheryaks, but their language is a dialect close to Kazan Tatar. They call themselves and their language simply Tatars, but the Kazan Tatars distinguish them from themselves, calling them Mishur..." (id. m. p. 287)

As we could see from Vászár's geographical description, the settlement areas of the Meschers are almost everywhere closely connected to the settlements of the Morzars, and in some regions they are mixed with the latter in a mosaic-like manner. For this reason alone, Vászár's working hypothesis, which - contrary to the entire Hungarian literature on the subject so far - considers the western Meschers to be a Russified Volga-Finno-Ugric people, seems highly unlikely.

615

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and he considers the eastern ones to be predominantly Tatars, among whom, in his opinion, the Volga-Finno-Ugric western Meschers and perhaps a smaller group of Mozars may have merged. The main support for this assumption is that while according to Vászár there are no problems with the Mozar-Hungarian correspondence, the word form yyyyyyy, miser (he does not speak of the form miZár here!) cannot be connected with either the MoZar or the Hungarian, megyer names "due to phonetic difficulties" (id. m. pp. 289-290).

However, we are already familiar enough with such Finno-Ugric phonological "difficulties" not to attribute much importance to them. We recall, for example, the "multiple-order difficulties" of the TESZ in the question of the derivation of our word sábla from the verb sabi (1. pp. 254-255). But even apart from this, the name of the Meszyrs occurs not only in the forms mester and miser, but also in the form mizúr, which can be very well connected to the name moZar. Even on a phonological basis!

That is why Vászár does not consider it "appropriate" to mention this Mizúr version in his above argument. "It is satisfied" with the forms mescer and miser. Thus, the unmethodical, one-sided, selective "source evaluation" that has exclusively characterized our Finno-Ugric origin trend from the beginning appears again.

However, Vászár's assumption is also contradicted by the fact that the Kazan Tatars distinguish themselves from the eastern Meschers, calling them Misur. So if they were originally Tatars, then why this distinction?

On the other hand, even if we accept this phonetic objection, the ethnonym yyyyyyy can still be compared with the Old Hungarian word mecsér (swordsmith, swordsmith) (l. Heckenast G. - Nováki Gy.: Kora Árpád-kori szolgáinépek) — which, however, Vászár again does not mention. However, knowing the outstanding quality of the metalwork of the conquering Hungarians, we cannot reject the conceivable possibility that the Mecsérs (Mecsérs?) may have formed the blacksmith tribe of the Hungarians who remained in the east; just as the Altai tribal names Tarkan, Tarhan (Tariján, Tárkányi) have an ancient meaning. This is evidenced by the aforementioned mosaic-like merging of the Mozzar and Meséer settlement areas, as well as by the fact that the eastern Meszchers, Temnikov and Sergaci, are located in "blacksmith".

the same area. Interestingly, the location of these two groups roughly coincides with the two larger groups of place names containing the MoZar element, which Vászár himself considers striking. (id. m. 289, p.)

justice. The Russian annals' report on the Kazan campaign of 1551 also points in this direction — with great weight: "people (on the hilly right bank of the Volga), And the tsar and the voivodes brought the mountain people to princes and murzas, princes of centurions and corporals and the Chuvashs 68 the Cheremis and Mordvins and Mordvins and Mordvins and Tarkhans..."

616

===== PAGE 607 =====

(Vászár: id. m. p. 285) Well, these tarhans (i.e. "blacksmiths") mentioned directly next to the mortars are very likely the people living next to and between the mortars. They cover meschereks (i.e. mecsérek, also "blacksmiths"), who, however, had obviously become Tatars by that time, and that is why the Russian source calls them by their Altaic name, tarhans.

In connection with this data, we would like to remind you that the degree of Finno-Ugric affinity of the Hungarian language expressed in % - excluding the Ob-Ugric - is highest in the languages of the Cheremis (26 and 30%) and Mordvins (26.5 and 25%) who are mentioned directly next to the Mordvins in the above Russian yearbook data of 1551 (1. Eeva Kangasmaa-Minn and Alo Raun's Finno-Ugric affinity tables are in Gy. László: The earliest sections of our ancestral history, p. 34 and p. 502 of this book).

In other words: the Hungarian linguistic affinity is greatest in that Finnish linguistic subgroup, which, according to historical data, even in the 16th century lived in the immediate vicinity of one of the larger groups of Hungarians who remained in the east (Mozars, Meszcs), and into which these Hungarians probably later partially merged. This also provided significant support for our assumption that the different degrees of Hungarian linguistic affinity of the individual Finno-Ugric languages can be explained with much greater probability by repeated and varying intensity of areal linguistic influences than by the Finno-Ugric language family tree theory, which can now be rightly considered obsolete (1. pp. 508-509).

The second very remarkable circumstance is that another compact focus of mortars was in the present-day Chuvash area and later mixed with them, merged into them. In other words, the "Chuvash character" of our Turkic loanwords with "Chuvash criteria" can probably be explained precisely by the fact that these phonological characteristics were taken over by the Turkic Chuvashs — and even partly by their predecessors, the Volga Bulgars — from the Hungarian masses who had previously lived directly next to them for centuries and who gradually merged into them after the 13th century, namely — according to the testimony of Mahmud Kasgár — already starting in the 11th century (1. Benzing: id. m. II. book, pp. 235-236.). This is also confirmed — also with great emphasis — by the fact that the common Turkic " only became "6"-56 in the Chuvash language in the 13th century (Ramstedt).

These latter data, however, are generally closely related to the question of our loanwords called Turkic, since our linguistics has established that the lion's share of our pre-conquest Turkish loanwords very likely originate from such "Chuvash-like" Turkish languages (or, according to our linguists, languages); If we compare the number of Chuvash-like words among the criterion words with those of common Turkic character, we find that the ratio is approximately 3 to 1 in favor of Chuvash. It is likely that the same is true in our uncriterion words.

617

===== PAGE 608 =====

ratio, i.e. the Chuvash influence before the conquest can generally be considered at least three times as strong as the Common Turkic, and perhaps even greater, because some of the Common Turkic-type words can perhaps be attributed to the Cuman-Bechengy influence." (Bárczi: id. m. Tanulm. a bulgár-magyar kapcs. körébyl, p. 47) According to these, it is obvious that the research of the origin of our "Chuvash-type" words from several angles, and their more precise definition, can be called a key issue in the examination of our words considered to be of Turkic origin. We cannot be satisfied with simply assuming a priori that each of our words that show such "Chuvash criteria" could only have come to us from Turkish, and not from one, but from at least four hypothetical "Chuvash" language varieties, of which, however, not a single one can be dated apart from modern Chuvash, and what is more, their mere existence is extremely unlikely, since the Turkish dialects are still so close to each other today that we cannot in any case count on several Turkish language varieties 1500, let alone 2-3000 years ago, which were already so sharply separated from each other phonologically at that time, as our linguistics postulates. The high degree of similarity of today's Turkic languages clearly indicates that the breakup of the Proto-Turkic language community and the separation of individual language variants cannot date back to earlier times than the independence of the various Slavic or Neo-Latin languages, which process in both cases occurred roughly in the second half of the 1st millennium AD.

However, our linguistic and pre-historical research does not deal with such considerations, questions, or perhaps investigations in this direction, because under the spell of the dogma of our exclusively Finno-Ugric origin, it tries to explain almost every Hungarian linguistic phenomenon, folk tradition, etc., based on the Ob-Ugric parallels. This "narrowed" approach has been preventing for more than a century a thorough and careful research into the real origin, formation and development of the Hungarian language, an objective and unprejudiced examination of the roots and connections of our folk culture, and a realistic insertion of the ancient Hungarians into the historical and cultural history of the past. In short, the theory of our exclusive Finno-Ugric origin simply lacks historical perspective, and what it does have is the outdated, long-disproven foundations of the 19th century Indo-Germanic cultural priority. It is an absurdity of linguistic and cultural history to examine the development of the Hungarian language and its past historical role in Eastern Europe practically reduced to the 19th century relations of the Voguls.

This one-sided approach also leads to the fact that almost every Hungarian word that has a parallel in a Turkish language is considered a Turkish loanword. However, this a priori assumption raises many questions that are still unresolved today, 5 and there are still many questions to be answered in this area.

===== PAGE 609 =====

Even among our researchers who profess a firm Tin-Ugric origin, opinions are irreconcilably divided; While according to one view, the Hungarians, who separated from the Ugric people, gradually, as a result of internal development and complex external influences, achieved by their own efforts the highly developed semi-nomadic way of life, the level that we have before us at the time of the conquest, according to the other extreme, the Turks organized a primitive, horse-riding, fishing-hunting people and gave them a high culture... At the center of the debate are our loanwords of Turkish origin. On the one hand, they claim that our loanwords clearly reflect the nature of the Turkish influence, but on the other hand, they claim that the evidentiary power of these words is very small." (A. Róna-Tas: The nature of Hungarian — Bulgarian-Turkish contact, Hungarian prehistoric studies, p. 267.) We also learn from Róna-Tas that "... in the light of the latest data of archaeology and historical science, the theory of the 'great migration of primitive fishing-hunting Ugric peoples' is untenable." (id., p. 268.)

This deplorable situation, on a theoretical and methodological level, means that the methods used to approach the issue so far cannot under any circumstances be based on solid scientific foundations, because otherwise, diametrically contradictory conclusions would not be able to come to light, even though all of them come from qualified researchers, and therefore act with the claim of methodical scientificity. As in so many other cases, here too Gyula László formulates the question most clearly, pointing out its essence: it still remains, but we must look for an explanation for its appearance in a different assumption." (Gyula László: On the Conquests, p. 17). Bar Róna-Tas highlights Gyula László's "sharp vision of the problem," " So we did not need any Turkish "influence" to transition to a farming and animal husbandry life. Naturally, our Turkish vocabulary was but he does not clearly see how the theory of the double conquest explains our Turkish word stratum (Róna-Tas: id. m. Magy. ystort. tanulm. p. 268, note 5).

Of course, he cannot see it with an exclusively Finno-Ugric perspective, because Gyula László's insight in this issue is primarily manifested in the fact that he does not accept as fact the unprovable — and, let us add, anthropologically absurd — dogma that the Hungarians as a whole are Finno-Ugric or "Ugric" in origin, but rather researches our origins in several branches, of which, in his opinion, only one — and not even the most significant one — was Finno-Ugric. This perspective is in harmonious agreement with the known historical sources and data of the time, but not least with the recently discovered facts of archaeology. That is why Gyula László demands a separate, multi-faceted (complex) examination of all recognizable ethnic components of the Hungarians, which are indisputably composed of several branches, as well as a thorough research into the history and origins of non-Finno-Ugric branches.

619

===== PAGE 610 =====

The research possibilities of the exclusively Finno-Ugric perspective have clearly been exhausted long ago, and in the last half century they have been practically barren.

The activity of this school is manifested in repetitions, in the ever-new grouping of the sparse linguistic material, and in the development of various unprovable chains of linguistic, paleontological and prehistoric hypotheses built on these incomplete linguistic data. In other words: research along an exclusively Finno-Ugric line has come to a dead end.

It is therefore obvious that in the future, our investigations to uncover the development of the Hungarian people and language must be completed in the direction set by Gyula László, and only with his complex methodology can we achieve more certain results in this field than before. This is also the case with the question of our Turkish loanwords.

A few words about the "Bulgarian-Turkic" languages

We have seen above that our linguistics attributes outstanding importance — and we readily agree with it — to our 'Chuvash-like' Turkish word parallels. We have also pointed out that the only 'Chuvash-like' Turkish language known today — that is, Chuvash itself — developed relatively late, and mainly in an environment where the descendants of the Hungarians who remained in the east played a very significant role, and it can even be proven that a compact group of these Hungarians merged with the Chuvash. On the other hand, our linguists have shown that in the 1st millennium AD, several Let us ignore for the time being the fact that — as we mentioned earlier — the very high degree of similarity of the Turkic languages even today makes the idea of "Chuvash" Turkic languages splitting into several significantly different language varieties 1500 years ago highly improbable, and let us first look at what we know about the Middle Bulgarian, Hun-Bulgarian or Onogur-Bulgarian peoples.

The data that we must emphasize in the Preface is that the languages of these peoples are practically unknown, and their alleged "Chuvash character" is itself only a linguistic — and not a historical — hypothesis, which is not least supported by Hungarian linguistics and prehistory research with a Finno-Ugric perspective. Thus, the same vicious circle that Dénes Sinor — To explain the above-discussed sound differences in Hungarian words considered to be of "Chuvash-Turkic" origin, he proposed the so aptly describes was at work in the birth of this thesis (1. pp. 601-602).

As a second very important aspect, we point out that the term "Bulgarian" is a general term in international scholarship.

===== PAGE 611 =====

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According to his theory, "means "mixture", i.e. a mixture of peoples that were composed of two or more ethnic groups, 5 the appearance of unity being only due to within the framework of the various steppe horse-nomadic empires, these ethnic groups appeared to outside observers as one people, since they belonged to the same political organization.

An undoubted sign of this confusion is that in the centuries following Attila's death, in the same areas of Eastern Europe, we encounter dozens of different ethnic names in the wellheads of the same period, very often even in cases where the same ethnic groupings are clearly involved: Hunugur (Hun-Ogur), Onogur, Onogur-Hun, yyy-gur-Bulgar, Hun-Bulgar: Sabir, Sabir-Hun, Hunugur-Sabir; Ogur, Ugor, Ugni, Ugri; etc. etc. The picture is only complicated by the fact that, in parallel, the sources often mention these peoples by the contemporary names of the ever-changing smaller tribal alliances: on-ogur (ten ogurs), bes-gur (five ogurs), utur-gur (thirty ogurs), alti-ogur (six ogurs), úcs-ogur (three ogurs), kutur-gur (tk. tukur-ogur = nine ogurs), sar-agur (white ogur), etc. An exhaustive summary of these people names can be found in the works cited by Géza Nagy and Gyula Sebestény.

At the same time, however, we must not forget that the ancestors of the Hungarians, according to all available data, are to be found among these "Bulgar", "Ono-Gur-Bolgār", "Ogur-Ugric" and Hun-Bulgar-Szabir" groupings: our conquering ancestors are also predominantly mentioned in contemporary sources under the names Hungar, Ungar (Hunugur, Onogur), Ugri, Ougroi (Ugor, Ogur), and Sabir.

All this suggests that the idea of a predominantly Turkic language and ethnicity of these mixed ethnic groups — which Hungarian linguistic and pre-historical research tacitly posits — is not only unverifiable, but also downright improbable. If all these peoples had really been Turks in their majority, then — as we will show in detail below — we would speak Turkish today and not Hungarian. Where are the descendants of these "Western Turks"? Where have they gone — despite the fact that in the 10th-13th centuries they received massive replenishment and replenishment from the east with the migrations of the Pechenegs, Uz, Cumans and Kipchak-Tatars? Their traces can only be interpreted in the Turkic language of the Danubian Bulgars.

We find them in inscriptions, but they could not have been many, otherwise they would not have been Slavized within a century or two. Another remnant of them is perhaps the Chuvash ethnic group, although it is very doubtful whether they were originally Turks and did not become Turkized only in the 10th-13th centuries - precisely under the influence of the invading Bashkirs, Kipchaks and other Eastern Turks. We remind you that according to the studies of F. Hanczar, the language, legends and folk customs of the present-day Chuvash are full of similarities with the Caucasians, and their anthropological structure is also very close to the 621

===== PAGE 612 =====

they are more similar to the Armenian and Eastern Mediterranean races (Book 1, p. 48). This has also been confirmed by recent Soviet anthropological research. According to Professor Smirnov, Tibor Tóth's Declaration; Boros-Rapcsányi: Vendégségben "One of the fundamental factors of the Volga Bulgars was the numerous groups of the Volga Sarmatian period, certainly of southern, eastern Mediterranean origin." (1. yseinknél, p. 203).

From what has been said so far, we can conclude with considerable probability that these "Bulgarian", "Hun-Bulgarian", "Onogur-Bulgarian" or "Ogurian", "Ugric" ethnic groups consisted of Turkic, i.e. Altaic elements, ethnically and linguistically, only to a lesser extent. However, there is other data in this direction.

For example, according to several renowned researchers, the Sabirs were not Altaians (F. Altheim, HW Haussig, etc., our book I. y., p. 267). This is very important, because Constantine, born in Bibor, directly writes that the name of the Hungarians was Sabir in the past. It is therefore reasonable to assume that several of the tribes of the Sabir tribal alliance spoke Hungarian. This probability is almost certainly confirmed by Theophanes' well-known statement that the head of one of the western tribes of the Unni people called Sabir - Gordas - was baptized in Byzantium, but his people rebelled against him and elected his brother, Muageris, as their prince.

Thus, two sources, completely different in nature and relying on independent data, mutually support each other; Emperor Constantine heard about the old Sabir name of the Hungarians from the Hungarian leaders who visited him, and Theophanes, based on historical works, mentions the personal name Muager (-15) among the Sabirs, the connection of which with the Hungarian ethnonym is also recognized by the majority of our historians. Moreover, at the same time, our names Ungar, Hungar, Ugri used abroad are also closely related to the Sabirs: we know that Jordanes uses the ethnonyms Sabir and Hunugur (onogur) as synonyms, and Victoris' chronicle calls the Sabirs "ugni", i.e. Ugrians (see Gy. Németh: Attila and the Huns).

The situation is similar in the case of the Khazar language. Various Arab sources speak of the Khazar language as either Turkish or completely foreign, completely different from Turkish, but not comparable to Iranian. Since the Khazars ruled over several peoples, there is no contradiction in this, but it is very significant for us because the language of the people in question belonging to the Khazar Empire was neither Turkish nor Iranian: at that time and in that region, it could hardly have been anything other than Hungarian. In this regard, Masudi's statement that the real name of the Khazars was Sabir is very serious,

We do not want to dispute that a significant proportion of the population of the Khazar Empire spoke Turkic, we would only like to point out that there was by all rights a very significant number of Hungarian-speaking people among them.

622

===== PAGE 613 =====

We can count on ports. This is also strongly supported by the statement of Emperor Constantine, according to which the Kabards, who broke away from the Khazars, soon learned the language of the Hungarians, but the Hungarians were also taught their own language. Well, this mutual and relatively fast (less than 55 years) language learning in Hungarian means that the two languages could not have been very far from each other. It should also be noted that the emperor does not speak of completely separate languages, but specifically of the dialects, i.e. dialects, of the Hungarians: "kai mechri tou nun tén autén dialekon echousin" (De admin. imper. chap. 39).

prove with very thorough reasoning the presence of the Hungarians among the ethnic groups of the mixed people called "Bulgars". But the others, specifically Simonyi Dezső Jordanes, In his study "The Pannonian Bulgarians and the Formation of the Hungarians". Using contemporary source data, he shows that the Onogurs (i.e. actually of the Hungurs—Sabirs) in the Carpathian Basin since the 470s. In fact, even earlier: according to Jordanes, these "Hunugurs/Sabirs" already lived in Thrace, Moesia and Dacia - i.e. in Transylvania and Trans-Tisza - during the Hun Empire, and after the death of Attila they retreated to the Pontic region.

In connection with this data of Jordanes, we must definitely point out the very obvious possibility that the Szekler are perhaps the descendants of the remaining fragments of these Sabirs (or Hun-Ugurs). The probability of this assumption is greatly confirmed by the results of the research of the Austrian professor Mitscha-Märheim, according to which several Hunnic tribal groups lived in the Carpathian Basin even after the death of Attila: the Sadags and Turkilings certainly, and the Szirj of Edekon and Hunwulf — at least in part — can probably be counted among the Huns (more on all this in I. pp. 286-287).

Further on, based on contemporary source data, Simonyi proves categorically that the steppe, Bulgarian" — i.e., basin, during the 5th-7th centuries, "mixture" — the repeated intrusion of peoples into the Carpathian

1. Around 475 the The arrival of "Pannonian" Bulgarians.

Note: perhaps at the call of Edekon or his son Hunwulf? — We know, that around this time the Ostrogoths marched to the Balkans, thus Pannonia was partly vacant, and at the same time Hunwulf was appointed by Byzantium as magister militum, or military governor of Illyria and Pannonia (1. Mitscha-Märheim: Die Herkunft der Baiern; — described

Book II, pp. 287-288).

2. In 568, Bulgarians also arrived with the Avars; in addition to these, at that time 10,000 Kutrigur Bulgarians settled in Dalmatia.

3, in 597 new "Varchonites" come to the Carpathian Basin: the Tarnids; Koks, the Kotzagers and the Zabenders (the latter name apparently contains the ethnonym Sabir; ~ our note).

623
===== PAGE 614 =====

Among these we can count — as a fourth wave — the Onogur-Bulgars of the Griffes-Indás lineage who arrived in the Kárpát basin around 670, Gyula László, the early Hungarians whose immigration Simonyi does not consider proven, but the archaeological finds speak in clear language of the mass appearance of a new people from the east at the end of the 7th century (L. László jý. Simonyi's information that the Avars occupied Friault in 663 is also very important. double conquest"; Mitscha-Márheim: id. m. 1963). This is because the occurrences of the folk names "Ugor", "Hungar", "Hungarian" in the Friaul area in several sources before 895 can be explained in an obvious way (L. II. our book, pp. 366-367). Simonyi supplements the main data listed there with another: in the second half of the 9th century, the Bavarian Geographer in the Danube-Tisza region Vulgares, He mentions the Ungare, etc. people (see Simonyi: Studies on the Bulgarian-Hungarian Relations, p. 84). However, these historical questions are of secondary interest to us now, since the main emphasis this time is on our Turkish loanwords. Let us therefore turn to the linguistic history and development implications of Simonyi's investigations, which he discusses in detail in the last pages of his paper (l. id. m. pp. 84-88). In the preface, he emphatically emphasizes: "... the problems of the formation of our Finno-Ugric-speaking people are shrouded in obscurity... instead of not quite satisfactory hypotheses, new clues must be sought... First of all, we need to revise two previous assumptions. One is the generally accepted hypothesis, according to which the seven tribes of the Hungarians arrived in our country in 896 with such a large Finno-Ugric mass that it numerically surpasses the population found here, and this would be the reason why our language could still survive. Of all this, only the fact of the conquest and the founding of the state can be accepted, while the other statements cannot be justified, and as we will see, they contain many contradictions." (id. m. p. 84) In connection with this clear criticism, we recall our own objections of the same nature, which we have repeatedly presented in our Book II (1. 257. and 362-363. pp.). At the time, we stated that our exclusively Finno-Ugric historiography unfoundedly inflates the number of conquerors, and at the same time strives with all its might to marginalize the determining role of the population of the Carpathian Basin before 895 in the ethnic composition of the Hungarian people, which anthropological studies, starting with the research in this direction by Lajos Bartucz, have increasingly and unequivocally confirmed. We also pointed out that the exclusively Finno-Ugric school needs this manipulation because otherwise it would be inexplicable that our people still speak Hungarian today, in the foreign-speaking mass of the former population of our country.

624
===== PAGE 615 =====

For Árpád's colonists — a significant proportion of whom, moreover, probably did not speak Hungarian, because they consisted of Turkic-speaking ethnic groups (cf. our Turkic tribal names!) — would inevitably have had to lose their Hungarian language, or at least such a degree of linguistic alienation would have had to occur that a kind of mixed language would have developed, which in its structure and vocabulary would be somewhere halfway between the Turkish, Slavic and modern Hungarian languages, at their intersection.

Simonyi then explains that the other hypothesis - the theory of Géza Nagy and others - according to which Finno-Ugric-speaking tribes appeared in the Carpathian Basin long before the conquest - this thesis has been ignored until now, but not refuted -, although it cannot be proven, "... but nevertheless it can be said to be more probable than the currently accepted theory, because it contains fewer contradictions and makes many things understandable. Even if I cannot agree with G. Nagy's hypothesis in some details, I must state that his statements are close to the conclusions I have drawn." (id. m. p. 84)

He further states that the assumption prevailing today "... excludes the necessity of the gradual development of our Finno-Ugric-speaking people, and thus violates the law of transformism. First of all, it is in no way believable that barely a century would have been sufficient for the assimilation process of our people and language, nor is it acceptable that the assumed social transformation and the creation of new settlements could have taken place during the 10th century. All this would require at least two centuries, not to mention that, if this hypothesis is maintained, it is not even conceivable that the aforementioned transformations could have taken place in seven or eight decades after the Age of Adventurers (899-970), and certainly not during it... On the other hand, the 11th century Our sources from the beginning of the 19th century also reflect conditions (social strata, agriculture and other branches of occupation, villages, geographical names, etc.) whose development requires a longer history. Older interpretations of this cannot be taken into account without taking the following facts into account." (id. m. p. 85)

According to Simonyi, these facts are as follows:
1.2 The finds from the Avar-era cemeteries with several hundred graves suggest class division long before the conquest.
2. More and more so-called Late Avar cemeteries from the 9th century are becoming known.
3. The former settlers of the 16th-19th century cemeteries constituted the common people of that time, so the names of the settlements derive from them,
47 The finds also reveal that the main occupation of the common people was agriculture, but there are also finds indicating craftsmen, fishermen, and shepherds.
625

===== PAGE 616 =====
If we take these objective facts into account - he continues -, the written records of the 11th century become more understandable. The So there were quite a large number of poor workers here, that is common people, whom Árpád and his leaders had already found here, did not have to provide for a "large" army of servants to augment the "small population";" (id. m. p. 86 ~ emphasis by Simonyi)

Simonyi then emphatically emphasizes that the survival of the Avar-era common people was confirmed by the results of historical anthropology. Bartucz had already established in 1934 that the Avar types did not die out, but survived in the Hungarians of the Árpád era and have survived in various regions of our country to this day, so the Avars played a primary anthropological and ethnic role in the construction of the Hungarian nation, and according to him, the survival of Avar elements can be proven en masse. These anthropological findings are of course not meant for the thin ruling class, but for the common people of the Avar period: "... The ruling class did indeed cease to exist, perhaps even perished... but the people remained, and in our opinion it was this defeated people that appeared in our sources under the name Bulgarian for several centuries. We can see how important the finding of historical anthropology is that the anthropological types of the Avar-era cemeteries differ only slightly from the types of the bone remains of the larger cemeteries of the Árpád-era. This confirms that the survival of the Avar-era population lasted not only until the 9th century, but also during the 10th and 11th centuries. Furthermore, it has been confirmed that in our larger "Avar-era cemeteries, various anthropological types occur mixed together, i.e. not only European, but also Eastern (Mongolid, Mongoloid, Turanian, Pamiri and others), which is undoubted proof of the mixed nature of these peoples. It can also be stated that ethnicities (tribal elements) no longer depend on social strata, there are rich and poor people of all races." (id. m. p. 86 — emphasis by Simonyi.)

Finally, Simonyi raises the question of the language of the common people before the conquest. Although we do not have specific data on this subject, "... but it is certain that the linguistically ununified (called Bulgarian) ethnic groups assimilated each other over time. One of these groups certainly assimilated the several Ural-Altaic tribal groups that settled with them, including the leading class, so that the process must have occurred in all probability over the course of two or three centuries, since various Ural-Altaic ethnic elements were present here from the turn of the 5th-6th centuries..." (ibid., p. 86-87)

What kind of language was the assimilated one that absorbed the others is clearly shown by István Kniezsa's 11th century settlement map, Simonyi states. There is no doubt that this situation reflects a picture valid for the 10th century, which is a fact of linguistic distribution.

===== PAGE 617 =====

points to the inhabitants, which took several centuries to develop, the fact that our Finno-Ugric language has occupied such a large area in just seven or eight decades would certainly be considered a very wonderful phenomenon. In the Kniezsa map we can see the spread and even symbiosis of two dominant peoples, and even some Turkish "islands". From the 7th-8th century sources, two dominant peoples are also mentioned: the Hun-Bulgarians and the Slavs... On the Kniezsa map, this population is everywhere Hungarian-speaking in large areas... Our Hungarian place names originate from the common people already living in the Finno-Ugric language community, to whose sound system the other Ural-Altaic languages have adapted. We would just like to note that if we can confirm the sighting of Géza Nagy (71915), it will be easier to understand the survival of the Hunnic tradition, the adoption of our Bulgaro-Turkish loanwords, and many other difficult questions of prehistory." (id. m. pp. 87-88)

If we follow this complex analysis of Simonyi carefully, we must come to the compelling conclusion that the "Bulgarian" language that flowed into the Carpathian Basin after Attila's death, i.e. our language, would have been present among the In each of these "mixed" ethnic groups, we must reckon with a very considerable number of Hungarian-speaking ethnic groups, otherwise it could not have remained mass of peoples speaking other languages. This means in Hungarian that the majority of the peoples called "Bulgarians", "Onogurians" or "Bulgar-Turks" spoke Hungarian.

The first-rate evidence for this is provided by the Turkish-language Hungarian chronicle "Târih-i Ungürüşz", which is about to be published in the translation by Prague Turkologist Professor József Blaskovics, and was compiled by Mahmud Terdzûman, the interpreter of Suleiman II, from the Hungarian codices he saved during the capture of Székesfehérvár in 1543. According to Professor Blaskovics' preliminary information, the chronicle never mentions Hungarians, Avars, or Huns, but always mentions only the people of Hunor, the "Üngürüşz". It describes how these Üngürüşz had already occupied Pannonia before Attila, under a leader named Katar. Later — the chronicle does not reveal how much time passed — another wave of Üngürüşz arrived in Pannonia, and the people they found there spoke their language — that is, Hungarian. Then there is another blank spot, and then a branch of Hunor's people living in Scythia reappears: According to the chronicle, the Almos were sent by relatives living in Pannonia to come after them. Professor Blaskovics then states with emphasis: "The chronicle fully proves the double conquest, and makes the triple one probable. But other Turkish sources also mention the peoples of the Carpathian Basin, and according to them, three Hungarian waves can be distinguished." (Professor József Blaskovics to journalist Tamás Horváth, in "Új Tükör" issue 31, 1981, under the title "The History of the Huns"), statement given; published in 627

===== PAGE 618 =====

It is also essential to note that the "Târih-i Ungürüşz" was discovered by Vámbéry in Constantinople more than 120 years ago, and the Turkish-language manuscript has been gathering dust in the Academy's library since 1860. Budenz declared it "even more vile than the vile", and his army of loyal disciples firmly condemned the authoritarian principle that is rampant like a cancer in our historical science. "methodological" basis — he paid no attention to it, but accepted it blindly, even the great master

Well, from the above brief description, we can see why Budenz threw the “Tárh-i Ungürüz” in the trash: by claiming the triple conquest of Hungary and the Hungarian language of the Huns and Avars, the chronicle clearly refuted Budenz’s and Hunfalvy’s theories about the exclusively Finno-Ugric origin of the Hungarians, the adoption of the Hungarian Hunnic myth from the Nibelungen, and so on. In other words: the sinking of the “Tarih-i Ungürüz” once again shows more clearly than ever that Budenz did not research with objective means, did not serve science in the true sense, but in the interest of their preconceived ideas, from the very beginning they systematically silenced and disappeared all data and sources that spoke against the exclusively Finno-Ugric derivation of the Hungarians. In other words, they consciously manipulated the known historical data. This “good habit” has been faithfully preserved by Hungarian prehistory research, working on the conveyor belt of Finno-Ugric linguistics, to this day — as we have already shown in countless cases in this series of studies.

All these circumstances: the testimony of the “Tárh-i Ungürüz”, as well as the contemporary written sources, archaeological finds, the results of anthropological studies and the lessons of place names, through the approximate and extensive comparison of which Simonyi developed the historical, ethnic and linguistic picture outlined above, we are convinced that not only do they prove that Géza Nagy and his followers were right in that “Finno-Ugric” speaking tribes appeared in the Carpathian Basin long before the conquest of the people of Árpád, but they also make the other organic component of the theory of our scientists very likely, that the Eastern European peoples mentioned under the names “Hun-Bulgarian”, “Hunugur-Onogurian”, “Sabir-Hun”, “Ugor-Ogurian” and “Bulgarian-Turkic” did, by all indications, really form a separate ethnic and linguistic community, such as Nagy Géza and his family understood the concept of “Ugric-Hungarian” peoples and languages.

We can therefore state that, in addition to the Chuvash, those other “Turkic” peoples among whom our linguists are looking for the origin of our so-called “Bulgarian-Turkic” loanwords with the “Chuvash criterion” and the explanation of the phonetic differences of these Hungarian words, are for the most part hardly distinguishable from the ancient Hungarians who were in Eastern Europe in the second half of the 1st millennium AD, and according to all available data, a considerable amount of = 628

===== PAGE 619 =====

However, they probably included a dominant majority of Hungarian-speaking ethnic groups. This is also confirmed by the various names by which contemporary sources refer to our conquering ancestors: Magyar, Turk, Hun, Avar, Ugri, Ungar, Hungar, Sabir, etc. In the light of these insights, perhaps Gyula Sebestyén’s interpretation was not wrong either, who explained the ethnic names “ut-urgur” and “kut-urgur” as “ot-ugor” and “hat-ugor” in Hungarian, and the names “bes-gur” and “alt-ogur” that appeared somewhat later in the same region as the Turkic names of the same peoples (bes = five in Turkish; alti = six) (Sebestyén Gy.: The Conquerors; Kodolányi: In Search of the Finno-Ugric Homeland). It is easily possible that these mixed peoples had parallel Hungarian and Altaic-Turkic names, which were used alternately according to the ethnic and linguistic affiliation of the tribal confederation.

Let us now look back to the times before the Hunnic era, and note that in the historical and geographical data of ancient authors, traces of the Western Turks and the “Ugric-Hungarian” peoples of Géza Nagy appear closely side by side in the Eastern European-Western Asian region. Strabo mentions a people called “Urg” (Ourgoi) near the royal Sarmatians, and also states that they “are said to live on both banks of the Ister”. (Strabonis Geogr. libri XVII. C. III.) We can consider this information very significant because, in our opinion, it can clearly explain the “battle cry” with which the leader of the Pannonians, who were rebelling against the heavy tax, threw his boot at the emperor Constantius, who was trying to force them to pay the tax, saying: “Cattle, cattle!” (Ammianus Marcellinus: Rerum gestarum XIX. 11. 10.) This event took place in 359 AD in the Szerém region, around Sirmium, before the great Hunnic migration, and suggests that we must also count on a considerable number of Hungarian-speaking people among the Sarmatian tribes called “Jazig”. Pliny mentions them between the Tanais River (Don) and the Ripheus Mountains (Urals), and “turcae” people. Pomponius Mela also speaks of a “turcae” people in the same region. Ptolemy mentions them alongside the royal He reports on a people called “materi”. According to his data, this materi people lived on the left bank of the middle Volga. Sarmatians. Further east, in the area of Lake Aral, in Turkmenistan, we have already become acquainted with the early Parthian data that testify to the close coexistence of Sabir-Hungarian and Turkic peoples: the second half of the ethnonym “saka-uraka” presumably preserves the plural form of the Hungarian common word “úr”, and the singular version is known from the coinage of an Indian king “sakauraka”, on which the king calls himself “sakaur”. At the same time and in the same place, the name of the Sabirs also appears in the form of “sabarol”, and the Avar (apar, aparnoi) is also a folk name. The presence of the Turks is also indicated by the princely titles of the “vusuns”, “kuns”, and the name “pasiani” (paskin), which 629

===== PAGE 620 =====

The name “Asi” is very reminiscent of the name of the Pechenegs, the ethnonym “Asi”, which with a strong Valosian “inflection” means the “Uz” (this is also confirmed by Bériini’s above-mentioned report that the “Aes” lived in ancient times with the Pechenegs on the banks of Lake Aral, and their language was strongly mixed with the language of the Pechenegs and the Chovarezmi), and finally the name of the “Kang-kü” empire mentioned in Chinese sources can be linked to the “Kangar” tribal group of the Pechenegs (L. 270-271., 289-293., 567. and 571.pp.). Well, in light of the already mentioned anthropological studies of Tibor Tóth, according to which the anthropological counterparts of the conquering Hungarians are found among the Sarmatian finds, in the Sarmatian-era graves of the region between the Caspian and Aral Seas, we can very likely say that all these data cannot be a coincidence, and that the ancestors of the Hungarian ethnicity in the Sarmatian and Parthian periods must certainly be sought among the Daha and Saka (Sabarol, Sparnoi, Aparnoi, Sakauraka) tribes of the Caspian-Aral region, which, as mentioned above, were already ethnically mixed at that time, and included a significant amount of Turkic elements, and the further east we look, the more Altaic Turks we have to count among them. From all this it follows that the Western or “Bulgarian” Turks had close relations with the ancient Hungarians since Sarmatian times.

It is therefore obvious that the “Chuvash-like” Hungarian-Turkish word parallels, in the light of the historical, ethnonym, anthropological and other data listed above, are not necessarily Turkic loanwords in Hungarian, but on the contrary: they can be explained much more clearly as Turkic borrowings from Hungarian, or more precisely from one of the dialects of the proto-linguistic group that Géza Nagy and his colleagues called the “Ugric-Hungarian” language community. Our Turkish loanwords and Sumerian linguistic monuments Let us then see whether we can find linguistic traces that support this view, which can most likely be deduced from historical and other data, from the linguistic side. Let us start from the assumption that if we are on the right track with this thesis (which is actually a modern development of the theory of Géza Nagy, Károlyi Fiók and their colleagues, expanded using the latest archaeological, prehistoric and language development findings and embedded in our current knowledge), then we should expect that on the one hand, in the so-called “Chuvash” criterion layer of our Turkish loanwords, we will find phonemes that differ from Common Turkic in some ancient language in the same form as Hungarian, and on the other hand, that close parallels of Hungarian words without criteria or derived from Common Turkic can also be found in the same ancient language.

630

And then
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So let us take a look again at the Sumerian linguistic monuments, in which we have found a large number of close parallels to our words, which are said to be of ancient Ural-Finno-Ugric heritage and considered to be loanwords of Iranian origin. Below, we first present the Sumerian equivalents of twenty-two so-called “Chuvash-like” Turkish loanwords. For each word, we always first provide the TESZ etymologies.

- ý. calf (Kalb, Jungtier): Old Turkic loanword of Chuvash character; vý. Chuv. páru Osm. buzayı , calf”; also found in Mongolian; vý. Mong. bírayu; burj. The Iranian origin of the Mongolian word burý is “Uaz”. The Turkish-disputed.
- Sumerian “buru,” = “Vieh, Kleinvieh” (domestic animal, small cattle); cf. also barom, birka, bürge, bārāny.
2. del (Mittag, Süden): Old Turkic loanword with Chuvash characteristics; vo. Kyrgyz. tus “del as a time of day and as a world”; cf. also Mong. dili “uaz”. It does not exist in modern Chuvash.
- Sumerian “dal” = “Flamme, Hitze, überreich an Glut” (flame, heat, sweltering heat); in composition: “dal-ba-na” = “Mitte” (middle).
3. dól, dil (to stretch, lie, come into an oblique position, sink, collapse, flow, decide): Chuvash-like loanword of the fifth century; cf. modern Chuvash tús- “to fall, to collapse”; the modern Chuvash equivalent is missing.
- Sumerian “dul” = “überschwemmen” (to flood); cf. dól a viz:= “niederwerfen” (to throw down, to lift, to throw down); cf. le-dól; in compound: “dul-lu” = “Trümmerhügel” (debris hill); cf. dilledék.
4. planer, planer (Hobel, Planierbagger, hobeln): of disputed origin; a) an Old Turkic loanword of Chuvash character — but its direct antecedent cannot be traced, only its possible verb stem and other derivations; cf. Common Turkic yis-, yisi-, planer”; absent from Chuvash; b) an ancient legacy from the Ugric period; cf. jar, iarra “horseman”.
- Sumerian, gal,” = “to flatten” (lapit, simit, vasa!); = “a part of the Pfluges” (a part of a plow), akkádul: “le’u” = “Tafel” (table, sheet, plate); that is, apparently, the metal plate called today a “plough” (Pflugschar), which cuts and levels the ground horizontally.
5. weed (Unkraut): Old Turkic loanword of Chuvash character; cf. Chuv. sum “weed, weed”; alt. yoy “weed raised on the lawn”6.
- Sumerian , gum” (bum) = “to crush, to destroy” (to break, to destroy); cf. also , gaz” = “to break, to break, to damage, to kill” (to break, to

631

===== PAGE 622 =====

- or the weed damages, destroys, kills the crop), wades, rascal,
6. ring (Ring, Reif): Old Turkic loanword of Chuvash character; Cf. Chuv. (s9r2) osm. yüzük, “ring”. Its root word is the Turkish yüz, “feeling, member, finger joint”, Sumerian “gur” = “drehen, siegeln” (screw, roll, seal); Cf. a ring is a ring-shaped object twisted around the finger, which is also used for sealing, and in the past — especially for men — this was its primary purpose; “gur,, kur,” = “sich beugen, kriimmen, biegen, Krümmung, Kreis” (bend, curve, curve, curvature, circle); “har, hur” = “Ring, Krümmung, Schlinge” (ring, hook, loop).
7. bell (Glocke): of Turkish origin; cf. Chuv. xyan; Osm. gazan , cauldron”. The Mongolian garangxa “Erckorong, gong” is a Tibetan loanword and does not belong here.
- Sumerian bar” (hur) = “schreien, brüllen, dröhnen, Geklirr, Getöse” (shout, make noise, jingle, clang, music); cf. harsog, hurrog, kurjant, snort etc. But cf. also the “bar, bur”; “gur,, kur,” with the aforementioned meanings (krümmen, biegen, Krümmung, Kreis). And then, viewed in this Syntlingual context, it seems very likely that the above Tibeto-Mongolian word is still belongs here”,

Moreover, we even dare to question whether our word "körġ" is actually a Slavic loanword — as the TESZ claims. We must also accept with great skepticism the statement of the TESZ that the word "circle" "came into being" through word-
deduction from the words "around", "around-around", etc. This is because the TESZ itself derives the word "circle" from the root "körty", which is "an ancient heritage from the Finno-Ugric period" and means in various Finno-Ugric
languages "tire, ring, small wheel, disk, hoop, cartwheel, loop, string ring, etc." Here, Géza Bárczi's epoch-making biological-developmental discovery, already mentioned, is again palpable, according to which the "expansion of consciousness"
among the Hungarians 800-1000 years ago had not yet reached the stage at which "the loose succession of a small number of concepts flashing in a narrow consciousness was replaced by a slowly expanding consciousness,
in which the larger number of concepts are grouped into a close logical relationship." (1, pp. 534-535) That is, in the case of the word circle, the TESZ does not assume that the Old Hungarians used this geometric concept 8, idd (Zeit, Jahr,
Zeitpunkt, Zeitalter, Wetter, Gewitter, Sturm): disputed 6100618. a) Old Turkic loanword of Chuvash type, The Hungarian word
their "narrow-scoped consciousness" could have perceived.

632

===== PAGE 623 =====

no corresponding Turkish antecedent has been identified, only the root word of the presumably formed word; cf. Turkic dd "time", Cf. also Mong. üde "evening, noon", It is absent from Chuvash. It is a certain phonological problem that, based on
the Hungarian linguistic history of s76, the Proto-Hungarian "idey antecedent should be fol. b) A consolidated syllabic formation, the hyphenated version of the adverb ide.

Sumerian "ud" = "Tag, Zeit, Monat, Wetter, Sturm" (day, time, month,
weather, storm); cf. with the different meanings of time! ,1d," = "Monat" (month).
According to these, the solution to the above "phonetic problem" can be easily found.

We can leave it to the Finno-Ugric linguistics based on the sound law, because on the one hand the semantics of the Sumerian "ud" (id.) is completely identical to the meanings of the Hungarian time (the regional language "üdö6"), and on the
other hand this so-called "problem" was constructedor itself only by Finno-Ugric linguistics anyway; on the basis of the Hungarian linguistic history of the word, no "idey"-like phoneme can be assumed in Old Hungarian, considering that none
of the examples cited by the TESZ in the linguistic records contain the ending "g" or any other consonant.

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ir (malen, schreiben): Old Turkic loanword with Chuvash characteristics; cf. Chuv.
Sir-, osm. yaz-, write". Cf. also Mong. jiru- „, draws a line, paints, writes".
Sumerian "ir," = "einritzen, zeichnen" (scratch, draw); "har, hur, ur-, ri,," = , uaz"; cf. also scratch, scratch, groove, ró.
writer (Buttermilch): an Old Turkic loanword of Chuvash character.
Its equivalent is only found in the second member of a Bashkir compound; v6. bask. may
yaöu , butter". The verb stem is bask. yaö-, yaz
"churn".
Sumerian "ir" (Akkadian izutu) = "Schweiss, schwitzen" (sweat, sweat);
cf.: "black butter" - "sweating butter" (?)
Note: this parallel can only be considered flawless after the transfer of meaning is supported
by other means.
Peg (Pfahl, Zaun): Old Turkic loanword with Chuvash characteristics.
There is no equivalent in modern Chuvash, but it is generally widespread.
and other Turkic languages; cf. Ottoman. kazyk. "pole". It is also found in Mongolian

Szumér , gar" = "to enclose, enclosure, border"
(enclosure, enclosure, fence, frame); "arm., arm," = "enclosure"
(closure, enclosure), cf. enclosure (according to TESZ, perhaps a Turkish loanword, but
it could be Slavic).
yellow, mud (hell, bläss, gelb): Old Turkic loanword family with Chuvash characteristics; cf.
Chuv. \$yró, white"; Türk sarı , yellow, pale". Can also be found in Mongolian; cf. Mong. Sara,
Sira , yellow",
Sumerian "Sar" = "gelb, fahl, bläss, grün" (yellow, dull, pale, green).

633

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dragon (Drache, cine Art Schlange): Old Turkic loanword of Chuvash character; cf. Osm.
sazagan "dragon, large snake"; the Chuvash bdl is missing.

Sumerian "Sur," = "schrecklich, grimmig" (terrible, horrible,
fierce, fierce); cf. terrible, monster, and: "kur" = "Schwanz" (tail); in compound form therefore: "Sur," + "kun" = sickle (Sichel): probably an Old Turkic "wolf monster".
loanword of Chuvash character; cf.
csuv. On the sickle". The word has not been found in any other Turkic language, its
origin is unclear. It has entered several Finno-Ugric languages from Chuvash,
as an independent adoption from Hungarian.
Sumerian "Sar," = "schneiden, abschneiden" (cut, cut off); cf. also sara- rabol (according to the TESZ, a borrowing of the Bavarian-Austrian "schrabben" = , scrape, scratch").
Note: since the word "siró" does not occur in any other Turkic language besides Chuvash, we can say with a probability bordering on certainty that it can hardly be an "Old Turkic loanword" of Chuvash character in Hungarian, but is
clearly a Hungarian loanword in Chuvash, and likewise the Finno-Ugric parallels are not borrowings from Chuvash independently of Hungarian. However, the TESZ - characteristically - does not even raise this possibility: after all, according
to the Finno-Ugric dogma, we would have learned agriculture from the "Chuvash"-speaking Bulgar-Turks...

sweep (fegen, kehren): Old Turkic loanword of Chuvash character; cf.
csuv. (SdBdr) , broom"; new. sipir- , broom". Cf. also mong. sigür-,
Si'ür- "broom". Originally, it was an expression related to agricultural life; it denoted the
gathering and sweeping of grains.
Sumerian. ~~tegeb~~, reinfegen, reinigen" (sweep, sweep, clean);
"sub-, su-ub" = "abwischen, abstreifen" (to wipe, to clean, to strip, to strip off); doubled: "sub-,sub," = "ernten" (to reap); this word therefore points back to the most ancient early Neolithic times, when the harvest still consisted of manually plucking
the grains from the ears of corn. The Sumerian "sibir," = "Ernte" (harvest) may be the result of a later word formation.
threshing floor (Tenne; ring-shaped place, object): probably a 6th-Turkic loanword of the
Chuvash-505 type; cf. Chuv. (\$drt) accusative yüzük , ring"
etc. All these Turkish words are also the predecessors of our word ring.
Sumerian "sur, kur" = "begrenzen, abgrenzen, Umschliessung, Gemach" (to delimit, to enclose, to enclose, to locality); cf. also half-time, barn, barnyard (with this latter word cf. also Sumerian "Suru, Surun" = "Stall,

===== PAGE 625 =====

Pferch; pen, stable, corral"); = "spinnen, drehen, bigen, beugen, wickeln, schlingen" (spin, weave, turn, curve, twist, loop); v. serit (wind), csür(-screw), winch.
From the above meanings, it can be clearly established that "sur, Zur"
it is merely a synonymous variation of "gur., kur,," with a hissing sound, one might say "satem"
sound (Book I, pp. 140-142 and 187-188).
It will not be uninteresting if, by way of comparison, we briefly
We will review the derivation of the above Hungarian words according to the TESZ.
half-shed, shed (Schuppen): a combination of half (halb) and "ser, row, side"; barn (Scheune, Tenne): a Bavarian-Austrian or Middle German loanword (despite the fact that its second meaning is "Tenne", i.e. threshing floor)).

hürde (hürde, flechtwerk): probably a derivative word; it could have originated from a verb *ser -tsir -sür, probably belonging to the family of the verb szy;
serit (spinnen, drehen) and serül (sich wenden, sich drehen): word family of onomatopoeic origin;

csýr (drehen, spinnen): the independent element of the twin word csűr-csavar, which, however, comes from the verb çvay, which has a disputed origin; a) Hungarian development, the counterpart of the verb kavár; b) Turkish loanword; cf. csag. Cavar-, turns, turns, screws"; winch (Spulrad, Hebewinde, Spill): a derivative of the noun pipe (Rohr), which in turn is a Slavic loanword. 17. szýcs (Kürschner): Old Turkic loanword of Chuvash character; cf. Chuv. (Savaza) , tailor"; alt. yikci , seamstress, one who sews". These are derivatives of Chuv. (Sava) or the Common Turkic yigi, yig, yik , "seam, seam". The word is also found in Mongolian. Sumerian "sig-, sikr" = "Wolle, Haar, Haarfell, Fellkleid" (wool, fur, fur coat, clothing made of fur). 18. szúr (sehen, filtern, Wein ausheben): Old Turkic loanword of Chuvash type; cf. CSUV. sar-, sör "filter"; nig, siz "clean". Szumér "sur, Sur" = "to press, to tumble, to penetrate, to mix wine" (kísajtól, szőlőt tapos, áthatol, át- nyom, bort kever). 19. tar (kahl, ruined), tarol (Kahl machen, verwüsten, zerstören): Old Turkic loanword of Chuvash character; cf. New. taz "Bald". Cf. also Kalmyk (Mong.) tar , bare, bald". It cannot be detected in Chuvash.

Szumér "tar" = "to cut off, destroy, devastate, pluck off, verwüstet, zerstört" (cuts down, destroys, destroys, devastated, desolate). 20. knee (Knie): probably a derivative of the word sármazessz6. Its root word is the Old Turkic loanword "tér- of Chuvash character, which cannot be independently determined; Cf. csuv, 635 ===== PAGE 626 ===== dar) new. tíz "knee", Cf. also Mong. fürei "bootshoe". Sumerian "dir" = "Spalt, Gelenk" (gap, joint). 21. tür (erdulden, geduldig sein): Old Turkic loanword of Chuvash character; cf. tsuv, füs- (Tatar loanword); Tat. tüz "endure", tolerate, suffer". Cf. also Mong. türe- (from hunger) ", suffer Sumerian "tar" = "to be humble, to submit" something).

So these 21 "Chuvash-like" Hungarian words (apart from the only questionable correspondence "iró" - "drop of sweat") have very close and clear parallels in Sumerian, and in each case in the same form as the Hungarian phonemes with the "Chuvash criteria". That is, not in the so-called Turkic languages, but in Sumerian, from where they were transferred to Hungarian. But apart from that, we According to these, "Chuvash" sound criteria are indeed ancient — only would like to make a few more comments. First: out of the 21 "Chuvash-like" words listed, 8 are not in Chuvash, one has no equivalent in the Common Turkic languages (syrla "sir- ló"), and one is a Common Turkic (Tatar) loanword in Chuvash (tüs-, eltýr). That is, in this representative cross-section of words we call "Chuvash-like" loanwords, 38% of the words are missing from Chuvash (8 words), and 10% of them (2 words) are loanwords in Chuvash. We can then rightly ask: under such circumstances, why are these Hungarian words "Chuvash-like" and what proves that they are loanwords? For example, based on the above, the word "sir-ló" is undoubtedly a Hungarian loanword in Chuvash and not vice versa. In Hungarian, this statistical comparison also speaks very strongly in favor of the fact that — as historical data have already shown (1. 614-618]. pp.) — the present-day form of the Chuvash language developed its characteristic sound changes under the massive influence of the Julian-like Eastern Hungarian masses, who merged with the Volga Bulgarians, which distinguish it from all other Turkic languages. In other words, the phonetic features that our linguistics call "Chuvash criteria", which differ from common Turkic, should by all indications be called "Hungarian criteria" in Chuvash.

Secondly: at the same time, among the above 21 "Chuvash-like" Hungarian words, 12, i.e. 57%, have impeccable Mongolian parallels, and yet in all cases the core of the word correspondences that are identical to Hungarian words — and different from Common Turkic — are precisely the words with "Chuvash-like" sound forms. This is not saying anything new, because it has long been well known that the Hungarian-Turkish-Mongolian the "Chuvash criterion" (1. e.g. Róna-Tas A.: id. m. Magy. ýstört. tanulm. p. 271). These "Chuvash" Mongolian phonetic similarities are explained by modern research — as we saw above (p. 611) — by the fact that the words in question are Bulgarian-Turkic loanwords in Mongolian. In the , Bulgarian-tő-előzők, however, we have shown that the 636

===== PAGE 627 ===== In the case of the "rók" we are also talking about peoples among whom we must in all probability count a considerable number of Hungarian-speaking Lake Magyars (1. pp. 620-630). We explain all these facts — the 20 unambiguous, also "Chuvash-like" Sumerian parallels of the 21 "Chuvash-like" Hungarian words (more than 95%); the 12, also "Chuvash-like" Mongolian equivalents (57%); and finally the 8 (altogether nearly 48%) — by saying that the so-called "Chuvash criteria" are the absence of a word "Chuvash-like" in Chuvash itself, and the obviously non-Chuvash origin of 2 other words peculiarities of the Proto-Hungarian language, and were transferred from it to the Altaic language of the Bulgar-Turkic heritage. into the languages of its groups, Mongolian, but also into Chuvash itself, the latter language becoming "Chuvash-like" precisely because a large, compact group of Hungarians who remained in the east merged into the Chuvash - or their predecessors, the Volga Bulgarians. Going further, however, it also seems obvious that these words entered the Proto-Hungarian language from a dialect of the ancient Proto-Asian language Koine, and — as evidenced by the 95% perfect Sumerian correspondence — probably from the Sumerian language itself. We can therefore conclude that these data again largely support the theory of Károly Fiók, Géza Nagy and their colleagues about the "Ugric-Hungarian" proto-language group, which, due to its central geographical location (Eastern Europe-Western Asia), had strong influences on the Altaic languages consolidating in its eastern neighborhood. Let us then examine our so-called common Turkic loanwords, in which no proto-linguistic connections of word parallelism in Sumerian can "Chuvash criteria", and let's look at some of these Hungarian-Turkish be demonstrated. At the beginning of the entries, we again state the derivations of the TESZ. 1. arat (ermen): of disputed origin; a) an ancient heritage of Finno-Ugric, possibly

from the Uralic period: a variant of irt separated by hyphenation; b) Old Turkic ere-, to cut, to detý; cf. osm. ora- Sumerian cut with a sickle", etc. The first explanation is more likely. »ur, = "ernten" (harvest); "ur-, uru-, " = "auslöschen, austilgen, wegschleifen, wegfegen, wegschleppen" (to erase, to destroy, to destroy, to sweep away, to carry away); cf. irt, regional dialect orot. Note: so either "Finno-Ugric", or in the final case, both words we take "Turkish" origin, come from Sumerian. 2. axe (Beil, Streitaxt): of Turkish origin; cf. osm. axe Sumerian 637 "axe" etc. It is found in all branches of the Turkic bal" = "Beil, schlagen, spalten, trennen" (axe, hit, split, separate); cf. balta, pall (hit, beat; schlagen), pallós, fejt (sewing), fejt (following), ax, válke, szőle, But cf.

===== PAGE 628 ===== also Akkadian pilakku (Beil); Hungarian balaska; Ancient Greek (Homeric) pelekys (Schlachtbeil, Streitaxt; battle axe); Latin falx (sickle, knife, pickaxe); German Beil (axe), ballern (to strike repeatedly), spalten (to split; — the initial s" sound of the word á means "s-shot"), falzen (today's meaning "nuten; groove", but in Old High German it was "schlagen; hit, beat"), wählen (to choose). Note: it is clearly visible in both Hungarian and German the semantic — and not temporal! — b" > „p" > 1" ýý" hangát-menct. According to the TESZ, however, the word balta is a Turkish loanword, the Slovak balaska is a Slovak loanword, the verb pall is of onomatopoeic origin, the word pallós is a wandering word (its ultimate source is unclear, but it is "not impossible" that it spread from Hungarian), the válik, vált is an ancient heritage from the Finno-Ugric period, the fett is a derivative of the verb feslik, which is of Ugric or Finno-Ugric origin, the axe is of uncertain origin, perhaps the Derivative of the verb "fett". With this latter explanation, compare the other meanings of the polysemantic "left" foundation stone Sumerian verb: "ausgiessen, entleeren, graben" (to pour out, to empty, ás); cf. Hungarian fej (melken), fejt (i.e. "milks" the milk, as wine is churned 15 "they take the smaller one out of the larger barrel", followed by a head, an axe, a hollow. 3. cattle (Vieh, Rind): of Old Turkic origin; cf. Csag. barim "wealth" etc. Sumerian "buru-" = "Vieh, Kleinvieh" (domestic animal, small cattle); cf. also borjú, bárány, birka, bürge. Note: of these words, according to the TESZ, borjú is an Old Turkic loanword of Chuvash character, bárány comes from an unspecified Slavic language, and birka and bürge are Czech loanwords.

4. stamp; arch. billog (Brandmarke, Einbrennstempel, Kennzeichen): of Old Turkic origin; cf. Türk bilig , "knowledge", New Turkish bilik , "sign", etc. The original meaning of the stamp was "a mark of ownership burned on the body of an animal". Sumerian "bil" = "brennen, Brand, Feuer, heiss" (burn, burn, fire, hot); "lah" = "drücken, aufdrücken, schlagen" (imprint, ränprint, hit). In composition therefore: "bil" + "lah" = "Brandaufdruck" (fire ränprint).

5. ber (Lohn, Miete): of uncertain origin. Perhaps an Old Turkish loanword. The corresponding Turkish noun cannot be identified, only its verbal antecedent; vý, Türk ber-; Csuu. par- , ad" etc. Szumér "bar" z - sharecropping, rental sprcis" (félbér, bér). 6. pickaxe (Pickhacke, Streitaxt): of disputed origin (Turkish or Slavic), but the Turkish origin is more likely, since the Slavic parallels themselves come from the Turks; cf. uig. "batsatabrý" etc. Cagan, Sumerian "sag," = "schlagen, spalten" (hit, split); to break (e.g. to cut off a head), axe; cf. also Scythian sagaris, "battleaxe". 638 ===== PAGE 629 ===== 10. 11,

12.

Note: according to the TESZ, the root word of szakít, szak (Abschnitt, Teil), is probably an ancient legacy from the Ugric, possibly Finno-Ugric era, and szekerce is a South Slavic loanword.

whip (the penis of larger animals; a whip is made from it): (ýýýý
origin; cf. osm. cök
"penis" etc.,

Sumerian „sig,” = "schaffen, erzeugen, zeugen" (create, form, beget).

csökik, csökken (to lose weight, to fall behind in growth, to become)

vermindern): the basis of szócslád, the root(s) is of Turkish origin; v. uig.

Cök- "descends, falls, sinks, collapses", etc.

Sumerian "sig" = "vertrocknet, dürr, schwach, zart, klein werden, wenig werden, sich vergeringern" (dried up, dried up, weak, thin, emaciated, becomes smaller, becomes less, shrinks); cf. still small, shallow, strip, sik, sikkad, síkár, stung,

szüköl, dara (Griess, Schrott): of Old Turkic origin; cf. Osm. dari köles" etc. Sumerian "dar, tar" = "spalten, zersprengen, zerstückeln" (split, split, cut into pieces); cf. ög tör, tördel. In compound

form: "dar-a" = "ein Stück" (one piece); cf. darab, darabol (these are Slavic loanwords).

touch (berühren, ankommen, reichen, langen): of disputed origin; a) Old Turkic

loanword; cf. Osm. er- reaches" etc.; b) ancient heritage from the Finno-Ugric period; cf.

Vog. sarit-, saritaxt , touches, touches".

Sumerian „ár," = "nahe, sich nähern, antasten, herbeikommen" (near, approaching, touching, coming here); cf. reaches, arrives, touches, comes here; but cf. also the Sumerian , ir," with its full semantic meaning (1. pp. 577-578), as

well as the meanings of the ancient Greek verb , air" (haire) (1. p. 578).

slander (schmähen, schimpfen): of disputed origin; a) a derivative of the noun "gyala,

slander", borrowed from the Old Turkic, but not proven; b) the

The basic word is pawn, Derived from Finno-Ugric tjal meaning "foot". One of

the explanation is not convincing either.

Sumerian "gul" = "to destroy, to make bad" (to destroy, to spoil, to do badly) , hul" = "to hate, to destroy, to despise" (to destroy, to destroy, to destroy); cf. also hate.

gyalom (Zugnetz): of Old Turkic origin; cf. Turkic. yilim, fishing net"

etc. The Turkish word is probably a derivative of the verb yil-, "to bind".

Sumerian "gil" = "wickeln, verwickeln, Riegel" (knot, twist, tangle, Összebogoz, Összekuszál, latch); cf. also net (TESZ; of Uralic origin), latch, handle (TESZ: derivatives of a basic verb meaning twisting, rotating movement), key (TESZ; Slavic loanword).

639

===== PAGE 630 =====

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wool (Wolle): of Old Turkic origin; cf. Ottoman yapagi, "sheep's wool", etc.

This is probably a derivative of the Turkish verb yap-, "to cover, to wrap".

Sumerian "gub, gib" = "wickeln, schlingen" (to wrap, twist, loop); cf. also cocoon, cocoon, tangle (cf. "woolly hair", tangled hair").

produces (verfertigen, machen, schaffen): of Old Turkish origin; cf. osm.

zékaka. "create" etc. The Turkish word is yara- "fits, corresponds" verb száрма-yarat ,

Sumerian "gar" = "to do, to make, to create, to complete" (to make, to create, to complete).

mourning (Trauer): of Old Turkic origin; cf. Kasgári Mahm. yas , harm, halal", etc.

Sumerian, "gas" = "to damage, to crush, to trample, to kill" (to damage, to crush, to trample, to beat to death, to kill); cf. "még gazol".

reed mat (Rohrkolben, Rohrmatte, Binsenkorb): of Old Turkish origin; cf.

tat. yikán , bulrush". The original meaning of the word is "Rohrkolben" (nádszár).

Sumerian , gi" = "Rohr" (reed); "kim," = "festfügen"(to join tightly); "gam" = "wickeln, schlingen" (to wrap, twist, loop). In composition, therefore: "gi" + "kim," = "festge-fügte Rohr" (tightly joined reed) or , gi" + "gam" = "Rohrgeflecht" (reed braid).

Note: these correspondences are not considered to be absolute, as the compounds cannot be given in Sumerian.

reins (Strick, Seil, Band, Zügel): of Old Turkic origin; cf. Turkic. yüplük

"yarn, thread" etc. The original meaning of the word may have been "rope, bond".

Szumér , gub, gib" = "to wrap, to loop" (teker, kötöz, hurkol);

= "führen, leiten" (to lead, to direct).

candle (Kerze): of Old Turkic origin; cf. alt. yarta "used for fishing"

torch" etc. Its root word is Turkish yar-, yaru-, shines",

Sumerian "gir," = "leuchten, glänzen, blitzen, Blitz" (shines, sparkles, flashes, lightning); cf. also with the Semitic and Indo-Germanic linguistic equivalents (1. p. 610).

stomach (Magen): of Old Turkic origin; cf. Ottoman yumur "one of the ruminants"

stomach" etc. Its root word is the Turkish yum- "to squeeze".

Sumerian, gum, kum" = "zerstampfen, zermahlen" (to trample, to crush, to crush); for the semantic connection cf. zuza "wing stomach",

knead (kneten, durcheinanderdrücken): of Old Turkish origin; cf. Osm.

yoður-, „gyúr".

Sumerian "gur" = "umwenden, drehen, umstürzen, biegen, beugen" (turn, roll, twist, bend, bend); cf. also gu-rit, roll, curve, curve, knead.

ame omeriya

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21. gather, gather (sich versammeln, sich ansammeln, sammeln, ansam meln): probably of Old Turkic origin 526058146; cf. osm. yil "gather"; osm. yigil , gathers", etc.

Sumerian "gul" = "to be full, to be rich, to be sufficiently abundant" (to be saturated, to be rich, to be abundantly available). In composition: "gul-ha" = 22. gyúr (zerknittern, zerknüllen): of "Totality" (6sszesség).

uncertain origin. Perhaps a high-pitched version of the verb gyúr.

Sumerian "gur" = l. above the word gyúr.

23. thimble (Fingerhut): of Old Turkic origin; cf. Osm. yüksük , thimble" etc. Its root word is the Turkish yük , seam, seam" (from the word tyü-, to sew). Sumerian , g15" (iz, is) = „Baum, Zweig, Hand, Glied" (tree, branch, branching,

hand, limb); cf. also knot, stem, hand, duck, joint (joint), as well as Turkish yüz (joint, limb, finger joint), yüzük (ring).

Note: these proto-linguistic correspondences indicate that the

TESZ confuses the basic words for thimble and ring. The reason for this is

It seems that the secondarily developed, pidgin-like Turkic languages — unlike the proto-

language — use the Sumerian yüz +/- "gis, taste" =

"iz, member") they formed their word meaning "ring" (yüzük , finger

"true"), while Hungarian has preserved the original proto-language semantics and

hangalaki összefüggést; vö. szumér "gur" (to turn, to seal), "gur-, kur", (to curve, to bend, curvature, circle), "har, hur" (curvature,

Ring); Hungarian ring.

24. sand (Sand): of Turkish origin, from the earliest period of Turkish-Hungarian contacts; cf. Alt. gumag, sand, etc. The root word gum is found in most Turkic languages; cf. New Hungarian gum, sand, etc. It may have been transferred

to Hungarian in its Uralic homeland. The most convincing explanation for the correspondence of the initial Turkish "q" to the Hungarian "h" is that the initial "K" of our word is of Finno-Ugric origin, Sumerian, hum, humu, kum" = "to

crush, to crush, to dust". In composition: "ukum" = Together with k" it participated in the Hungarian "K">„X">„h" change.

"Staubwolke" (dust cloud).

Note: according to this, the initial "h" sound of the word "sand" did not have to change much, because the initial "h" (hard "h") can already be detected in Sumerian: "hum" — in a typical way about the tones of the voice --

presumably at the parallel to the sound form "kum" (gum), with the same meaning. So here too, it is not a question of a "regular" sound development occurring over time, but merely

dialect level.

25. loop (Schlinge): of Turkish origin; cf. alt. urug , a stick or strap with a loop for holding 416",

641

===== PAGE 632 =====

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642

Sumerian "har, hur" = „Krumm ng, Schlinge" (curve, loop); also hook. In combination: „har-muSenna" = "Vogelschlinge" (today darfo_6 loop)

kantar (fence, carrying net, usually made of straps or twine)

angefertigt wird): probably of Turkish origin; cf. csag. gantar „fö-
"rider", etc.

Sumerian "gan, kan" = twine, . . . a tubular device for inserting pointed-bottomed jars, frame, band, tie, belt, rope" (threads for inserting pointed-bottomed jars, frame, rope,
bond, rope); cf. also with other meanings of "gan" (1. above, 579-581.
old.).

gate (Tor): of Old Turkic origin; cf. ýý. gapuy "gate" etc. Its root word is the verb torok gap- , to close. Sumerian sahab" = "Riegel" (latch); cf. . . . "hab" = "verriegeln" (to close, to lock); cf. also "heften". In combination: "heften" (to fasten) and "Haft"
clasp. Note: but cf. also German Or the Latin "capio" (catch, seize), and from it the derived "captus, . . . (to bind, to close). The roots of these German words are probably also of Old Turkic origin.
captive" (captive, , captured"). We can see that this Hürde has come a long way): of disputed origin; a) Slovak loanword; cf. Szik. kram, kram , occasional dwelling, accommodation of . . . "gap- verb of Old Turkish origin! corral (hut,
woodcutters, charcoal burners; satócsbolt, lim-lom"; this is an adoption of the German Kram , kacat, stuff, (corner) shop; b) of Turkish origin; cf. baraba-tat. goram , yard; yard in which animals are kept". The verb root word of the Tatar noun is represented
in many Turkic languages; cf. Kashgari Mahm. gori , surround, guard" etc.

Szumér „gar, kar", = „enclose around, enclose, border, border.
Einschliessung" (enclosure, surround, frame, fence), boiler (Kessel): of Ottoman-Turkish origin; cf. Ottoman kazan , üst", Etymologically identical to the Turkish antecedent of bell, which entered our language much earlier.

Sumerian "gaz" = "zerstampfen, zerstoßen, Mörser" (trample down,
crush, mortar); cf. still wading.
It should also be noted that the first occurrence of the word "kazán" in the linguistic record (in 1632) in the text in question refers to saltpeter and a boiler, but saltpeter was usually crushed in a mortar when making gunpowder, not cooked in a cauldron.
On the other hand — also interestingly enough, the German word "Kessel" (boiler) also has a very close phonetic parallel with the Sumerian word "kazán" (mortar) and the word "age" (Zeit, Zenalter, Zenabschnitt, Lebensalter); of . . . accidentally"
Old Turkic origin; cf. Osm kur "person, thing of the same age; class, degree, measure".

===== PAGE 633 =====

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Sumerian "gur, kur," = "Mass, Getreidemass, abschneiden, Abschnitt" (measure, grain measure, cut off, section); see also jug (unit of measure); cf. also Sumerian "gur,." = "Tonne, Getreidemagazin, ein Hohlmass" (barrel, grain mass, a type (Irish
measure) and Magyar barrel (also volume measure), göré.
Note: we do not consider this agreement to be perfect for the time being,
since the . . . semantic relationship between "space measure" and "time measure"
it still needs to be proven from another perspective.
oroz (stehlen), orv (Dieb): probably of Old Turkic origin; cf. Kyrgyz. ürü;
Uzbek: Ugri, thief; equivalents in all branches of the Turkic language.
Sumerian „ur,." = "to take away, to carry away, to drag away" (take away, carry away, drag away); in compound: "tug-ur,." = "stehlen" (steal).
eternal (dauernd, ewig): probably of Old Turkic origin; cf. New Turkic. ürük
long-lasting, permanent".
Sumerian "uru,." = "everlasting, eternal" (eternal).
ör, ýrol (mahlen): the base of the word family, the derivative of the verb ýrol. Its base word, the
verb stem Or-, is of Old Turkic origin; cf. ýý. ävir- , to grind". The same
The Hungarian derivative of the verb stem is our word örven, as well as presumably the verb öriz, and as a Turkish derivative of the verb stem, örül is also related to this.
Sumerian ur,." = "to grind, to make fine flour" (to grind, to make fine flour).
ýý, Öriz (Wächter, wachen, hüten): of Old Turkic origin; l. the "guard" heading
under.
Szumer "uru,." = "to guard, protect, watch over, watch over" (véd, ýý,
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to go crazy (wahnsinnig werden, verwirrt werden): of Old Turkish origin; 1. the
Under "Ördü".
Sumerian "ur,." = "torn" (torn, torn, split). In composition: "al-ur,-ra" = "Verwirrung" (confusion, confusion).
whirlpool (Wirbel, Strudel): of Old Turkish origin; 1. under the heading "the mill".
Sumerian "uru,, uru,." = "Flut, Sturmflut" (flood, storm flood, flood tide); cf. still flowing, flood.

szór (to winnow, to sprinkle, to pour out, to smash, to hurl,
zerstreuen): of Old Turkic origin; cf. Osm. savur- , szór, hint, dob" etc.
Sumerian sur" = "to pour out, to scatter, to scatter."
stubble (Ackerfeld, Getreidestoppel, Stoppefeld): of Old Turkish origin; cf.
antecedent of . . . "field" etc. Its root word is probably identical to the root word of the Turkish
ma-osm, tarla gyar dara (dari etc., miller"). 643
===== PAGE 634 =====

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Sumerian . . . tar" = "to cut wood or grass, to break, to destroy, to be devastated" (to cut wood or plants, to break, to destroy, to be desolate); cf.
distort, distort, strain, break, store, store.
Note: it seems very likely that our words tarol and tar -
The word "taz" is derived from another Turkish word (taz, "bald")
— they belong organically here, and neither does stubble mean "fieldland" in Turkish
tarla etc., as TESZ claims, but each of them
The Sumerian "tar" is a derivative of the root word. The semantic composition of these words is
dependence is indisputable in itself: in Hungarian, tuló does not denote the stubble of grain stalks,
unlike in Turkish. However, the Sumerian translations raise . . . "arable land in general", but rather the state of the arable land after harvest, which is strewn, distorted, and tarred, or
this already obvious semantic connection to the level of . . . tar" just presented the
certainty, and even directly admit that it was the Turkic languages that were once the recipients:
they knew nothing about the deeper semantic connections of the word tuló, and only understood it to mean some kind of "field".

It is another question whether stubble can ultimately be truly connected with the meaning of "arable
land" in general, and indeed also with the Sumerian root word "tar".
through its semantics: one of the other meanings of this word is
"graben, die Erde aufwühlen", i.e. "ür, digs up the earth". This meaning
however, it dates back to the earliest stages of Neolithic times
us when they were still loosening the soil with pointed sticks, so they were really digging it.
law (Gesetz, Gerichtsverfahren): of uncertain origin. Some kind of

may be related in a similar way to the following Turkish words; in this case, it is a pre-conquest Turkish loanword in our language; cf. Turkish törü "law, government, custom"; Csag. töré, law, custom, government" etc. Sumerian tar = "to hear, decide, judge, hold accountable, punish" (to interrogate, decide, judge, hold accountable, punish). Note: i.e. the law is clearly related to the meaning of the verb tör, "to hold accountable, to punish", and this semantic connection is also in the can be traced back to the Sumerian language. Cf. also "breaking bread" (the division of heirs according to old customary law); "breaking a stick over it" (condemning). tür (zusammenfallen, zusammenrollen, einstülpen, aufkrempeIn): of Old Turkic origin; see also csag. tür "fold, roll, pile up" etc.

Sumerian: tar = "bend, turn, roll" (bend, twist, roll, roll). Of the 40 Hungarian words considered to be common Turkish loanwords, 38 are again there is an impeccable Sumerian parallel (the mat and the age are debatable 644 ===== PAGE 635 =====

(not counting the equivalents). In other words, the number of certain matches here is around 95/96, just like the 6700016 Sumerian language. Together, we examined "Chuvash people". a total of 61 Hungarian words said to be of Turkish origin, i.e. about 20% of our Turkish loanwords, and among these, in 58 cases we found very close Sumerian parallels that were recognizable at first glance. This high ratio undoubtedly indicates that these words cannot be considered simply Turkish loanwords in Hungarian, but that they entered Hungarian and Turkish, but also evidently Mongolian, from the linguistic state of the Proto-Asian language koine called Sumerian.

We would like to remind you that we found practically the same picture when, in a representative cross-section of about 10-12% of the words we considered to be of Finno-Ugric origin, we managed to show 50 impeccable Sumerian correspondences out of the 60 words examined, which corresponds to 83.3% (1. pp. 516-526). Sumerian linguistic influences appear somewhat weaker, but also dominantly, in the case of our loanwords considered to be of Proto-Iranian origin, which we examined as a whole (1. pp. 553-557), and among the 53 Hungarian-Finno-Ugric words determined by Harmatta to be of Proto-Iranian origin, we found 39 completely unambiguous Sumerian correspondences (73.6%). Moreover, the organic correspondences with polysemantic Sumerian roots in several examples taken from among the Hungarian words considered to be German and Slavic loanwords (1. pp. 574-581 and 594-598) allow us to conclude that in many cases Sumerian linguistic material is hidden behind our German and Slavic "loanwords", and that in these cases we cannot simply speak of words of German or Slavic origin, but - similarly to the others - here too we must seriously consider the possibility of direct Sumerian-Hungarian borrowings.

All these clear data make it obvious to the unbiased observer that when researching the origins and most ancient connections of the Hungarian language, we cannot dispense with a thorough examination of the Sumerian-Hungarian linguistic parallels. An areal approach to the relations between the Uralic and Altaic languages

At the same time, however, these Sumerian-Hungarian linguistic connections forcefully testify that the "Ugric-Hungarian" Osnyelvi theory of Károly Fiók, Géza Nagy and their colleagues can be supported with great force not only archaeologically, but also from the linguistic side, and the outlines of the paleolinguistic development of Eastern Europe and Western Asia that can be outlined today point more and more definitely in the direction that these wonderfully far-sighted researchers of ours clearly recognized 80-90 years ago: the linguistic facts known so far, as well as the newer archaeological finds and historical connections, truly prove that in Eastern Europe,

645 ===== PAGE 636 =====

In Western Asia, we must reckon with their "Ugric-Hungarian" proto-linguistic community at a very high level of vernacular, which played a dominant role in the language family-like equalization processes of the Uralic-Finno-Ugric languages in the north and the Altaic-Torric languages in the east, but at the same time it also played a very strong role in the consolidation and unification of the Eastern Indo-Germanic language groups, primarily the Indo-Iranian and Balto-Slavic languages. All quantifiable data indicate that this was the so-called "A-complex", which modern Indo-Germanic studies have established was close to the Uralic-Altaic languages and undoubtedly formed the proto-Indo-Germanic language alliance of several — at least two — linguistic components in the 1st millennium BC. It formed one of the most important components of its consolidation in the 2nd millennium (1. pp. 550-552).

Here we must once again recall another aspect of the question of our loanwords called Turkish, which was discussed in the article "Linguistic Statistical Considerations" We have already touched on this in our sub-chapter. We pointed out that there is a striking contradiction in the numerical distribution of the so-called ancient Altaic word layers of the Ural-Finno-Ugric languages and Hungarian: while in other Finno-Ugric languages the ancient Altaic word layers range between 43 and 31, our linguistics counts only 24 such ancient word layers in Hungarian, although all researchers with a Finno-Ugric perspective agree that the ancestors of the Hungarians once lived in the southernmost strip of the Finno-Ugric settlement area, so we should expect the strongest ancient Altaic linguistic influences from them, because they were closest to the ancient dwellings of the Altaics. We also pointed out that these missing ancient Altaic word layers are in all likelihood so-called

We have to look for them among our "Old Turkic" loanwords, where they must have been transferred during the "Ugric-Turkish War", because Budenzék, In their argument against Vámbéry, the Hunfalvy had a vital interest in referring as many Hungarian-Turkic word cognates as possible to later periods and declaring them to be ordinary Turkic loanwords (1. pp. 499-500). We would now like to substantiate our well-founded suspicion and, if possible, support it from several sides.

Let us begin by saying that Budenz himself originally defined 122 Hungarian-Turkish word parallels as ancient Ural-Altaic correspondences at the time — even before the "Ugric-Turkish War" became more intense. Of these, only 24 remain today, and a good 4/5 of them have migrated to later Turkic loanwords, or — regardless of the impeccable Altaic linguistic correspondences — have become "of Finno-Ugric origin." It is difficult to believe such a thing, especially when our linguists record almost twice as many ancient Turkic correspondences as the northern Finno-Ugric ones. Moreover, this statement becomes downright unreasonable if we consider that this late, mostly AD, Among the Turkish-Hungarian word parallels attributed to loanwords from the second half of the 1st millennium, there is also a more unformed, primary root verb,

646 ===== PAGE 637 =====

whose late borrowing is only not absurd for those who, like Géza Bárczi, imagine the Hungarians of the period immediately preceding the conquest of the country as Paleolithic savages at the cerebral level of the Neanderthals, and assume from the outset that they did not have words to express even the most elementary actions, Let's look at some such examples from Vámbéry's book "The Origin of the Hungarians" (1st ed., p. 265): magyar 1year Hungarian Turkish ás ball, ball as, es ful áz(ik) jaur, juur cap ergyür az, ez gyül(ik) tap (szopik) havj (yüw mál) em(ik) koj (put down) (remains pure) ~~unvaluable~~, sír fill(me) thanks to)

The verbs listed are divided as follows today (TESZ): ér, gyür, gyülík, ír: Old Turkic loanwords; tsap, emik, ful, hgy: Finno-Ugric or Uralic in origin, but possible Altaic their relationships require further investigation"; at this comment However, the TESZ — carefully, with expediency — omits clear Turkic correspondences, as shown in the table above, because these are much closer in both sound and meaning to questionable Hungarian words, as compared to the parallels in the dictionary Finno-Ugric words (e.g. tsapkini = "drum"; ful - divide. pixnamia = "turn sour" etc.); ás, ázik, folyík, hál, jut: according to TESZ, these words are partly of uncertain origin (ás, folyík), partly ancient legacies from the Finno-Ugric (ázik, hál) or Ugric (jut) era (cf. e.g. ázik - vog. tat'- = from a point of view. their Turkish derivation is wrong" — but again only "forgetting" the obvious and much closer Turkish parallels will be wet"); emphasizing in particular that,

Such deceptions are an integral part of the basic methods of Finno-Ugric linguistics — or more precisely, its everyday practice. The words we claim to be of "Ugric, Finno-Ugric or Uralic origin" have a series of impeccable Turkish linguistic equivalents, which - similarly to the examples above - are in many cases much more obvious than the Finno-Ugric parallels collected with the help of phonetic reconstructions. The aforementioned compilation by Vámbéry, who is the author of Budenz Ugric-647, provides an overview of the number of these mutual Hungarian-Finno-Ugric-Turkish word correspondences. ===== PAGE 638 =====

The Hungarian Comparative Dictionary compared 663 words out of 996 words with the Turkish languages (1. Vámbéry: id. m. pp. 241-253 and 502-648 or pp. 482-483 of this book). For 217 words, Budenz found Turkic parallels that were completely equivalent to his Finno-Ugric correspondences, for 208 cases Turkic correspondences that were much more obvious than the Finno-Ugric correspondences, and only 210 words showed closer, unambiguous Finno-Ugric parallels, while the etymology of 28 words was obviously completely wrong. In the end, of the 635 Hungarian words considered to be of Finno-Ugric origin by Budenz examined by Vámbéry — not counting the 28 obviously incorrect etymologies — only 210 (33.1%) can be considered to have clearly had close linguistic relations with both Finno-Ugric and Turkic languages, while 208 "restricted" evidence of Hungarian-Finno-Ugric linguistic affinity, 217 words (34.2/90) testify that the Hungarians were once the words (32.7%) are a direct sign that our language is somehow, independently of Finno-Ugric, also particularly connected to Turkic languages.

One could suggest — as Finno-Ugric studies have done — that Vámbéry "embellished" his results and examined only 663 words from Budenz's 996 collations because he could not doubt the Finno-Ugric nature of the other 333 words. In this case, the statistics would change as follows (not counting the 28 incorrect etymologies here): Of the 968 words, Finno-Ugric 210 + 333 = 543 (56/99); Turkish 208 (21.5%); common 217 (22.5%). That is, even in this case, the Turkish layer would remain very respectable.

However, this is not the case! Modern international Finno-Ugric studies today only consider 450-500 Hungarian-Finno-Ugric word correspondences acceptable (e.g. B. Collinder's great Finno-Ugric etymological dictionary), which clearly proves Vámbéry right and proves that Vámbéry did not "purposefully" select the 663 words in question from Budenz's 996 correspondences, but that the word statistical ratios he established are indeed generally valid for all 996, or — again counting the 28 incorrect correspondences — 968 Budenz-like words. Let's see why.

Vámbéry's 33.1% clearly Finno-Ugric parallels correspond to 320 Finno-Ugric cognates in relation to 968 words, and the 34.2% bidirectional (Döörök and Finno-Ugric) correspondence corresponds to about 330 words, and if -- as is obvious -- we consider half of these to be Finno-Ugric and half to be Turkic, we get a further 165 Finno-Ugric correspondences, which means a total of 485 solid Hungarian-Finno-Ugric correspondences, i.e. almost exactly as many as Collinder's dictionary records. Moreover, the 32.7% clearly Turkic parallels correspond to 316 words according to the above conversion, which, however, strikingly matches the number defined by our linguistics as "over 300".

648
===== PAGE 639 =====

with Turkish loanwords. From this it can be clearly established that Hungarian linguists count only these indisputably Turkish correspondences among the Turkish "loanwords", while the approximately 330 mutual Hungarian Turko-Finno-Ugric correspondences are almost without exception classified as words of "Finno-Ugric origin" (cf. the Finno-Ugric derivation of the primary basic verbs presented above). Thus, as a starting point, approximately 650 words of nyS1 heritage come together",

Now it is slowly becoming clearer to us how Hungarian Finno-Ugric studies came to the assumption of its own "about 1000" Hungarian words of Finno-Ugric origin. The following The next step is obviously the objection mentioned above that Vámbéry "combed" Budenz's results, so the 333 collocations he omitted are "certainly" all Finno-Ugric. By applying this "method", our "words of Finno-Ugric origin" can be nicely expanded to about 760 (217 words of Vámbéry's category I -- 210 words of group 2 + the 333

" , probably" Finno-Ugric words, which Vámbéry "refrained from examining"). After that, it was only a matter of referring to newer, more sophisticated methods of linguistics, no and a little generosity is needed to generously round up these 760 words to "approx. 1000". — For example, by declaring words that are obviously onomatopoeic and onomatopoeic in nature to be "Ancient Heritage" (the cognateness of which words is precisely denied by phonological linguistics), or by ignoring much more obvious Turkish parallels, or by commenting that "... the question of the possible sub-linguistic connections of the Finno-Ugric word requires further investigation" (1. above for basic verbs).

Returning to the specific lessons of Vámbéry's word statistics, to the surprisingly constant ratios shown by the word parallels of our language only Finno-Ugric, only Turkish, and Turkish and Finno-Ugric (33.1%, 32.7%, and 34.2%, respectively), we can state that we have once again reached Géza Nagy's "Ugric-Hungarian" proto-language group: all these linguistic facts can most obviously be explained by the fact that we must reckon with the former existence of the ancient linguistic community they postulate as a real reality, and that this language group in all probability really occupied an intermediate position between the Finno-Ugric and Turkish languages (both linguistically and geographically).

In this regard, we should also briefly recall that orthodox Finno-Ugric linguistics is not only a study of the massive differences between the Uralic and Altaic languages. not only does it consistently ignore word correspondences, but it also systematically ignores the grammatical and structural correspondences that can also be detected sequentially, even though these connections are distributed in roughly the same proportion between Hungarian, Finno-Ugric and Turkish languages as the dictionary correspondences described above. A detailed discussion of these issues would lead too far, so we will now

649
===== PAGE 640 =====

We only refer to the work of Sandor Cs "ke, who demonstrated a large number of Uralic-Altaic grammatical and linguistic structural correspondences (1. e.g. Csdke: Sumerian-Finnish-Mongolian-Turkish comparative grammar).

In conclusion, in the light of the historical, archaeological and linguistic data known so far, we can consider the theory most likely that approximately 330 words from the language of the "Ugric-Hungarian" proto-language group of Géza Nagy and his family migrated to both the Finno-Ugric and Turkic languages, and in addition, the Finno-Ugric and Altaic-Turkic languages separately adopted around 320 words from this ancient language. In other words, the number of words of "Ugric-Hungarian" origin in the languages of both language families is apparently at least 650, and at the same time, we also find numerous "Ugric-Hungarian" grammatical elements and language structural features in each of them.

The obvious conclusion is that neither the Ural-Finno-Ugric nor the Altaic-Turkic languages are some kind of ancestral linguistic and ethnic community, like a family tree. branches, i.e. there is no real genetic language relationship between them, but they developed in later times, sometime around the 2nd-1st millennium BC, through areal language equalization, in the manner of language associations. All signs indicate that this language equalization process was catalyzed in the case of both "language families" by some southern, ancient language community - which, however, must have been very close to Proto-Hungarian in any case -, and we must view both the common Hungarian-Turkish-Finno-Ugric and the separately occurring Hungarian-Finno-Ugric and Hungarian-Turkish lexical, grammatical and structural parallels as the dominant effect of this proto-language group.

It is not yet possible to establish with sufficient accuracy which archaeological culture (or cultures) this "Ugric-Hungarian" proto-linguistic group can be more closely associated with. All that seems certain is that — with reference to the 3rd millennium BC — it had a lot to do with the Kuban-Maykop culture of Transcaucasian origin, the South Turkmen civilizations also originating from East Asia and their Altai derivative, the Afanasyev culture; in the 2nd millennium BC, with the Early Bronze Age civilizations of the South Urals (Balanovo, Abasevo, etc.), then with the Andronovo peoples; and finally with the Scythian-Sarmatian culture of the steppe of the 1st millennium BC,

Based on our current knowledge, the historical and linguistic connections of A7 can most likely be outlined as follows. In the first stage, starting from the turn of the 3rd/2nd millennium BC, in the east the language of the Turkmen-Proto-Asian settlers of the Afanasyev culture influenced the ancestors of the later Torks and Mongols, and in the west the language of the Early Bronze Age colonists of the Ural region of Kuban-Transcaucasian origin layered over, as it were, in the manner of a "Jingua franca", the Proto-Finnic, Proto-Obian and

650
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into the roots of the groups mentioned above, In Hungarian: this would roughly be the common linguistic layer that Finno-Ugric studies considers as the "Urdu Finno-Ugric heritage", As a second stage, we must take into account the dominant role of the Andronovo civilization, which was largely of southern origin and had a unified anthropological and cultural profile, and its related ethnic groups, which was effective for several centuries and which influenced the leveling of languages, in the second and third quarters of the 2nd millennium BC; the numerous common Hungarian-Finno-Ugric-Altaic linguistic parallels can obviously be attributed to these convergent linguistic influences, extending from the Altai to the Volga. At this time, the Andronovo civilization was directly adjacent to both the Finno-Ugric and the Turkic territories, it was an outstandingly developed, leading cultural element and a decisive ethnic majority of the region in question, and as a result, it undoubtedly played a primary role in the development of the common elements of the two language families. In other words, the correspondences that appear in the linguistic public consciousness as signs of the "Ural-Altaic" ancestral relationship would largely be relics of these Andronovo times.

The third stage, the Scythian-Sarmatian cultures of the 1st millennium BC, should be viewed in terms of language in such a way that at the very beginning of this era, around the turn of the 2nd/1st millennium BC, a certain degree of separation of the steppe peoples from the more northern areas can be observed: the descendants of the developed Early Bronze Age cultures of the Ural region become insignificant, the earlier rich finds practically disappear, i.e. the population obviously moved further south. We already know the reason for this quite well. Archaeological finds clearly show that around the 2nd/1st millennium BC. At the turn of the 20th century, the equestrian nomadic economic form emerged in the Eurasian steppe zone (more precisely: it reached the stage of development during this period when it is undoubtedly reflected in archaeological finds), which economically exploited the vast expanses of grassy steppes with compacted soil that had previously been unused — because they were not yet suitable for farming with the primitive tools of the time — and made them habitable for humans.

In the river valleys with looser soil and in the plowable areas of the forest-steppe, people of southern origin, who have been engaged in agriculture and animal husbandry since the beginning of the 2nd millennium BC, The settlers - having multiplied enormously by the turn of the 2nd/1st millennium (we must count on a 6-8-fold increase in the population over 6-700 years) - sooner or later had to look for new ways of making a living if they did not want to starve. Undoubtedly, the development of the steppe horse-nomadic economic system and way of life can be attributed to them: only the descendants of these colonists had - their previous mixed agricultural-animal husbandry economy and the high level of metalworking - 651.

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as a result of their knowledge of the nalü -- their adequate economic and technical preparation (relatively large livestock, vehicles, carts, metal tools, weapons, etc.) to be able to slowly and continuously transition to the equestrian nomadic way of life. This can explain the decline in importance of the cultures of the more northern areas and the simultaneous flowering of the steppe cultures around the turn of the 2nd/1st millennium BC.

For a few centuries thereafter, there was still plenty of free space on the steppe for the population growth of the horse-riding nomads, but it seems that the space began to narrow in the middle of the 1st millennium BC: at this time, in addition to the east-west migrations (Cimmerian, Scythian and Sarmatian movements), we can again observe a northward expansion, for example in the development of the Ananino civilization, which is undoubtedly a steppe colonial culture, on the northern outskirts of the South Russian steppes, in which the Scythian-Sarmatian steppe peoples practiced organized animal husbandry and agriculture in the form of animal farms, subjugating and putting to work the Finno-Ugric indigenous population they found there (Book 1, II, pp. 324-326).

These more recent, closer contacts between the steppe peoples and the Finno-Ugric peoples must be evaluated as secondary effects from a linguistic point of view, and we can obviously attribute to these later contacts a considerable part of the similarities and parallels that we can observe separately between Hungarian and the individual Finno-Ugric language subgroups (Obi-Ugric, Permian, Volga-Finnic, Baltic-Finnic), but which are absent from the other Finno-Ugric languages.

However, if we claim that our theory forms a complete whole and explains all linguistic phenomena between Hungarian and the individual Finno-Ugric languages, as far as possible, then we must also take into account a tertiary layer of Hungarian linguistic influences, much later than the 1st millennium BC, which is primarily evident in the languages of the Ob-Ugric and, to a lesser extent, the Volga-Finnic (especially the Cheremis); in these languages we find much more Hungarian similarities and parallels than in other Finno-Ugric languages, and their degree of affinity to Hungarian, expressed in percentage, also shows significantly higher ratios than in other Finno-Ugric languages (1. p. 502 of this book).

Well, there is more than one clear historical evidence of such late contacts between the ancient Hungarians and the aforementioned Finno-Ugric groups: in the 5th-7th centuries AD, contemporary sources speak of the northern migrations of the steppe peoples of the southern Pontus-Caucasus-Kuban region to the Southern Urals and the middle Volga region. The first such data comes from Priscus, who in about 460 informs us that the Avars displaced the Sabirs, who in turn displaced the Onogurs, Saragurs, Urogs, and Akacirs from their previous nomadic territories. Since it has been proven since the 1950s that the Sabirs — and later the Avars as well

===== PAGE 643 =====

-- They invaded the Kuban-Pontus region from Transcaucasia (Altheim, Haussig; 1. 11. our book, pp. 265-267), it is obvious that the mentioned peoples fled to the north to avoid them. This is also proven by Jordanes' statement from the 6th century, from which we learn that the Hungurs (Onogurs or Sabirs) control the fur trade of the northern regions, 8 they transport the expensive northern furs to the Byzantine markets.

In the second half of the 7th century, contemporary chroniclers report new northern migrations: after the collapse of the Onogur-Bulgarian empire in the Kuban-Don region, the central With the arrival of the Volga Bulgars — accompanied by Sabirs and Hungarians — they took control of the northern fur trade. The correctness of all these source data has been clearly confirmed by recent archaeological research: in the middle Volga and Kama region, the first, so-called Harinai, group of the southern Lomatovo culture appeared at the end of the 4th century AD. This was followed by the Nevolinoi group at the turn of the 5th/6th century, which undoubtedly included the fur trader Jordanes. We must see the Hunurs or Sabirs, and finally, around the turn of the 7th/8th century, finds of the Volga Bulgars appear in the middle Volga region; among these we can also look for the legacy of the Volga Hungarians of Julianus. Since our ethnic name used abroad, Ungar, Hungarus, Ungri, Ugri, undoubtedly derives from the name Onogur (Hunugur), and this name is closely related to the name of the Sabirs (Jordanes), which ethnic name, however, is mentioned by Emperor Constantine as the old name of the Hungarians, we must rightly count on a significant number of Hungarian-speaking groups among the Hunurs, Onogur-Bulgars and Sabirs who appeared on the middle Volga in the 5th-8th centuries, and their presence clearly explains the Hungarian parallels in the languages of the Ob-Ugric and Volga-Finns, which show particularly strong connections, and which are absent from the other Finno-Ugric languages. In any case, we have reliable data from later times on the presence of Hungarians on the middle Volga: Julianusz found one of the larger groups of Hungarians who remained in the east there. Summarizing what has been said so far, it can undoubtedly be stated that we are still quite far from having the last word and making a final judgment on the issue of our so-called Turkish loanwords,

I. We do not have any real, unambiguous evidence to systematically support the Turkic origin of these words, and their Turkic derivation is in fact only an a priori assumption of Hungarian linguistics based on the exclusively Finno-Ugric origin of the Hungarians, which, however, very often leads to irresolvable contradictions even within orthodox Finno-Ugric studies: even in scientific publications with a clearly Finno-Ugric perspective, mutually exclusive theories often come to light.

653
===== PAGE 644 =====
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The phonetic forms of our words, which are called Turkic loanwords, raise questions that phonological linguistics cannot explain, and as a result is forced to formulate the most absurd hypotheses (cf. e.g. Bárczi's four or five "Chuvash languages", although we know only one). The most important of the so-called "Chuvash criteria" can be stated from contemporary sources that they only began to spread in some western Turkic languages in the 11th and 13th centuries (Benzing, Ramstedt); therefore, all indications point to the fact that these sound forms, which differ from common Turkic, were formed among the Chuvash under the influence of the language of the Volga Hungarians who remained in the east: significant masses of these Hungarians merged with the Chuvash. The same can be said about the practically unknown languages of the Bulgar-Turkic people, from which Chuvash is derived: among these mixed ethnicities, in the 11th and 13th centuries AD, in the second half of the 1st millennium, we must also count on massive Hungarian-speaking populations, not only in Eastern Europe, but also among the "Bulgarian" (i.e. mixed) ethnic groups that have been detectable in the Carpathian Basin since 475 and were repeatedly supplemented by new waves of immigrants. In addition, the close coexistence of the ancestors of the Hungarians and the Western Turks in the steppes of East Europe and West Asia can be traced back to the Scythian-Sarmatian times with the help of contemporary source data and recent anthropological studies, and is highly probable. Our so-called Turkic loanwords — both those of Chuvash character and those derived from the common Turkic languages — have a series of impeccable correspondences in Sumerian, and what is most important: in the case of each examined "Chuvash-character" Hungarian word, these proto-linguistic parallels also show "Chuvash" phonological criteria, but even in our loanwords considered to be of common Turkic origin, they are mostly closer to the phonetics and meaning of Hungarian words than to the corresponding Turkish words.

However, among the words we claim to be of Finno-Ugric origin, we find a large number of clear Turkish parallels, which, however, are consistently ignored by Finno-Ugric linguistics, even though they are often much closer to the Hungarian words in meaning and phonetic form than the Finno-Ugric equivalents. The Hungarian-Finno-Ugric-Turkish, Hungarian-Finno-Ugric and Hungarian-Turkish word correspondences show constant ratios (34.2%; 33.1%; 32.7%) which undoubtedly indicate that Hungarian is located between the Finno-Ugric and Turkish languages; this conclusion is strongly supported by the distribution of grammatical and structural correspondences and parallels between the aforementioned languages and Hungarian, which is exactly the same as the dictionary correspondences.
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In the light of recently discovered archaeological, anthropological facts, historical and other source data, and the linguistic development insights of modern Indo-Germanic studies, it is highly probable that the theory of the so-called "Ugric-Hungarian" Syntingual Group, advocated by Károly Fiók, Géza Nagy and several other renowned Hungarian researchers at the end of the 19th century — which, according to these scientists, played a very significant role in the process of language family-like unification of the Ural-Finno-Ugric and Altaic Turkic languages -- cannot be considered simply a curiosity of the history of science, but rather, it appears to have actually existed in the East European-West Asian region, at least since the beginning of the Andronovo period (c. 1700 BC).

However, all these recently discovered anthropological, archaeological and historical data and linguistic facts are consistently ignored by traditional Finno-Ugric linguistics and the Hungarian prehistory research based on it, because they ruthlessly and clearly show that the paleolinguistic and prehistory approach and methodological principles of the dominant academic school are completely flawed, both in their details and as a whole are largely outdated and in need of radical revision — for example, in the issue of our so-called Turkic loanwords.

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657
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660
===== PAGE 651 =====
BOOK IV
THE SUMERIAN QUESTION
Archaeological, ethnogenetic, linguistic and research history analysis
===== PAGE 652 =====

Chapter 1
THE DEVELOPMENT AND INTERCONNECTIONS OF NEOLITHIC, EARLY 1 1 FEMINIST CULTURES
Preliminary remarks

When in the last decades of the last century, and then in the first third of our century, more and more light was shed on the Mesopotamian Sumerian civilization of the 3rd millennium BC, we still knew relatively little about the circumstances and beginnings of the formation of this high culture, and virtually nothing was known about its antecedents — not to mention the broader contexts of the development of pre-Asian civilization in the eras preceding the 3rd millennium, beyond the narrower borders of Mesopotamia.
Only gradually, with the development of larger-scale excavations and with the help of deep excavations carried out in the area of several ancient Mesopotamian cities, did the gloom begin to dissipate. The archaic layers of Ur, al-Ubaid (el-Obed), Uruk and other rubble mounds were traced back to a thickness of 20-30 m in some places to the 5th millennium, and even to the second half of the 6th millennium in Eridu. It turned out that in the southern half of Mesopotamia, on the alluvial plain, with the so-called Eridu-Ubaid culture, the Neolithic cultural development began around 5400. Since the first high culture undoubtedly developed in Southern Mesopotamia, in view of these millennia-long antecedents, the prevailing opinion for a long time was that the beginnings of higher human culture, and the development of the agricultural way of life, should be sought in the fertile plain areas of the great Mesopotamian rivers.
However, the archaeological picture soon changed fundamentally: in northern Mesopotamia and Syria, ancient agricultural settlements were discovered one after another, the beginnings of which greatly preceded those in the south. Milestones in the understanding of the ancient civilizations of the northern regions were MvOppenheim's excavations at the Tell Halaf ruin field, followed by the discovery of the 6th millennium cultures of Samarra and Hassuna and the archaic layers of Nineveh.
The precise stratigraphic observations of the aforementioned sites have proven the obvious idea, long-stated by many, that a sufficient amount of
In the northern areas with natural precipitation, agriculture began much earlier than in the southern plains, where, due to the lack of rainfall, it was only possible to achieve significant yields and practice regular agriculture with the help of complicated irrigation systems, river regulation, water storage structures and their continuous maintenance.

However, the regulation of the Tigris and Euphrates and the canalization of southern Mesopotamia also contributed to these general, ubiquitous prerequisites for irrigated agriculture in Mesopotamia.

663
===== PAGE 653 =====
The beds of both rivers are above the level of the plain due to the continuous deposition of alluvium, and thus the risk of flooding is extremely high. In addition, the Euphrates flows 9 m higher than the Tigris at Hit (on the northern edge of the plain).
In the construction of the canal network, therefore, special attention had to be paid to this difference in level, and intermediate locks had to be used. In addition, the annual floods only occurred between the end of April and the beginning of June, after the harvest, which began in Mesopotamia in April, meaning that their water could not be used directly for irrigation of the fields, but huge artificial water storage basins had to be built. In addition, the canals silted up extremely quickly, they had to be deepened every year with a great investment of work, and the dredged sediment soon towered like a wall on the banks, so that after a short time new canals had to be dug parallel to the old ones (for all this, see Lloyd 1981, pp. 16-18).

It is therefore clear that the significant population of the southern Mesopotamian plain could only have occurred when the technical and social development of the primary food-producing cultures further north reached the point at which the enormous water-regulation works outlined above became technically and organizationally feasible.
The next milestone in the study of the ancient civilizations of the Middle East was RJ Braidwood's so-called "Iraq-Jarmo" project, starting in the late 1940s.
Braidwood's carefully thought-out and carefully prepared research program aimed to uncover and study the roots of pre-Asian Neolithic cultures, the beginnings of agriculture and animal husbandry, and he largely achieved his goal. More importantly, however, his work greatly stimulated interest in the study of the Mesolithic and Early Neolithic in the Near East, and now we have a fairly clear picture of the development and spread of the food-producing economic form — not only in Pre-Asian but also in the adjacent areas.

Braidwood's basic concept was that we should look primarily for the earliest foci of food-producing communities in the northwestern-northern-northeastern fringes of the so-called "fertile crescent", i.e. the roughly semicircular mountainous and hilly region stretching from Palestine through northern Iran and southeastern Anatolia to the Zagros Mountains, because this region had the most important prerequisites for the transition to agriculture and animal husbandry; sufficient rainfall, the cereals, herbaceous plants, etc., and the occurrence of domesticable animal species in the wild. However, further research modified Braidwood's original idea, insofar as it was also found outside the mentioned area.

664
===== PAGE 654 =====
Very early Neolithic sites have been discovered, notably in the southern half of Western and Central Anatolia.
Below, we will present the current state of archaeological research in the Near East primarily on the basis of five recent standard works (the works of Müller Karpe, Narr, Hrouda, Amiet, Lloyd given in our bibliography), but also taking into account the relevant volumes of the "Archacologia mundi" series (1. in the literature mainly the works of Margueron, Alkim, Perrot, Huot), as well as volumes XIII and XIV of the latest edition of "Propyläen Kunstgeschichte". Given the inevitably necessary large number of references to specialist literature, we have largely limited the intertextual references to the author's surname, the year of publication of his work and the page number cited.
First of all, however, we must clarify a fundamental terminological issue, namely the problem of the precise naming of certain paleoarchaeological periods. In this area, there is often considerable confusion, which often hopelessly confuses the archaeological chronology — especially for non-specialist researchers.

We encounter two such phenomena most often. One is the inaccurate application of the concept of the Neolithic. In modern archaeology, the primary and indispensable criterion for the Neolithic — for a good 30-40 years now — has been food production, i.e. agriculture and (or) animal husbandry. This technical term therefore now exclusively denotes an economic stage. In the past, it was customary to associate the concept of the Neolithic with the presence of polished stone tools or ceramics. Since then, however, it has been repeatedly proven that neither can be considered a defining characteristic: where there is no suitable — easily worked — type of stone, there we often find unpolished — chipped or microlithic — stone tools typical of the Mesolithic — even in advanced food-producing cultures, i.e. in the Neolithic stage; on the other hand, e.g. In Pre-Asia — and elsewhere — agriculture and animal husbandry had been practiced for thousands of years in several places, that is, the Neolithic, when pottery appeared. This Neolithic without pottery is called the Neolithic without ceramics or pre-ceramic (aceramic or pre-ceramic) Neolithic. At the same time, in more backward areas, we often find ceramics — which the people there took over from their more developed neighbors — the population was evidently at a fishing-hunting-gathering economic stage, that is, at a late prehistoric (Upper Paleolithic) or transitional stone (Mesolithic) level. It is obvious that in such cases we can by no means speak of "Neolithic" civilizations in the sense —. However, there are no traces of agriculture or animal husbandry, of modern archaeological terminology,

Well, exactly such an archaeological culture — the culture of the Paleolithic-Mesolithic fishing-hunting settlements of the Eastern Urals, discovered by Chernyvetov,
665
===== PAGE 655 =====
where ceramics of southern, Celtic origin also occurred - e.g. István Fodor consistently calls it "Neolithic" (1. Fodor 1975, pp. 40-47. cf. also our book II. pp. 309-310.).

Fodor's "fisherman-hunter, Neolithic" without breaking leads us into the second terminological confusion, which, however, perhaps causes even greater confusion than the first, especially in terms of assessing and comparing the cultural development of areas that are more distant from each other. This also quite often observed, impermissible and unmethodical practice consists in absolutizing the temporal section of the cultural development of a certain — but completely arbitrarily (or very consciously?) selected — area, and then projecting it to characterize and name the archaeological cultures of other regions of our Earth. This area, considered as the absolute measure of all things, is of course — perhaps it goes without saying — the northern half of Central Europe, the zone of the so-called ribbon-decorated and then cord-decorated ceramics, one of the most frequently assumed ancestral homes of the Indo-Germans, where the Neolithic — as is known — began roughly BC. It lasted from 4000 to 2000.

— "Methodists" who apply this method in any other field also speak of "Neolithic" civilizations in all cases when it comes to a culture belonging to between 4000 and 2000 years ago — we can of course add: especially if it fits into their perspective in one way or another. As Fodor did, apparently in his effort to prove a relatively higher cultural level for the hypothetical Uralic aborigines, whom he placed in the area of the East Ural archaeological culture.

To illustrate the absurdity of this practice, we merely point out that — if we want to remain consistent — according to this terminology, we should call the entire Sumerian and Akkadian high culture of the 3rd millennium "Neolithic", solely on the basis that somewhere in Europe at that time — but not everywhere! — it was still Neolithic. These issues are discussed in detail in volume II of Narr's cited handbook (1. loc. pp. 9-14).

It is quite obvious that behind these machinations lies once again the obsession with Indo-Germanic cultural priority: the absolute measure of the chronology of cultural development, according to this view, is the German-Polish region, the territory of the supposed ancestral homeland of the hypothetical ancient Indo-Germans, and everything else is relative to it.

Proto-Asia in the Neolithic Age

In the different regions of Neolithic East Asia favored by nature (sufficient rainfall, crops that could be cultivated, and the presence of wild species of animals that could be easily tamed), various periods of time-666

===== PAGE 656 =====

in, but roughly during the 10th-9th millennium BC it began in smaller or larger groups, and by the beginning of the 7th millennium it could be detected in a continuous chain almost everywhere from Western Anatolia through Syria, Palestine, Eastern Anatolia, Northern Mesopotamia to Western Iran. So far we know of four well-defined Neolithic yyyyy: Palestine-Syria, the western and central region of Southern Anatolia, Eastern Anatolia-Northern Mesopotamia and finally the Zagros region in Western Iran. For the sake of orientation, it is sufficient to mention only the most important sites (1, Table 1 and Map 1).

I. Palestine-Syria

In Palestine, the transition from the Mesolithic to the Neolithic period began with the settled groups of the Natufian culture around 10,000 BC. The Natufian settlements represent the earliest known permanent settlements.

The lowest layer of Jericho (C: around 8350) is also such a temporary settlement, whose inhabitants were already engaged in the systematic collection of wild grain varieties growing in the area, but had not yet sown them. Farming only appears after a hiatus of uncertain duration, around 7000, in the second layer calculated from the bottom. In this period, the population of Jericho numbered about 2000 people, the settlement had a protective wall with a massive tower, i.e. to a certain extent it can already be called urban.

Further south, Beidha is also a Neolithic settlement around 7000, with clear signs of agriculture.

However, at the end of the 7th millennium, the early Neolithic settlements in Palestine were depopulated, presumably as a result of climatic changes (drying). We do not know of any Neolithic settlements in Palestine from the 6th and 5th millennia, and population only began again at the very end of the 5th millennium from the north, from the direction of Syria.

The oldest Coreneolithic settlement in Syria is Mureybet (southeast of Aleppo), which dates back to the 9th-8th millennium BC (around 8500-7500 BC). However, here, typical of the Natufian

In addition to a tool kit, they also found certain signs of incipient agriculture, and even pottery had already appeared. This is our uniquely earliest ceramic find to date, dating from around 8000 BC.

On the Syrian coast, the ceramic Early Neolithic layers of Ras Samra (ancient Ugarit) date back to the end of the 8th millennium, with pottery appearing around 6436 + 100 (C" date), often with incised or impressed decoration with the shell of the cardium shell (cardium or impression pottery).

In several sites on the Amug Plain (the area around ancient Antioch), the Neolithic Age appears at the end of the 7th millennium, with the so-called Amug A horizon, but it should be noted that the oldest cultural layers have not been reached anywhere due to groundwater.

667

===== PAGE 657 =====

The lowest Neolithic layer of Byblos dates back to the turn of the 7th/6th millennium BC, also with so-called Cardium pottery.

II. Anatolia

In Anatolia, the Beldibi, Belbasi and Karain caves near Antalya have been the site of successive Mesolithic layers dating back to 10,000 BC, the finds of which are very closely related to the Natufian in Syria and Palestine.

However, in the two upper layers of Karain, signs of agriculture are already apparent, accompanied by blackish, monochromatic pottery, which shows very close, almost identical, parallels with the earliest pottery of Mersin and Tarsus (from the end of the 7th millennium). In Beldibi B layer, identical ware was found with pottery from layers X—XI of Catal Hüyük (ca. 6500-6600 BC),

UBAlkim classifies these cave settlements as belonging to the Neolithic period from the beginning of the 7th millennium (see the above in 1. Alkim 1968, pp. 48 and 251).

In the layers of the Cayönü settlement in eastern Anatolia (near Diyarbakir) dating from about 7600-6500 BC, fully developed animal husbandry (sheep, goats, pigs) and agriculture (wheat, peas, lentils) can be clearly observed. The construction is also surprisingly advanced. Regular rectangular houses were found, with stone foundations and polished terrazzo-like floors.

with stone floors. Moreover, cold-hammered copper tools also appear around 7000. The C" age of the lower layers is, according to two measurements, 7570 and 7520 BC (I. Burney-Lang 1975, p. 528).

All this points to beginnings at least in the 9th millennium.

The ceramic Neolithic layers (VII-I) of Hacilar (Western Anatolia) date back to the end of the 8th millennium. In the AV layer, the cultivation of two types of wheat, barley, lentils, and vetch was recorded.

Already in the lower layers, houses built of mud bricks were found, with walls painted red and white, sometimes with painted geometric patterns. The settlement was abandoned sometime in the middle of the 7th millennium and was not re-founded until around 5900.

The lowest layers of the eastern settlement hill of Catal Hüyük (Konya Plain), which according to the excavator J. Mellaart extend very deep, have not yet been excavated. During a test excavation in 1965, ceramics were found in the XIII. layer, calculated from the top, whose age is about 6800 BC. This find undoubtedly points to the beginnings of the 8th millennium. During the more thoroughly examined XI. layer, between 6500-5700, the settlement was already clearly urban in nature (13 hectares), with cult rooms, wall paintings, human and animal sculptures, representations of anthropomorphic deities, mostly mother goddesses in the sanctuaries, weaving, and about Since 6400 (layer IX) with the processing of copper and lead, now by smelting and casting. A whole range of crops were grown (14 varieties), and signs of incipient irrigation were also observed. The extremely high level of stone working is evidenced by the tiny holes in drilled obsidian beads, through which even the thinnest modern steel needle cannot often be inserted (see

668

===== PAGE 658 =====

Burney-Lang 1975, p. 46). The much-discussed question of the durability of houses built of mud bricks was also brought to light at Catal Hüyük: on the walls of the houses of each layer, 100-120 layers were found, which clearly indicates that these houses stood for at least 50-60, but more likely 100-120 years (counting twice or once whitewashing per year),

In the lowest layer of Suberde (Konya Plain) (measurement C: ca. 6750 BC), ceramics were not yet known, but a molten copper ore was found.

All these early Anatolian finds leave no doubt that the beginnings of metalworking must be sought in Anatolia, which is actually not surprising, since the ore richness of the mountains there is well known, but metal processing could obviously only have developed in a region where there were ore deposits nearby.

The lower layers of Mersin (Cilicia) (XXXIII-XXVII.) belong to ca. 6300-5900, already with pottery. The so-called cardium and impresso (or nail-impression) pottery also occurs here.

It should be noted, however, that the excavators never reached the virgin soil due to groundwater.

In Tarsus (east of Mersin), a Neolithic settlement corresponding in every way to the Mersin Neolithic and belonging to the same culture was found, the lowest examined layers of which can also be traced back to the end of the 7th millennium, but groundwater — similarly to Mersin — prevented the excavation of earlier layers here too.

The next Neolithic stage — between ca. 5900-5000 — is represented in Anatolia by, among others, the XXVI-XXI. layers of Mersin, the 9 layers of the newer settlement of Hacilar (ca. 5900-5000), Can Hasan (ca. 5500-4800), Sakceğözü, the western settlement hill of Catal Hüyük (ca. 5500-5000). Numerous metal objects were found everywhere at these sites, therefore the research speaks of a Chalcolithic Age from the middle of the 6th millennium onwards.

Another very important piece of information is that the very close parallels of the monochromatic ceramics of these settlements at the beginning of the 6th millennium are found throughout the EI6-

They can be detected in Asia, from Anatolia to Iran, and even in the Aegean region, e.g. on the island of Skyros (I. Narr 1975, p. 133). This undoubtedly indicates mutual and regular contacts affecting far-flung regions, already in this early period. Moreover, such extensive contacts can be detected as early as the 8th-7th millennium: the Natufian tool kit, e.g. from Syria, Palestine to the aforementioned Western Anatolian cave settlements, is largely similar: the obsidian finds of the Jericho aceramic layers come from Central Anatolia and the Armenian mountains: the characteristic human head sculptures of the skull cult first discovered in Jericho — modeled from clay on skull bones — (around 7000) are found in the same form in the aceramic layers of Hacilar, also dating from around 7000.

In the first centuries of the 5th millennium, life ceased in almost all settlements in the central areas of Anatolia, with only traces of the Chalcolithic period in Cilicia (Mersin, Tarsus) and in the west, at the settlement of Beycesultan.

669

===== PAGE 659 =====

Kr. e.	Anatolia	Syria-Palestine		
Karain, Beldibi, Belbasi		Natufium (Mesolithic)		(Mesolithic)
Mureybet collected				
Hacilar		Ras Samra (ceramics)	6	Without Beidha Catal Hüyük (east)
Hacilar (keramikus)				
Catal Hüyük (article)				
Beycesultan				
Table 1: Neolithic cultures in Pre-Eustria 670				

===== PAGE 660 =====

Mesopotamia

III. Northern Mesopotamia

Sanidar dates back to the 10th millennium, also with Mesolithic finds, but goat and sheep breeding was already documented at the nearby Zavi Chemi settlement around 8900 (measurement C).

From this phenomenon, research generally concludes that the inhabitants of Jarmo adopted pottery from some nearby, more developed center (obviously from Western Iran — 1. below), which then gradually degenerated among them.

The Coreneolithic settlement of Mlaffa'at appears to be somewhat earlier than Jarmon, around the second half of the 8th millennium, but here too we find a fully developed rural settlement.

The seven layers of Tell Bugras (along the middle reaches of the Euphrates) are roughly contemporaneous with Jarmo (7th millennium BC), but since pottery -- here the so-called Old Monochrome ware -- appears only in the uppermost layer, the settlement is often placed before Jarmo.

IV. Western Iran

can be distinguished: 1.) The region of Luristan.
674

===== PAGE 662 =====

(south and southeast of Kermanshah); 2.) The northern mountainous area of Khuzistān (Susiana); 3.) The area around Urmia-16 (Azerbaijan).

Ad 1. The finds from the intermittent settlement of Tepe Asiab, starting around 10,000, mark a transition from the Mesolithic period to food production.

In the lowest layer of Gang Dareh Tepe (according to C¹⁴ measurements 8450 ± 150 BC), agriculture cannot yet be directly proven, but it is very probable (the abrasion marks on several stone blades indicate their use during harvesting). In the following layers, however, wheat cultivation and goat breeding can already be dated, and even unfired clay pots appear around 7500 BC, accompanied by clay figurines in the shape of animals and humans. The uppermost layer is C¹⁴ dated 6960 ± 170 BC.

At the Tepe Guran settlement, 12 layers have been excavated, starting around 6900, but the bottom of the cultural layers has not yet been reached. In the second layer from the bottom, agriculture can already be dated, and in the fourth - according to the latest dating around 6700 - ceramics also appear.

The lower layers of Tepe Sarab also belong to the 7th millennium. The pottery found here is practically identical to the early pottery of Jarmo, i.e., it can be dated to around 6500. Animal husbandry (goats) is also dated to this period, as well as clay figurines in human and animal forms: e.g. the so-called "Sarab Venus" from around 6250, which already has all the characteristic features of the steatopygous female idols that were later widespread throughout East Asia, and can thus be considered their direct prototype.

Ad 2. Agriculture and animal husbandry (goats) also began well before 7000 in the northern mountainous regions of Susiana.

Ali Ko\$ 33 layers, measured at over 112 C, date to between ca. 7950-5750 BC. Hammered copper appears in the first half of the 7th millennium, and ceramics around 6500 BC.

Bus Mordeh is roughly contemporaneous with the Ali Kos settlement, and the beginnings of Djaffar (Dzsaffar) can be dated to around 6500-6400.

Ad 3. The three lower layers of Hasanlu (X—VIII.), the approximately contemporaneous Haggi Firuz (deepest layer C: 5537 ± 89 BC) and Dalma Tepe, Pisdeli Tepe, Janik Tepe (lowest layer C: 5297 ± 71 BC) sites reflect a highly developed Late Neolithic, almost Chalcolithic cultural state: their ceramics show unmistakable parallels with Hassuna and Tepe Sarab (1. above).

Therefore, we must either date the Coreneolithic of these areas much earlier — even if we do not know of such older settlements — or we must consider the above sites to be newly founded colonial settlements of farmers who arrived from the south.

However, the findings of Ghar-i Kamarband (Belt Cave) discovered near the southern shore of Lake Caspian, where the Mesolithic to the Neolithic period are dated to a much earlier period, testify to the fact that the Neolithic period in question in the northwestern Iranian region dates back to a much earlier period.

===== PAGE 663 =====

The transition to *tikum* is observed without a break, just as in the aforementioned southern Anatolian caves. Above the Mesolithic layers belonging to the 10th–8th millennium BC, in the ten upper layers dated to the 7th millennium BC, the beginnings of sheep and goat breeding can be discerned (the proportion of young animals is significantly higher than in the wild herd), and primitive ceramics also appear.

We see the same development in the layers of the neighboring Hotu Cave, only from a slightly later time.

These cave finds from the South Caspian region therefore show that influences from the northern regions of the Zagros Mountains triggered the transition to food production in this region (cf.: pottery appears simultaneously with the earliest beginnings of sheep and goat breeding, which is otherwise nowhere to be seen in primary coreanolithic foci).

Also worth mentioning here is Tepe Sialk in central Iran (south of Tehran), where the Neolithic settlement began in the second half of the 6th millennium (according to some, around 5500, others place the lowest layer around 5400-5300), immediately with advanced agriculture, animal husbandry and ceramics, and not long after, hammered copper objects also appeared.

This means that we are not dealing with an independent initiative here either, but with a colonial settlement — presumably to exploit the local copper deposits. This is also confirmed by the strong Hassuna-related features of the ceramics of the oldest period (Sialk I). Furthermore, we know that numerous copper objects have been found at the sites of the Hassuna-Samarra cultural circle, so metalworking was already known there in the middle of the 6th millennium (1. below).

Northern Mesopotamia in the Late Neolithic and the first half of the Chalcolithic

From the above it can be seen that around 6000 BC, throughout East Asia, we find a developed Neolithic period, covering vast areas and going back at least 1000 years (Map 1. 3), and in many settlements, the working of copper and lead had been known for centuries. Therefore, Near Eastern archaeology calls the first half of the 6th millennium the Late Neolithic, and from the middle of the millennium onwards speaks of the Chalcolithic (Chalcolithic — in Soviet archaeology, the Aeneolithic). This period is calculated to the beginning of the Sumerian Bronze Age in Mesopotamia, around 3200 BC,

In the Copper Age, Mesopotamia and its immediate surroundings increasingly take on the leading role in Near Eastern culture: initially the northern areas, present-day Northern Iraq, Northern Syria and Western Iran (the region later referred to by the Sumerians as "Subir-Ki"), and then, since the second half of the 5th millennium, Southern Mesopotamia. For this reason, the Near Eastern chronology of the Copper Age is adapted to this region; In the following, the chronology of the

===== PAGE 664 =====

the first

We present geologically more obscure sites (1. Table 2.57 and Map 2)

Although the Hassuna stage (ca. 6000-5600) still belongs to the Neolithic, it seems appropriate to begin the description of the Copper Age with it in order to better understand the connections

In Nineveh, the former capital of Assyria, unpainted, scratched and grooved pottery was discovered in the lowest layer of a deep excavation in the 1930s (Nineveh I ware), which could not be chronologically classified at the time, but later proved to be identical with the Hassusa pottery that gave the period its name (Samarra was found in Nineveh IIb, Tell Halaf in IIc, so-called "northern Ubaid" in IIIL, and Uruk pottery in IV; all of these 1, below). This was one of the first stratigraphic observations that provided concrete clues for determining the temporal succession of Mesopotamian pottery types before the Sumerian high culture.

In Hassuna (southwest of Mosul), a settlement dating from the end of the 7th millennium BC was excavated. In the lowest layer (Ia), primitive, monochromatic gray-black clay vessels were found, which showed close parallels with the oldest, so-called Old Monochrome ware of the Amug plain (Amug A, B) and Cilicia (Mersin, Tarsus). From layer Ib up to layer IX, the typical Hassuna pottery appeared (the earlier Nineveh I ware), but already in layer III, the so-called Samarra pottery appeared (I below) and lasted until layer VIII. Moreover, starting with layer VI, the Tell Halaf ware also appeared, and can be detected up to layer XIII. At the same time, however, from layer XI up, the so-called Northern Ubaid pottery can also be found. We will get to know all these ceramic types in more detail later.

From the layer observations made at Hassuna, it became clear that the different types of pottery mentioned were layered on top of each other, but always in such a way that the older style forms continued to survive for a long time. That is, the transition was continuous, with the new fashion only gradually replacing the old.

The excavated layers of Hassuna date back to between about 6100 and 4200 BC. In the meantime, more than 40 Hassuna-era settlements have been found (e.g. Jarim Tepe, Umm Dabaghijah, etc.).

The Samarra stage (ca. 5600-5000) is already distinctly Chalcolithic in character. The definitive chronological classification of the Samarra pottery, discovered by Herzfeld between 1912-14, was only achieved in the 1960s with the excavation of Tell es-Sauwan (in the immediate vicinity of Samarra, north of Baghdad). Here, Hassuna pottery was found in layers I-IV, counted from the bottom, but the typical Samarra ware appeared in layers II (ca. 5500), and became dominant in layers III-IV. However, in layer V, Halaf pottery was also represented. The continuous transition can therefore be observed here as well.

The layers of Sauwan begin around 5600 BC. Copper objects have been found even in the lowest layer. Around 5400 BC, during Layer III, the settlement was ditched.

675

===== PAGE 665 =====

Kr.e. [korszak] Hassuna Ninive Samarra- TSoga Mami Sauwan
Samarra cor

ty c 4 3 is) if
Northern Ubaid period

Uruk-Gaura kor

Table 2: Northern Mesopotamia in the Chalcolithic Age
676

monochromatic goods

Hassuna goods

Samarra goods

Halaf goods

Nineveh I (Hassuna)

Nineveh IIb. arrow (Samarra)

Nineveh III. aru (Halaf)

Nineveh III ware (northern Ubaid)

Nineveh IV ware (Uruk)

Hassuna goods

Samarra goods

channels

"Fish goods"

Hassuna and Samarra goods

Eridu goods canal system

Halaf goods Haggi Mo- hamed goods

northern Ubaid ware

===== PAGE 666 =====

Tell Halaf | Arpadich Tell Brak

Halaf äru

animal goods

Samarra and Halaf goods

northern Ubaid ware

Samarra and Halaf goods

Your goods

Jemdet Nasr merchandise

Halaf goods

northern Ubaid ware

Your goods

Lord's Temples

Jemdet Nasr äru

Table 2 (continued)

6.500

6.000

?

Samarra and Halaf äru (Area A)

Halaf ware (Layer XX) Northern Ubaid ware (Ubaid 3) beginning of temples

Northern Ubaid ware (Ubaid 4) temples Uruk ware temples

Jemdet Nasr

goods

677

===== PAGE 667 =====

It was surrounded by a fortified wall. At the same time, traces of the beginnings of irrigated agriculture can also be detected, in the form of several smaller and larger canals.

Suitable sites have been found from the Iraq-Iran border, around Mandeli (TSoga Mami, Serik Kebir, etc.) to the Euphrates (Baghuz). 13080 The Mami site is very important for two reasons.

On the one hand, we know of the very first canal system, built according to well-thought-out plans, from the second half of the 6th millennium (!), which distributed the water of the Gangir River to the surrounding agricultural lands (L. Amiet, 1977, p. 517). On the other hand, the Hassuna and Samarra pottery, which also occurs mixed here in the lower layers, is associated with the Eridu ware of southern Mesopotamia in the upper layers (so-called Fridu-Ubaid 1st period; I. later), and even the typical pottery of the second period of the Eridu-Ubaid cultural stage, the in. Haggi Mohamed ware, is found together with the Hassuna and Samarra pottery. The same pattern of finds can be found in the neighboring Serik Kebir. All this indicates that the first settlers of southern Mesopotamia, the bearers of the Eridu-Ubaid culture, did not come from some distant region — from the neighborhood, from the area of the Samarra culture, as was previously generally assumed.

but the direct →

The assemblages of the Samarra culture settlements are an organic continuation of the previous Hassuna stage. For this reason, and because the two cultures also overlap geographically, many researchers Hassuna-Samarra speaks of culture.

The earliest known site of the Tell Halaf stage (in northern Syria, on the upper reaches of the Habur River) was excavated by M. von Oppenheim. He began his excavations before World War I and completed them in the late 1920s. However, the chronological classification of the multi-colored, magnificent Halaf pottery — similarly to that of the Samarra ware — also became possible only after the excavation of another settlement, Tell Arpacijeh (east of Mosul). At this site, fragments of pottery decorated with animal figures were found in the five lower layers (XV-XD), which are generally considered to be the precursors of the Samarra pottery. The Tell Halaf ware dominated in the X-VI. layers, but Samarra pottery also occurred alongside it up to the VII. layer. In layer V, northern Ubaid ware (Eridu-Ubaid Periods 3 and 4; 1 below) also appeared here, and lasted until the topmost layer. The ten upper layers of the settlement date from ca. 5000-3500 BC, the animal-shaped pottery to the 6th millennium.

Arpatijeh is also known for his famous Halafian domed structures, which were joined by rectangular forecourts and are surprisingly similar to the Mycenaean domed tombs of several millennia later. Such buildings are known from several Asian sites (e.g. Tepe Gaura, Carchemish, etc.), and their spread can also be traced westwards: in the second half of the 3rd millennium BC they already appear in Crete,

It should also be mentioned that in several places in the settlements of the Halaf culture, stone-paved streets were found, and its carriers were already familiar with the four-wheeled cart (with spoked wheels),

678

===== PAGE 668 =====

Halaf pottery represents the peak of polychrome pottery. It appears in Hassuna, Arpatijeh around 5000. After the turn of the 6th/5th millennium, it spread to far-flung regions, and can be found in Ras Samra (layer C-IV A), on the Amug plain (Amug D phase), in Mersin (layer jyyj-- XVI. layer), around Diyarbakir, Malatya, Elazig (Eastern Anatolia), on Tilki Tepe (near Lake Van), in Gird Banahilk (southwest of Lake Urmia), and sporadically in the Kermanshah area in Western Iran. It can be found in the south as far as Baghdad.

The beginnings of Samarra and Halaf pottery can be dated to roughly the same time (c. 5500). The Halaf ware, which appears in Hassuna around 5000, already belongs to the more developed, later types, and its beginnings can therefore be dated much earlier (L. Hrouda 1971, p. 54). The two cultures initially developed in parallel, in space and time, and then around 5000 the Halaf culture superimposed itself on the Samarra settlements, and by c. 4500 it became dominant from the Mediterranean to the Zagros.

The further development of the northern areas (after 4500) was determined by the northern Ubaid and then the Uruk-Gaura cultures. Since both civilizations are of southern Mesopotamian origin, it will be easiest to discuss them together with southern Mesopotamia.

Southern Mesopotamia and Elam in the Circular Period

The modern chronology of southern Mesopotamia and Elamite (Susiana) is based almost entirely on stratigraphic observations from deep excavations at Uruk and Eridu. These two sites therefore deserve special attention. However, it is necessary to briefly review the results of some earlier excavations in order to understand the development of the chronological terminology used today (1. see also Table 3 and Map 2).

The eponymous site of the cultural stage now known as the Eridu-Ubaid (or Ubaid for short) period is Tell al-Ubaid (in English literature, Tell el-'Obaid) near the city of Ur, where the 20th-century characteristic "Ubaid pottery" was first discovered in the 18th century. It was only much later that it was discovered that this ware belongs to the youngest period of the Ubaid period in the modern sense. The However, the name "Ubaid" has since taken root so much that modern research has retained it.

Not long later, in the early 1930s, L. Woolley also found such Ubaid pottery during a deep excavation in the city of Ur, but only in the upper layers of the research shaft. However, on closer inspection, the nature of the pottery changed noticeably: it was very similar to the Ubaid ware, but with significant differences. Thus, the original "Ubaid" period had to be extended backwards in time, and could be divided into two clearly distinguishable stages.

679

===== PAGE 669 =====

See Teer

The

beginning of the yyyyyy temples

Eridu and Haggi Mo- Haggi Moham- | 113881 Moham- | HagSi Moham- hammed ware | | med pots | med pots (med pots
Eridu- Uba- Eridu- Ubaid period 3 items 3 dru 3 goods (cemetery)

Eridu- Ubaid 1 y

Eridu-Ubaid 2 period

ea ga 53 a 6 4 B ga k. ma]

Eridu- Uba- | Eridu- Ubaid Eridu- Ubaid Uruk äru | | Eridu- Ubaid period 4 items 4 items 4 items 4 aru temples (Ubaid I) (U band I)

Uruk äru temples Uruk

äru

temples

(late)

pictography Jemdet Nasr

Jemdet Nasr

wares wares pictography Table 3; Southern Mesopotamia and

(cemetery)

Elam in the Copper Age 680

===== PAGE 670 =====

HagSi Ras Susiana was

A

born (Eridu Ub. 1.)

Haggi Mo- Haggä Mo-Eridu-Ubaid

Susiana b äru hammed äru | | hammed äru Susiana cd 3

(Eridbu-Ub. 2.)

years Eridu- Ubaid |

body (Eridbu-Ub. 3.)

Eridu- Ubaid 3 cserepek 3 telepiilés

Jemdet Nasr Eridu- Ubaid | Eridu- Ubaid | Susianae v. (Eridu Ub. 4) 1

Uruk is

Uruk is

Susa B ware temples | (early Uruk)

Susa C äru more than

(late Uruk) 3 m long | 1.5 m long

painted

| tour layer tour layer church

Jemdet Nasr | Jemdet Nasr Jemdet Nasr Jemdet Nasr Susa D äru äru Table 3 (continued) 681

goods

seal-

goods

(Jemdet Nasr) cylinders

===== PAGE 671 =====

divided: the pottery found at the deepest point in Urban was called "Ubaid I" (note the Roman numeral!), the younger one, which was exactly the same as that found in Tell al-Ubaid, was called "Ubaid II".

Then came the deep excavations in Uruk, during which the "Ubaid II" pottery was again discovered under the layers of the so-called Uruk period (1. below). Then between 1937-39, German archaeologists excavating in Uruk examined the Haggi Mohammed rubble mound near Uruk, from which a previously unknown type of pottery was found, which, however, showed organic parallels with the "Ubaid I" ware found in Uruk. For now, therefore, in the absence of precise stratigraphic observations, it was cautiously dated to the beginning of the Ubaid period.

In the meantime, excavations had reached the Archaic layers at several other southern Mesopotamian sites. It must be emphasized, however, that at most sites the deep layers belonging to the 4th millennium were only very superficially examined, and the excavations were almost always concentrated on the first half of the 3rd millennium. Naturally, in their publications, the main emphasis was placed on the 3rd millennium, and the deep layers that had only been excavated in a hurry were somewhat lost in the excavation reports, and they are mostly completely absent from the summary handbooks. As a result, earlier ideas have been able to survive in the indirect literature to this day, according to which the Sumerians only appeared in Mesopotamia sometime towards the end of the 4th millennium.

For this reason, it is absolutely necessary to briefly review the less well-known results of deep excavations carried out here and there at the more notable southern Mesopotamian sites.

In Kis, a 3-meter cultural layer was found under the Mesilim period (ca. 2800 BC) cemetery, from the middle of which, at a depth of ca. 1.5 m, typical Jemdet Nasr period (between 3200-3000) pottery was recovered (1. Schmökel 1974, p. 37). This means that the founding of the city dates back to the second half of the 4th millennium: as we saw earlier, a 1.5-meter layer of debris corresponds to ca. 250 years.

At the settlement called Jemdet Nasr, the eponymous site of the above-mentioned period (northeast of Babylon), the probing carried out to a depth of 3 m under the characteristic Jemdet Nasr pottery did not yet reach the beginning of the cultural layers (1. Schmökel 1974, p. 38). This means that the foundation of the settlement here can also be dated at least to the first half of the 4th millennium.

At Girsu (now Tellö), the later capital of the city-state of Lagaš, the cultural layers have been traced back to the Late Ubaid period, around 4000. In the lowest layer examined so far, Eridu-Ubaid 4 ceramics were found, together with typical Late Ubaid clay figurines, the so-called "bird-headed" terracottas. In fact, the excavators date some of the potsherds to before the Late Ubaid period. The

682

===== PAGE 672 =====

The continuity of the settlement can be traced without a break throughout the Uruk and Jemdet Nasr periods, in the form of Uruk pottery, Uruk-era seal impressions, clay figurines, numerous Jemdet Nasr seal cylinders, etc. (v6. all this in RLA vol. III. heading "Girsu", pp. 385-401; — especially pp. 396 and 400-401).

The sites of the Diyala region, an eastern tributary of the Tigris, were excavated in the 1930s. In the ruins of Tell Asmar (ancient ESnunna), a hastily carried out deep excavation beneath the Abba Temple from the Jemdet Nasr period revealed a cultural layer 3.5 m thick (1. Schmökel 1974, p. 39). This also indicates a history of several centuries, counting back from the Jemdet Nasr period, meaning that the beginnings of the settlement must clearly be dated to the first half of the 4th millennium.

The chronological picture is roughly the same at the other significant Diyala provincial site, Chafadji, but with the difference that the oldest layer of the temple of the moon god (Sumerian Nanna; Akkadian Sin) can be traced back to the late Uruk period, and Uruk-era tombs with typical Uruk grave goods were found in its vicinity (1. Lloyd 1981, p. 47). Further deep excavations have not yet reached the bottom of the cultural layers down to the groundwater level (1. Schmökel 1974, p. 40).

The archaeological horizon was significantly broadened by the excavations carried out on the rubble mounds of Tell "Uger in Middle Babylonia (F. Safar and S. Lloyd 1940-41). Here too, a typical Ubaid II (today: Eridu-Ubaid 4; between ca. 4000-3800) settlement was excavated, and next to it a late Uruk temple built on a terrace, the ground plan of which was identical to the so-called Uruk temple. "white temple"-Eval (around 3200; 1. below). The wall paintings of the temple (geometric friezes, leopards, other animal and human figures) are noteworthy, as they are the earliest finds of this kind in Mesopotamia. Unfortunately, systematic deep excavations were not carried out in "Uğér either, but from the scattered finds on the temple terrace the excavator Lloyd concluded that it can be dated to the turn of the 5th/4th millennium (1. Lloyd 1981, p. 41. chronological table).

The "painted temple" had several antecedents in the early Uruk period (between ca. 3800-3500) and its foundation dates back to the Eridu-Ubaid 4 settlement.

This was the archaeological situation when the excavation of the Archaic layers of Eridu began in the late 1940s (S. Lloyd and F. Safar). This excavation provided the long-awaited precise stratigraphic data for the definitive chronological classification of the different types of Ubaid pottery known so far. Nineteen layers were observed. It turned out that the settlement in Eridu began in the second half of the 6th millennium, around 5400 BC (layer XIX).

Thus, today the periodic chronological division of the so-called Eridu-Ubaid period is based on the successive ceramic types of the deep layers of Eridu.

The typical pottery of the XIX-XV. layers is called "Eridu äru" and is dated to between ca. 5400-4800. This is the so-called "Eridu-Ubaid I" period.

683

===== PAGE 673 =====

In the XIV-XII. layers, between about 4800-4500, exact correspondences of the Haggi Mohammed pottery (1. above) were found. This period occurred in the XI-VIII. layers, the pottery formerly called "Ubaid Eridu-Ubaid 2" ncvet capita I", between about 4500-4000. The new designation, the earlier Ubaid II" pottery, was found in the VII-VI. layers. This period is now called Eridu-Ubaid 3" periods.

It is called the Eridu-Ubaid 4" period; its age is approximately 4000-3800.

Susiana the ware — corresponds to Eridu pottery (Eridu-Ubaid 1);
Susiana b ware — corresponds to 11281 Mohammed pottery (Eridu-Ubaid 2);

Susiana c and d ware — corresponds to Eridu-Ubaid 3 pottery;
Susiana e or Susa A ware — corresponds to Eridu-Ubaid 4 kerámia;
Susa B ware — corresponds to early Uruk pottery;
Susa C ware — corresponds to late Uruk pottery;
Susa D ware — corresponds to Jemdet Nasr pottery.

All these Elamite ceramic types — except for the magnificent Susa A ware of high artistic quality, which is far superior in quality to the contemporary Eridu-Ubaid 4 pottery — are organically related in their decoration and form to the corresponding Mesopotamian vessel types, so much so that the sherds can often hardly be distinguished from the Ubaid or Uruk ware of the same period. At the same time, other Elamite finds also stand in very close parallel with the Mesopotamian forms of the Chalcolithic; that is, the cultural development of the two areas cannot be separated from each other. A certain degree of separation of Elam begins only at the end of the Uruk-Jemdet Nasr period, but even then it can only temporarily, for a few centuries, be independent of Mesopotamia; in the middle of the 3rd millennium, at the beginning of the Akkadian period, it is again completely under Mesopotamian influence. This is a relative culture. »

688
===== PAGE 678 =====
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The development of Elamite writing also falls on the period of their independence (the first half of the 3rd millennium BC), which, although clearly influenced by Sumerian pictography, differs from it to a certain extent.
Northern Mesopotamia and adjacent areas during the Eridu-Ubaid, Uruk and Jemdet Nasr periods
Let us now continue to follow the development of northern Mesopotamia-Syria in the later stages of the Samarra-Halaf period, and in the period corresponding to the 161 4 Uruk and Jemdet Nasr period. times (see also Table 1 and Table 2).
Northern Ubaid period
The Eridu-Ubaid culture, fully developed in periods 1 and 2, soon became dominant in the north, and eventually superimposed the finds of the Samarra-Halaf cultural circle everywhere. This process can be clearly seen already in periods 1 and 2 of the Eridu-Ubaid culture — e.g. in TSoga Mami and some neighboring sites, where "Eridu" and "Haßgi Mohammed" wares occur mixed with Hassuna, Samarra and Halaf wares (1 above, p. 678) — but to a greater extent it is only observed around 4500, at the beginning of the Eridu-Ubaid 3 period. We have already seen that, for example, in Arpañijeh, from layers V-IV (c. 4500) onwards, the Nineveh III and Hassuna yy. It also appeared in the 1st layer, also around 4500 BC in both places. We find a similar picture in all other northern settlements — Northern Ubaid pottery dominated, as did of the Chalcolithic period.
It follows that the period of Samarra pottery (ca. 5500-5000) roughly corresponds to the "Eridu ware" in the south (Eridu-Ubaid 1), and the northern flourishing of Halaf pottery (ca. 5000-4500) is paralleled by the "Haggi Mohammed ware" in the south (Eridu-Ubaid 2), as is confirmed not only by the mixing of these four types of vessel types at several sites (TSoga Mami, Serik Kebir, etc.), but also by the very strong Samarra-based "Eridu ware" and the equally clear Halaf parallels of the yyyy Mohammed pottery (L. Amiet 1977, p. 522).

We must therefore view the former relations in such a way that the colonization of southern Mesopotamia began primarily from the area of the Samarra culture around 5400. This is supported by the geographical proximity of the Samarra culture to the northern edge of the southern Mesopotamian plain, in addition to the strikingly close parallels of the Eridu-Ubaid 1 pottery just mentioned, but this is also supported by the fact that the beginnings of irrigated agriculture, the very first canal systems, can also be dated in the area of the Samarra culture from the second half of the 6th millennium (Tell es-Sauwan, in the vicinity of TSoga Mami; — cf. above, p. 678). So agriculture reached its peak here 689
===== PAGE 679 =====
first, the technical level that was essential for the greater settlement of the rain-poor southern plains.
It should be mentioned here that the colonization of the plains of Elam (Susiana) also began simultaneously with Mesopotamia, also from the neighboring northern areas (Ali Kos, Djaffar, etc.): around 5000 BC, irrigated agriculture can be dated in Susiana as well. It is very important to note that at the same time, the oldest pottery of Elam, the so-called Susiana A ware, shows such close and organic parallels with the Eridu-Ubaid 1 pottery, and the following stage, the Susiana B ware, also with the Eridu-Ubaid 2 pottery, that we can speak of the cultural unity of the two areas (L. Hrouda 1971, p. 69; but cf. also above, p. 688).

Then in the following period — around 5000 — the Halaf culture expanded into the core areas of the Samarra culture (after 5000, Samarra and Halaf wares occur mixed in most sites), and around 4800 it already had a massive impact on the Eridu-Ubaid culture, which originally originated in the Samarra areas, in its 2nd period (cf. the explicit Halaf parallels of the 11881 Mohammed wares!).
Shortly thereafter, however, the situation changed radically. In the next cultural stage, the characteristic ceramics of the southern Eridu-Ubaid 3 period, together with numerous other typical Eridu-Ubaid 3 finds, appear in the north, and after 4500 BC they are superimposed on the entire earlier Samarra-Halaf cultural circle. To the west it soon reaches Syria (Tell Brak), the Mediterranean coast (Ras Samra IIIB layer; the so-called Amug E stage on the Amug plain), Kilikia (Mersin XV-XI layers), to the north Eastern Anatolia (Malatya, Elazig region) and the area around Lake Urmia (Dalma Tepe, Pisdeli Tepe, Gird Hasan Ali, etc.), and to the east it reaches the Zagros region, Susiana (Susiana c, d ware), Central Iran (Tepe Sialk 1115 layer), and even Eastern Iran (Tell-i Iblis) (L. Hrouda 1971, pp. 64, 69-70 and 72; Narr 1975, pp. 141-142). This is the first "Koine pottery" over a vast area in the Near East — notes, for example, Hrouda (1971, p. 64). Amiet also states that the large-scale and uniform distribution of the characteristic expanding ceramic these northern types of the Eridu-Ubaid 3 and 4 periods indicates the existence of a comprehensive cultural Koine (L. Amiet 1977, p. 49). Therefore, the research calls the period between 4500-3700 in areas the Northern Ubaid cultural circle or period — omitting here the term "corresponds to the original Ubaid period" (Ubaid I-II; today Eridu-Ubaid 3-4).
"Eridu-" addition, since this period is precisely

| All this very likely indicates that the Stone Age
| in the last phase and the first half of the Paleolithic period, i.e. roughly the 6th-5th millennium BC, with mass emigrations to more distant areas parallel (these 1. later) simultaneously with the development of Middle Eastern culture in the central region, Syria, Mesopotamia, and Western Iran, also large-690

===== PAGE 680 =====
large-scale internal migrations and cynic mixing took place, during which a dynamic cultural (or ethnic) group sometimes had a dominant influence on neighboring civilizations, even superimposed them (cf. Hassuna + Samarra; Samarra > Halaf; Halaf > northern Ubaid), while the newly created cultural communities expanded to larger and larger areas. The Samarra civilization only encompassed NORTH Mesopotamia between 5600 and 5000 BC, while the Halaf civilization encompassed all of Syria, Cilicia, the neighboring parts of southeastern Anatolia and the adjoining strip of western Iran around 5000 BC; and the cultural koine of the northern Ubaid period, starting in 4500, had already penetrated far into the central and eastern regions of Iran. In Hungarian, ca. After 5500, we can speak of an increasingly greater cultural (and, in parallel, obviously linguistic) leveling in the central areas of East Asia (more details later).
The reality of this conclusion is raised to the limit of certainty by the already mentioned indisputable fact that any kind of agricultural culture could have developed in Southern Mesopotamia only at a fairly high level of cultural, technical and social development: in the absence of a centrally thoroughly organized and directed, unified society, forced to carry out orders without contradiction, in any case, the extremely complicated and enormous, non-stop investment of labor could not have been done. to create and continuously maintain a river regulation and drainage system (cf. e.g. Narr 1975, pp. 142-143: "In the dry Ketfolyamköz — but similarly in other alluvial floodplains of East Asia — well-functioning irrigation farming was necessary. However, the prerequisite for this in all cases is a sufficiently strict organization and the directed deployment of labor for the construction of dams and the canal network." — Emphasis by Narr).

It is another question, of course, whether this central power was called a "deity", a priest, a "deity's earthly viceroy", a king or something else at that time; or whether it was exercised by some kind of central council, the assembly of elders, the priestly order or even both together (we find clear references to the important role of the council of elders in ancient times in the Epic of Gilgamesh!). All this does not change the essence of the matter, the fact that in southern Mesopotamia, at least from the turn of the 6th/5th millennium BC, starting with the consolidation of the Eridu-Ubaid culture based on irrigation agriculture, there was undoubtedly a distinctly state-like organization that organized a considerable number of people, centrally controlled them, and coordinated public works for price regulation and canalization in larger areas.

we have to reckon with formations;
- with satisfactorily organized community relations, with an extensive central leadership, which was also able to assert its authority - 691

===== PAGE 681 =====
his will to live, he was able to get his ideas across. In other words, it is practically certain that the first states in Del-Mesopotamia had already formed in the 5th millennium (cf. Soden, Propyl-Weltgesch. Vol. 1, p. 532).
However, this briefly outlined social situation — the former existence of which cannot be doubted for a moment as a consequence of the categorical imperative of the given geographical circumstances — also presupposes, in a compelling way, common intelligibility; that is, a more or less uniform communicative language had to have developed in southern Mesopotamia by the beginning of the 5th millennium at the latest, on which the gigantic community 0081
Jott, masses of people living and working in constant and close contact spoke to each other.
This language obviously arose through the amalgamation of the dialects of the colonists from the more northern regions, the Samarra and then the Halaf cultural circle, i.e. through areal language equalization. In addition, however, we must also take into account that this negotiable language — the later widespread language of the Eridu-Ubaid cultural circle

Judging by its massive effects and dominant superstratifications on other cultures, after 4500 it reached beyond the narrower borders of Southern Mesopotamia, at least to the core areas of the earlier Samarra-Halaf culture, where — as we already know — the characteristic manifestations of the Eridu-Ubaid culture have appeared everywhere in massive and uniform mass in the assemblages of finds since 4500. This linguistic equalization in the northern areas was also facilitated by the fact that — as we have just seen — the spoken language of the Eridu-Ubaid culture was apparently originally itself a mixture of the dialects of the settlers from the area of the Samarra culture.

On the other hand, the dominance of the Eridu-Ubaid culture over vast areas draws attention to another, very important circumstance. This striking phenomenon can only be understood in the sense that, under the compelling influence of the geographical and climatic conditions of the southern Mesopotamian plain, the population there — willingly or unwillingly — had to repeatedly and inevitably achieve almost superhuman feats if they did not want to starve to death or perish completely during the first major spring flood. — It seems beyond doubt that the Sumerian flood myths are also based on this atavistic fear of floods.

"The natural environment therefore posed a constant challenge to human ingenuity, constantly forcing it to perform at its maximum. In southern Mesopotamia, the mental sluggishness that is a common human trait in areas more favored by nature could not prevail. On the plain between the Tigris and the Euphrates, however, mental agility was a matter of life, the brain's long-term 692

was under "stress", you

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The credence of this constant stimulus can soon be seen tangibly in the archaeological finds: in the explosive cultural development of the population of the Eridu-Ubaid culture, which undoubtedly had above-average intellectual abilities, and not least in the enormous size of this culturally extremely active population. In its expansion into the surrounding areas, it is primarily this circumstance, as well as the overpopulation caused by the previously unattainable yields of irrigated agriculture, that explains the increasingly dominant role of the southern Mesopotamian civilizations — first the Eridu-Ubaid, then the Uruk and Jemdet Nasr cultures — in the cultural development of almost the entire Near East, and soon much larger regions.

Let us now review the more northern areas surrounding Mesopotamia during the Uruk period. The strong southern Mesopotamian influences did not cease with the end of the Eridu-Ubaid period, but can be detected in many finds from the northern settlements throughout the entire Uruk period. Thus, at the beginning of the Uruk period, both the painted pottery of the northern Ubaid period and the still sporadic remains of the Samarra-Halaf type of colored ware disappear from northern Mesopotamia and Syria, and unpainted clay vessels very similar to the characteristic Uruk pottery appear (e.g. in layer IV of Nineveh, in Telul et-Talatat, in Nuzi, in the upper layers of TSoga Mami, etc.), which often also have the same shapes as the contemporary Uruk pottery (e.g. the so-called bell-shaped pot — "Glockentopf" — or the so-called flower pot — "Blumentopf" — etc.). In addition to ceramics, however, other types of finds characteristic of the Uruk culture also appear almost everywhere in the northern settlements.

The Gaura (or Uruk-Gaura) period is roughly contemporaneous with the Uruk period in southern Mesopotamia. It was named after the Tepe Gaura settlement northeast of Mosul, because at this site one could clearly observe not only the phenomena of the cultural development of the northern Ubaid phase in northern Mesopotamia, but also the continuous, uninterrupted transition to the Uruk period, as well as the diverse southern connections that remained unchanged during this period. For this reason, Mesopotamian archaeological literature often speaks of the Uruk-Gaura period, indicating the contemporaneity of the middle Gaura layers and the Uruk period, as well as their organic connections. The lowest (XX.) layer of the Tepe Gaura settlement mound excavated so far has yielded Halaf pottery starting from ca. 4750. However, Samarra and Halaf ware have been found in six layers at the north-eastern foot of the mound. These layers (the so-called "Area A") are earlier than layer XX. of the mound, and can therefore be considered as layers XXVI-XXL. It should be noted that the excavations have not yet reached the bottom of the cultural layers (neither on the mound nor in "Area A"), which is not surprising in this region, since 693

===== PAGE 683 =====

It is in the immediate vicinity of ancient settlements such as Hassuna, Nineveh, Arpacijeh, etc., which can be traced back to the beginning of the 6th millennium BC. 5 Zavi Chemi (Sanidar) itself is located barely 100 km away, where — as we have seen — sheep breeding was already recorded at the beginning of the 9th millennium BC (1. above, p. 670). In the XIX. layer of the Gaura rubble mound — around 4500, as in the other northern sites already mentioned — the northern Ubaid ware also appears, associated with several other characteristic Ubaid cultural phenomena, and lasts until the XII. layer at about 3700. Eridu-Ubaid 3 ceramics were found in the XIX-XVI. layers, and Eridu-Ubaid 4 in the XVI-XIII. layers. However, in parallel with this, the local vessel shapes and decorative motifs of the Halaf period also survived for a long time, which is clear evidence of the peaceful, gradual conquest of the southern culture.

Also in Gaura's XIX. layer, i.e. around 4500, the first structure that can be rightly called a temple in Northern Mesopotamia appears. The northern Ubaid culture culminates in Gaura during the XIII. layer, between about 3800-3700. Three contemporaneous temples were found in this layer, which are in many ways identical to the temples of Eridu's XI-IX. (4500-4100) and VIII-VI. (4100-3800) layers.

In Layer XII, around 3700 BC, the northern Ubaid ware disappears at Gaura, in parallel with the other northern sites, and plain, glazed pottery appears. At the same time, the sudden appearance of large-scale and highly technical metalwork, which coincides with the appearance of unpainted glazed pottery, is also characteristic here (cf. Lloyd 1981, p. 102). In the ca. 80 — mud brick or stone — Uruk period tombs excavated in Gaura (layers XI-IX, between ca. 3600-3200) a huge amount of accessories were found. In one tomb, for example, the dead body was sprinkled with 25,000 beads made of various materials (turquoise, carnelian, jadeite, hematite, marble, quartz, limestone, obsidian, lapis lazuli, diorite, shell, ivory, etc.). This lavish splendor can only be explained by widespread, organized long-distance trade: the nearest lapis lazuli deposit was, for example, It is found in Afghanistan, and the seashells are found in the Persian Gulf. Even more surprising were the extremely rich metal, mainly gold, jewelry accessories: rings, buttons, brooches, crescent moons made of gold plate, rosettes, diadems, an electron (gold-silver alloy) wolf's head made of several pieces soldered together with great skill, etc., all of which are of a remarkably high standard and technical sophistication for these early times. evidence of metalwork. Although half a dozen or so objects made of pure copper were also found in the earlier, northern Ubaid layers of Gaura (the earliest in layer XVII), the general use of metals only begins in layers contemporaneous with the Uruk period. According to the archaeologists who excavated, in some places "... the whole ground was colored green with weathered copper or shone with gold" (1).

Lloyd 1981, 694
===== PAGE 684 =====

104. p.). At the end of the Uruk-Gaura period, during the VIII. layer (between ca. 3200-3100), real bronze was already produced (1, Lloyd 1981, p. 104, i Amiet 1977, p. 509). Seeing this artifact, one of the most ancient Sumerian epic poems involuntarily comes to mind, the one that "Enmerkar and the Lord of Aratta" is a surprisingly accurate parallel to the epic narrative, transmitted to posterity the real events, cultural and ethnic connections of the second half of the 4th millennium BC. According to the epic, the connection with Uruk also indicates very deep connections, and probably at least partial ethnic identity — he sends metals and goldsmiths to Uruk as a payment for grain shipments from Uruk.

In connection with this question, it is essential to mention the characteristic red glazed pottery of the Chalcolithic cultural circle around Lake Urmia (HagŞi Firuz, Dalma Tepe, Janik Tepe, etc.), which is very similar to the red version of the typical Uruk ware, which appears in the vicinity of Lake Urmia as early as the end of the 5th millennium, around 4200 — that is, several centuries before the Uruk period, and with it the beginnings of Uruk ceramics — and is found in almost the same form for a whole millennium until the end of the Chalcolithic, around 3200 — at Janik Tepe, for example, in nine layers with a thickness of 9 m (see Burney-Lang 1975, p. 79). From this, many people conclude that we should perhaps look for the Sumerian homeland in the region of 22 6 — if the Sumerians really migrated to Mesopotamia at the beginning of the Uruk period, an assumption that is becoming increasingly unlikely in the light of recent archaeological research (more on this in I. below). It is therefore much more likely that the large-scale cultivation of metalworking began in these northern regions, as early as the second half of the 5th millennium, and that is why the earliest pottery artifacts found here are the cold-hammered copper tools from around 7000 BC found in the Cayönü settlement in the area, which are the earliest metal finds to date, as well as the ore "metal shock" effect. In any case, this possibility is made very consider- able by the fact that this is also the case in Transcaucasian-Eastern Anatolia. richness of the surrounding mountains: metalworking could obviously only have developed in an area where there were easily accessible ore deposits.

Massive Uruk-era southern Mesopotamian cultural influences similar to Gaura also appear at numerous other northern Mesopotamian sites. For example, in Nineveh's IV layer, the typical single-colored glazed Uruk "flower pots" and handle jars attest to the expansion of Sumerian culture. Similarly, in the IX-VIL-layers of yyy (near Kerkuk) (ca. 3500-3000), "flower pots" and Sumerian seal cylinders from the Uruk and Jemdet Nasr periods appear (1. Amiet 1977, p. 514).

695
===== PAGE 685 =====

At Tell Oaling Aga (near Erbil, east of Nineveh), the Uruk period is represented by sixteen layers. In layer IV, a huge terraced structure was excavated, with a façade over 60 m long, on which at one time there was undoubtedly at least one, possibly two, temples. However, the buildings on the terrace could not be examined in detail. In any case, this terraced structure, dating back to about 3300 BC, is the earliest architectural monument of its kind in Northern Mesopotamia. (I. Amiet 1977, p. 516) Further west, in the territory of northern Syria, signs of the direct expansion of the Sumerian Uruk civilization can be observed after about 3500, in the form of characteristic Sumerian-type finds (1. e.g. Amiet 1977, pp. 74-75). In Sagir Bazar (in the Habur River region), the Samarra-Halaf settlement (Layer XV-VI) is separated from the re-foundation in the middle of the 4th millennium by a 2 m thick sterile sedimentary layer, in the lowest (V) layer of which, here and there, traces of Halaf pottery can still be recognized, but for the most part, there is a monochromatic glazed pottery made on a disk, very similar to Uruk ware, in the company of numerous metal objects. — In other words, monochromatic, Uruk-like pottery appears here as well, parallel to the greater spread of metalwork.

At Tell Brak (south of Sagir Bazar), Halaf pottery and then the northern Ubaid pottery are followed by the red version of Uruk pottery, also around 3500, and soon the typical Sumerian Uruk temples appear: three successive ones, both architecturally and internally similar to the southern Mesopotamian temples of the Uruk and Jemdet Nasr periods. We were able to identify a temple that was completely consistent in its decoration, dating from the late Uruk period to the Jemdet Nasr period (the so-called "red" and "gray" church in

From the Uruk period, the "eye" temple dates from the Jemdet Nasr period. In addition to the characteristic "eye" idols (hence the name "eye" temple), numerous small finds were found in the rubble layer of the temples, which are practically identical to the typical southern Mesopotamian find complexes of the Uruk and Jemdet Nasr periods (animal figures made of terracotta and bone, rosettes, Sumerian seal cylinders from the Uruk and Jemdet Nasr periods, votive maces, etc., etc.). Tell Brak was therefore undoubtedly an integral part of the early Sumerian culture of the Uruk-Jemdet Nasr period (L. Strommenger 1932, pp. 33-34).

A large, towered, walled city of the late Uruk period, dating from 3500-3300 BC, has recently been excavated at Habuba Kabird and Tell Qaunas on the west bank of the middle Euphrates (east of Aleppo). The temples, which are completely identical to the Uruk type (the floor plans and the niche-pillar wall division are also the same), and the larger private houses were decorated with the so-called clay segment mosaics characteristic of the Uruk period. A number of very ancient, late Uruk Sumerian seal cylinders and seal cylinder impressions have been found, as well as Sumerian economic accounts,

696
===== PAGE 686 =====

clay tablets filled with keprás numbers = not only from the temple area, but also from private houses, which proves the widespread use of written accounting already at the end of the Uruk period, not only in southern Mesopotamia, but almost throughout East Asia (including among private individuals). We can form an idea of the size of the city if we consider that the defensive walls are only visible on the western side for a length of more than 600 m (L.Amiet 1977, p. 75). These finds rank Habuba Kabir among the full-fledged representatives of early Sumerian culture. The city was undoubtedly a Sumerian trading colony (cf. Propyl. Kunstgesch. Vol. XIII, p. 55 and XIV, p. 111; Strommenger 1978, p. 18 and 1982, p. 30-32).

Essentially the same direct Uruk and Jemdet Nasr outgrowths appear in the second half of the 4th millennium at Carchemish, about 100 km north of Habuba Kabira. Here — like Sagir Bazar — the settlement abandoned at the end of the Halaf period was rebuilt in the Uruk period. Typical Uruk pottery (flower pots, etc.) was found in the layers of this period.

On the Syrian coast, the effects of the late Uruk period appear around 3400, at first only in the undoubted Uruk parallels of the pottery, then in the Byblos IV layer around 3300 Sumerian seals from the Uruk V period also appear. At the same time, a rapid boom in metalwork can also be observed. In the following period, between 3200-3100, Mesopotamian seal cylinders from the Jemdet Nasr period appear in Byblos. Similar ones were found at several sites on the Amug plain from about 3100-3000 in the layers of the so-called AmugG stage (cf. Narr 1975, p. 153). Byblos at this time obviously played an important role for the more distant branches of the Sumerian Jemdet Nasr culture.

which apparently took place mainly by sea towards Egypt and other areas (see Narr 1975, pp. 159-160).

This important intermediary role of the Syrian coast — and consequently the highly probable presence of Sumerian trading and metalworking groups on the Levantine coast — is largely supported by the figurines cast from real tin bronze from Layer XIV (Amug G phase, corresponding to the Jemdet Nasr period) of Tell el-Djudaideh (Amug Plain), also dating from around 3100-3000, which were found in the company of plain-colored glazed Uruk-type ceramics made on discs and Sumerian seal cylinders from the Jemdet Nasr period (cf. Müller-Karpe 1968, p. 441). That is, these figurines date from exactly the same time in which the direct effects of the Jemdet Nasr culture also appear in the form of seal cylinders on the Amug plain, which in turn indicates the presence of Sumerian traders: the seal cylinders invented by the Sumerians in the late Uruk period were used as property marks — initially, at the end of the 4th millennium. ýýý-ýýýýý ýýýý Only the Sumerians ý as if replaced the signature of the owner.

697
===== PAGE 687 =====

We cannot yet outline exactly how far these direct Mesopotamian emanations of the Uruk-Jemdet Nasr period spread in Western Syria and Anatolia; however, it is already clear that the Mesopotamian Sumerian culture of these early historical times encompassed the whole of Northern Syria, and extended as far as Carchemish, Aleppo, and the neighboring regions to the west (cf. Propyl. Kunstgesch. Vol. XIII, p. 55).

The question of the Sumerian immigration to Mesopotamia, their ancestors and their homeland

At this point, we have an opportunity to examine in more detail the most important and controversial key questions of the prehistory of the Middle East: how long have the Sumerians lived in southern Mesopotamia, and where did they come from?

However, the seemingly heated debate, the problem that at first glance seems almost insoluble — if we try to understand the issue more closely and especially objectively, adhering to the facts of archaeology — soon becomes much simpler and clearer.

First, as a sort of orientation, let us take a closer look at a minor but all the more characteristic detail of this topic, which, in addition to providing many lessons regarding the nature of the debates in this area, also sheds a sharp light on the scientific methodological soundness of the questions generally applied in this field.

The earliest known pictographic clay tablets were found in the Uruk IVa layer, as we have already mentioned, and date from ca. 3300-3250. However, the language of these tablets - being pure pictography - cannot of course be determined pro forma with unambiguous, definitive accuracy, since, for example, from a mere drawing of a human head - ignoring all other circumstances, namely the cultural and ethnic environment in which it belongs - it can only be determined in which language it should be read.

However, we must also know that the entire context of these Uruk IVa tablets clearly and indisputably indicates that they were written in Sumerian: on the one hand, the pictographic tablets with the same symbol system found in Uruk, in layer IIIB (ca. 3100), only about 150 years later, already contain texts in the Sumerian language, 8 while on the other hand, according to the unanimous statement of a whole series of the most authoritative archaeological experts, no phenomenon indicating ethnic change can be observed between the Uruk IV and III layers,

The real situation, therefore - remaining with the above example - is the following: if the aforementioned human {ýý drawing is found in a clearly Hungarian environment, say among the comics of a Budapest newspaper, all the

===== PAGE 688 =====

It is clear to any thinking person that it should be read or interpreted in Hungarian. It was intended for readers who . "on a theoretical basis" of course it is also conceivable that the relevant puzzle, perhaps "on a theoretical basis" of the knew only French, or = again, only in principle" = perhaps it should be read in French so that it is more difficult to figure out. Such "considerations" then, in their own way, are the subject of the paper rabulistics. Well, this situation, which ~ we repeat ~ necessarily follows from the "methods" can be varied as desired in a wide variety of directions. essence of pictography, was used (and is still used today) by some to cast doubt on the Sumerian language of the pictographic tablets of the Uruk IVa period, on "principle" grounds.

In view of the above, we should not even need to prove that this hair-splitting argument can hardly be called objective, much less scientific. However, since this actually secondary topic is generally very typical of the real scientific level of the discussions surrounding the Sumerian "problem", let us quote the relevant lines of the experienced Near Eastern archaeologist S. Lloyd, who is undoubtedly one of the most competent on the Sumerian issue.

"However, since some scholars were led to conclude that there might have been an 'ethical substructure' of partly Semitic origin in Sumer before the (hypothetical) immigration of the true Sumerians, they also claimed that the texts dated to Uruk IVa might be written in a different language, not Sumerian. Speculations of this kind are part of the still ongoing debate about the origin of the Sumerians." (Lloyd 1981, 69th ed.).

In Hungarian: .However, since some researchers allowed themselves to be led (literally, to be led astray" = verleiten lassen (note from us)) to the conclusion that a partly Semitic "ethnic base layer" might have existed in Sumer before the (hypothetical) immigration of the real Sumerians, they therefore insisted in this case as well that the texts dated to Uruk IVa layer could also be of some other, non-Sumerian language. In part, such speculations form the basis of the debate about the origin of the Sumerians that continues to this day."

Lloyd's lines are unmistakable: with decades of excavation experience as an internationally renowned pre-Asian archaeologist (Lloyd spent more than thirty years in the Middle East, led several very important excavations himself, and in addition, as the chief archaeological advisor to the Iraqi government, he was present as an observer at almost every Iraqi excavation after 1945), with his thorough knowledge of the diametrically opposed data set of archaeological facts, he categorically rejects with superior certainty the speculations of certain researchers who allow themselves to be misled, devoid of any concrete basis,

699
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We can see from the quote: the alleged late immigration of the Sumerians is itself a mere hypothesis, and they conclude from this assumption that "possibly" a "probably" partly Semitic ethnic base layer could have existed in pre-Sumerian Mesopotamia. Then, based on this threefold hypothesis, they then base the further possibility that the Uruk IVa pictograph was also "conceivable" that it was not in a Sumerian language.

After this brief but instructive introduction, let us move on to the essence of the question, 5 and attempt to delimit the archaeologically most probable period of the Sumerians' appearance in Mesopotamia.

Let us first take a look at the statements of some authoritative experts on this subject. From these we can clearly see that in recent decades there has been an increasing amount of data indicating that the Sumerians were already living in Mesopotamia in the Erdu-Ubaid period, and that at the beginning of the Uruk period — if at all — only a second wave of them arrived in the region.

—Into the river.

In this literature review, however, we must also pay close attention to when individual scholars date the appearance of the Semites in Mesopotamia. This is absolutely necessary because it also sheds light on a much broader range of issues, including the degree of credibility we should attribute to the hypotheses that Lloyd most emphatically rejected in the previous quote: namely, that the proto-Semites, the so-called "Pro-Akkadians," might have preceded the Sumerians in Mesopotamia.

As early as 1940 (Die älteste Geschichte Vorderasiens), F. Hrozny took a position in favor of an early, Ubaid-era immigration of a group of Sumerians. By carefully comparing the findings known at that time, he came to the conclusion that the Sumerians originated from the Transcaucasian region and belonged ethnically — if not entirely, but largely — to the Subar/Subir peoples. All signs indicate — he writes — that we must reckon with two major waves of Sumerian immigration in Mesopotamia: the first Sumerian group developed the Ubaid culture (in modern usage, the Eridu-Ubaid culture), the second the Uruk culture. The two most important Sumerian dialects, the so-called The relatively significant differences between eme-ku and eme-sal are explained by the separate language development of these two waves, separated by many centuries and immigrating at different times. That is, Hrozny noticed as early as 1940 that 8,425,191 primary element-producing cultures moved from the core area, from the mountainous peripheral regions of the "fertile, fertile, moon", to the plains.

5. P. According to Tolstoy (Auf den Spuren der altchoresmischen Kultur) El6-The original inhabitants of Asia were the Sumerians, Subarians and Hurrians. These peoples, which belonged predominantly to the East Mediterranean and Proto-Asiatic-Caucasoid groups, created the most ancient Mesopotamian and small-700

===== PAGE 690 =====

Asian cultures. The Semites appeared only thousands of years after the peoples of Syria, Mesopotamia, and Palestine. A. Moortgat, one of the most well-known experts on Sumerian culture, in his study "Grundlagen und Entfaltung der sumerisch akkadischen Kultur", speaking about the immigration of the Sumerians, emphasizes that although the Sumerian nature of the Ubaid culture cannot be definitively and categorically proven, we cannot claim that it was not Sumerian either. In any case, he notes, the Ubaid culture passed without a break into the Uruk culture, and the latter is indisputably a part of the Sumerians. In the Jemdet Nasr period following the Uruk period, several changes can be observed, he continues, and these are perhaps signs of the first appearance of the Semites, but their presence in this period (3200-3000 BC) cannot yet be concretely proven.

H. Schmökel repeatedly states in several of his works (cf. e.g. Schmökel 1957, 1962) that the territory of later Assyria was not inhabited by Semites in the middle of the 3rd millennium, the population was Subar-Hurrian. We can read the same thing in WvSoden, in his study written for Volume II of the "Propyläen Weltgeschichte" (Der Nahe Osten im Altertum). In HWS Saggs' book "Mesopotamien", where he states that the carriers of the Uruk culture were certainly the Sumerians, but in the case of the Eridu-Ubaid civilization, research has not yet been able to definitively decide whether its population was Sumerian, although certain signs - e.g. the identity of temple architecture and cult in the Eridu-Ubaid and Uruk periods, etc. - testify that there was no ethnic change in Mesopotamia at the beginning of the Uruk period. The immigration of the Semites - Saggs continues - begins in the Jemdet Nasr period, but for a long time only on insignificant scales: the newcomers soon merged into the indigenous population. They only appeared in greater numbers around 2600, but even then we cannot speak of an Akkadian invasion, only of an increasingly large infiltration.

In his summary treatise for the "Fischer Weltgeschichte" (Die Ur- und Frühgeschichte des alten Vorderasiens), A. Falkenstein concludes that the Sumerians arrived in Mesopotamia at the latest (1) in the transitional period between the Eridu-Ubaid and Uruk periods (around 3800 BC), but it is very likely that they already formed part of the earliest Eridu-Ubaid period settlers. This probability is largely supported by the archaeological fact that no break in the development of the mentioned cultures can be observed. According to all available data, the settlers who appeared in southern Mesopotamia at the beginning of the Eridu-Ubaid period migrated from the north and north-east, from the area of the Samarra-Halaf cultural circle and the Zagros Mountains to the southern plains as a result of the overpopulation there.

701
===== PAGE 691 =====

In his book "Mesopotamien", JC Margueron takes an extremely cautious position on the issue of Sumerian immigration, and only considers it demonstrable that the Sumerians played a decisive role in the creation of the Mesopotamian high culture that developed in the late Uruk period, and that at that time (i.e. since about 3500) they must have been residing in Mesopotamia in large numbers, so we can count on their immigration in the preceding early Uruk period. However, he then emphasizes that the archaeologically demonstrable cultural and cultic continuity, as well as the mythological traditions of the Sumerians, which go back to the beginning of agriculture and speak of the culture descending from the mountains to the plains, first to Eridu (this was strikingly confirmed by the excavations at Eridu!) all testify to the fact that the Sumerians already participated in the earliest colonization of the southern Mesopotamian plains (cf. Margueron 1970, p. 176).

FR Kraus (Sumerer und Akkader, ein Problem der altnesopotamischen Geschichte) approaches the issue from the linguistic side, and he firmly casts doubt on the main (and we might add: the only) linguistic argument for the previously assumed late immigration of the Sumerians, according to which some Mesopotamian city names and several Sumerian common words are not of Sumerian origin, but rather come from the so-called Proto-Euphratesian language of the Eridu-Ubaid period. Kraus emphasizes that according to the results of recent research, these words can very likely be derived and interpreted from the Sumerian language. It logically follows from this - he writes - that more researchers have recently considered the language of the Eridu-Ubaid civilization to be Sumerian.

P. Amiet In his work "Die Kunst des Alten Orient" he deals with the question of Sumerian immigration on page 56. Some researchers explain the development of the urban high culture in southern Mesopotamia in the second half of the Uruk period by the immigration of the Sumerians, who, in their opinion, assimilated the indigenous population, while adopting many words from their language. However, it is very difficult to draw firm conclusions from the linguistic data available to us. Thus, to the point that this achievement (i.e. the creation of the Sumerian high culture) was essentially the work of the descendants of the indigenous "From the uninterrupted continuity of population, the Eridu-Ubaid settlers, we continue, Amiet. Later he emphatically points out that in Eridu the transition from the late Ubaid to the Uruk period is continuous: architectural customs, the conclusion can be drawn "...that the same population continued to live in Eridu, and that it was Sumerian" (1. Amiet 1977, p. 522).

S, Lloyd, the modern Nestor of Near Eastern archaeology, the explorer of Hassuna, Eridu, "Ugér, etc., etc. (to mention only his most important works), who worked as an active collaborator at many other sites (he discovered, for example, the 702
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world-famous praying figures at Tell Asmar in Diyala), 6 ~ as we have already mentioned - has several decades of direct experience in pre-Asian archaeology, in his work "Die Archäologie Mesopotamiens" he summarizes and evaluates in a very instructive, open and straightforward manner the question of the alleged late > Uruk period -- migrations of the Sumerians. The position of this Near Eastern archaeologist, who is in every respect one of the most authoritative, is presented in the aforementioned book "Vorgänger der Sumerer" (The Ancestors of the Sumerians). We quote the sub-chapter in its entirety due to the author's expert weight and the eminent importance of his definitive statements, which are repeatedly supported by archaeological facts, so that our readers can become familiar with the real state of this paleoethnological and especially paleolinguistic question in all its contexts, which, as we will see, is rooted only in the ideas of certain linguists and has been artificially inflated into a "problem" by them alone. In the vast majority of summary works dealing with the prehistory of Mesopotamia and Sumerian culture, we can hardly read anything about these questions. Almost all works in this direction

He is content with simply stating that the presence of the Sumerians in Mesopotamia is generally reckoned from the Uruk period onwards - sometimes even mentioning that "proto-Akkadians" may have inhabited Mesopotamia as early as the Eridu-Ubaid period. So let's see Lloyd's text.

, Let us return finally to the question of the origin of the Sumerians, which has caused so many puzzles to science in the past. The thoughts of linguists are striking here. were at odds with the conclusions of archaeologists. In short, the problem arose from the uncertainty as to when in the pre-Sumerian dynastic periods we could call Southern Mesopotamia "Sumerians". Some thought that it was the innovations of the so-called "early historical period" (the second half of the Uruk period; - our note) to which Mesopotamian culture owed its characteristics, which it then preserved throughout its long history. This was considered at the time a strong argument in favor of the thesis that the Sumerians had migrated into Southern Mesopotamia at the beginning of the said period. The opposing position of linguists in this context was largely based on a single assumption: that the names of some Mesopotamian cities mentioned in historical texts, founded before the "early historical period", were not of Sumerian origin. This then served as justification for a whole web of speculations about hypothetical migrations of earlier, possibly Semitic, peoples through the Near East. Archaeologists, on the other hand, pointed out the uncertainty of this assumption and the lack of evidence that these cities were actually called that way before the invention of writing. But archaeologists have not yet

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They are also influenced by the overwhelming amount of evidence that attests to the cultural continuity of the 'Obad and Uruk periods. The evidence for the Inuit religious, belief, and practice alone is completely lacking in three of the sites mentioned above (this is obviously a typographical error, for Lloyd lists four sites below; - our note) provides very strong data in this respect. In both El-Obad, Uruk, 'Ugér, and Eridu the chronological strata begin with the usual 'Obad settlement, and as in Eridu, so in the others we can assume that the first two (i.e. three; - our note) settlements each had a 'Obad temple, always rebuilt on the same site. When the 'Obad period ended, Bila Takossag abandoned the settlement in both places (i.e. in Ubaid and 'Ugér; for there is no hiatus in Uruk - our note).

Not so the temple: during the "early historical period", as well as during the early Sumerian dynasties, it was rebuilt once more, and in Eridu the ruins served as the foundations of the ziggurat built by the kings of the III. Ur dynasty. And in addition to all this, both in "Obad" and "Ugér the old "Obad-era settlement mound was used as a cemetery by the Sumerians of the dynastic period.

If we take this situation into account, the hypothesis that the indigenous population was displaced by foreign tribes becomes quite unlikely. One last question remains, which concerns the original origin of the first settlers of southern Mesopotamia. In the meantime, a minor sensation has been caused by the discovery of a flourishing 'Obedite settlement group in the south, on the Arabian coast of the Persian Gulf, extending a little further west, inland, at the height of the island of Bahrain. The first appearance of the Sumerians in this region is now dated to the middle part of the 'Obedite period. However, it has recently been shown by the neutron bombardment method that these settlers actually imported the pottery they used from Sumer. Consequently, the population of the settlement group must have consisted of Sumerian colonists or traders. It seems that with this discovery any speculation regarding a 'former homeland' of the 'Obedite culture is now definitively unfounded. (1. Lloyd 1981, pp. 78-79)

It would be advisable to summarize some of the most important scientific-historical archaeological facts from Lloyd's above lines and carefully note them down for ourselves.

1. The assumption of a relatively late immigration of the Sumerians was from the outset only a linguistic hypothesis, which was not supported by the specific finds.
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018026 was in blatant contradiction with the conclusions of archaeologists based on scientific evidence almost from the beginning of the excavations,

"2. This purely linguistic hypothesis was far from being stable even from the point of view of linguistics itself, because it relied solely on the unprovable, a priori assumption that some 681 Mesopotamian city names were not Sumerian.

3. From this highly improbable linguistic hypothesis, far-reaching prehistoric conclusions were immediately drawn, primarily in the direction that, "in Mesopotamia, even before the Sumerians, peoples of presumably Semitic origin" presumably "migrated here and there," although even the linguists themselves did not claim - because they could not claim with common sense - that the allegedly non-Sumerian city names in question were, perhaps, Semitic names. No linguistic parallels provided any basis for such a claim.

4. And they did all this despite the fact that archaeologists had at their disposal an "impressive amount of evidence", and this mass of data demonstrated the cultural and cultic continuity of both the Eridu-Ubaid and Uruk periods.

5. On the other hand, the presence of Semites even in the Jemdet Nasr period (between 3200-3000) cannot be proven, it can only be assumed with a certain degree of probability (l. above the findings of Moortgat, Schmökel, Soden, Saggs).

After that, we can draw our own conclusions. We have seen that the listed, internationally recognized, authoritative orientalist, paleontologists and other experts (i.e. not only the archaeologists excavating in Mesopotamia, who — as is clear from Lloyd's lines — have already clearly rejected the claims made by some vocal linguists for several decades)

(the hypothesis of a late Sumerian immigration, forced on the basis of "principle considerations") has meanwhile become increasingly

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Under the weight of the evidentiary power of the growing archaeological material, they are increasingly coming to the conclusion that the appearance of the Sumerians in Mesopotamia was practically simultaneous with the appearance of the Tegelso-Bridu-L Ubaid 1st century settlers in the second half of the B millennium. Of course, we cannot rule out the possibility that —,

smaller or larger immigrations to Southern Mesopotamia could have taken place in later times (e.g. the appearance of a larger group from the Halaf cultural circle around 4800 is very likely: let us remember the strong Halaf parallels of the Haggi Mohammed ware), however, according to the evidence of the continuous, unbroken cultural development shown in the archaeological finds, these did not cause significant ethnic shifts, obviously not because their ethnic groups came from the same Samarra-Halaf region as the first settlers themselves, and therefore they could have been more or less related to them.

705

===== PAGE 695 =====

On the other hand, we must also take into account a constant slow infiltration during the Eridu-Ubaid period, since the closest opportunity to channel the population growth of the Samarra-Halaf areas was in Southern Mesopotamia: there, with the construction and expansion of more than 4 canals, the cultivable land

The area could also be increased, 5 and the newcomers could — at least initially — find a livelihood. However, large-scale immigration seems to have ceased with the beginning of the Eridu-Ubaid 3 period: at this time the direction of migrations had already reversed, which is reflected in the rapid expansion of the northern Ubaid culture.

Thus, according to the data available to us, the ethnogenesis of the Sumerian people took place in the first two periods of the Eridu-Ubaid period, and by the beginning of the Northern Ubaid period, the process of gradual ethnic integration of the various Samarra-Halaf groups that had previously immigrated had long since been completed, and a unique Sumerian ethnicity had emerged, distinct from the surrounding peoples, with its own characteristic culture, civilization, and independent cosmological, religious, and mythological ideas.

This new archaeological situation, however, means that our previous discussions in this direction, during which in our previous volumes - consciously taking into account the views generally spread in public opinion - we have only spoken about Sumerians in Mesopotamia since the beginning of the Uruk period (c. 3800), must be revised based on the data currently available to us. We must assume with a very high degree of probability that the Sumerian people and language have dominated, and played a role in creating and developing culture in southern Mesopotamia since at least the turn of the 6th/5th millennium, and that the massive effects they had on the surrounding areas since the middle of the 5th millennium (northern Ubaid cultural circle).

That is, the Sumerian cultural and linguistic hegemony existed uninterrupted in southern Mesopotamia for a full three millennia, from about 5000-0601 to 2000-18, and starting in 4500, it exerted a profound cultural and linguistic influence in the central areas of East Asia from Cilicia to Syria, southeastern Anatolia to western and central Iran; that is, in the region of the cultural koine of the northern Ubaid civilization. As we have seen, these massive influences did not cease even during the Uruk -- Jemdet Nasr

period, as a result of which we must proceed from the fact that the uninterrupted dominant radiations of Sumerian culture and language played a decisive role in the development of the cultures and languages there for about 2,500 years (from 4,500 to 2,000).

A "Proto-Akkadian" theory

Here we must also remember the research trend in Assyriology, whose ideas we have already encountered several times above. This

706

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This topic would actually belong to the chapter on the history of research (l. later, Chapter 6), but since it is inseparably connected with the previously discussed Sumerian problem, i.e. with the assumption of the alleged late appearance of the Sumerians in Mesopotamia, which — as follows logically from the following — is first and foremost voiced by the representatives of this school, it seems necessary to familiarize ourselves with it now, immediately after the presentation of the Sumerian question.

A The scientific roots of the "proto-Akkadian" theory go back to the 1870s, and can be identified with the name of the Parisian Semitologist J. Halévy. Halévy — openly and openly referring to the view laid down in the Bible and generally accepted until the discovery of Sumerian culture, according to which the beginnings of human civilization were connected to the Semitic people and language family — categorically denied for decades, starting from 1874, the former existence of the Sumerian people and language, and interpreted the Sumerian cuneiform texts as the secret language of the Assyrian-Babylonian priests, as an allographic writing system (more in Chapter I. on the history of research, where we also deal with other aspects of Halévy's periodic success). However, excavations already in the early 1880s proved indisputably that the Sumerians they really existed, and even greatly preceded the Semitic Akkadians in Mesopotamia. Thus, Halévy's theories proved to be unfounded, and were for the time being removed from the scientific debate stage.

Since the 1930s, however, the increasingly prominent view of Halévy's former people and language, which has been observed again among some Assyriologists, could naturally no longer be denied outright "The Sumerian Renaissance". in our century in the manner of Halévy, so the methods also changed, but the goal — as we will see later — remained the same: namely, to prove the Akkadian cultural priority in Mesopotamia at all costs.

In accordance with the given situation (i.e. the fact of Sumerian culture), the supporters of the "proto-Akkadian" theory were therefore forced to postulate two organically connected, somewhat mutually complementary hypotheses. On the one hand, they claimed that the Sumerians were not indigenous to Mesopotamia, but had migrated there relatively late, according to some in the Uruk period, and according to others only at the beginning of the Jemdet Nasr period. In support of this assumption — as we already know from Lloyd's lines quoted above — they could only refer to the fact that some Mesopotamian city names were not of Sumerian origin according to some linguists. It should be noted that these linguists were naturally representatives of the school in question, and their argument was not at all convincing (cf. Kraus's opinion presented earlier, book 1, p. 702). On the other hand, they assumed that "possibly" already in the Ubaid period -- that is, the Sumerians also used this school in the Uruk or Jemdet Nasr era.

707

===== PAGE 697 =====

before the alleged immigration of the Akkadians - they "could have been" Sumerians, i.e. "proto-Akkadians" in Mesopotamia or its immediate vicinity, and we should evaluate the serial Sumerian parallels found in the Akkadian language as their linguistic effects; in other words, the mass of Sumerian-Akkadian dictionary collocations and correspondences would be predominantly loanwords of proto-Semitic origin in Sumerian. They then try to demonstrate this on the basis of the sound laws and with the help of the already well-known "linguistic historical analyses". These views have been appearing to a greater extent since the 1950s and 1960s, now forming a school - although such efforts can be observed earlier, e.g. V.

Christian in his 1932 study (l. later, book 883. p.). The "proto-akkadian" idea was first introduced to the public, as far as we have been able to investigate, by S.

It was first raised by N. Kramer in his work "Geschichte beginnt mit Sumer" (1959). It is possible, he writes, that Semitic hordes may have been nomadic in the area of Mesopotamia even before the appearance of the Sumerians and may have periodically migrated through Mesopotamia, herding their flocks.

However, due to the eminent importance of the question, we must again emphatically point out that these ideas do not represent the general opinion of international research, nor even the majority. On the contrary, they are held by only a small minority. In the previous sections, we have already dealt in detail with the recent results of Mesopotamian archaeological research in this direction, and we have shown that the presence of the Semites in Mesopotamia cannot be definitively dated even at the beginning of the 3rd millennium, while the Sumerians have been demonstrably living in Mesopotamia since at least the first half of the 4th millennium. We also saw there that the hypothesis of proto-Semites who may have preceded the Sumerians was only raised by some linguists, while the most authoritative archaeological experts have roundly rejected and refuted these views from the very beginning. (l. above, pp. 698-706).

In other words, this school, which can best be called "finger Levitism" - with its "sound law, evidential" procedure and its argumentative methods that ignore archaeological facts - is in no way different from the well-known "ahistorical" direction of Indo-Germanic studies, which is also merely

Based on his own pre-formulated thesis of cultural priority and the laws of sound, he uses the same methods to Indo-Germanize proven | non-Indo-Germanic languages, and even practically unknown idioms

ig, not even taking into account the recent archaeological and linguistic discoveries. \ The Pre-Asian cultural and linguistic koine Taking together the archaeological and cultural history data we have known so far, | let us now briefly review the current state of our knowledge of the cultural development of the Near East. \ 708 |

===== PAGE 698 =====

Khan could outline his horizon, his outlines becoming more and more distinct in the world of ever-increasing research results.

We have seen that after the consolidation of Neolithic cultures in numerous separate foci and the expansion of the Neolithic zone into large, contiguous areas, approximately after the beginning of the 6th millennium, the impulses that determined further cultural development gradually, but in an increasingly clear manner, increasingly concentrated on the central areas of Near East: Mesopotamia, Syria and the western part of Iran.

The very first culture that appeared relatively unified over larger areas developed in this region at the end of the 6th millennium: the (Samarra-) Tell Halaf cultural circle, which arose through the mutual interaction of the Samarra and Halaf groups, and finally through the dominant expansion of the Halaf culture into the former Samarra areas. In other words, from the consolidation of this cultural complex, i.e. from the turn of the 6th/5th millennium onwards, we can rightly speak of considerable cultural uniformity in the aforementioned region.

This cultural homogeneity, as well as the find complexes testifying to the relatively highly developed cultural conditions: in several places the significant processing of copper, lead, gold and silver, the settlements that were already distinctly urban in nature (respectable defensive walls, ramparts, church buildings), the great ceramic production and decoration

In view of its achievements, the extremely advanced textile production, the mass use of seal presses, etc., many researchers have concluded that at the time of the flourishing of the Halaf culture (between approximately 5000-4500 BC), state formations of considerable importance must have already existed in Northern Mesopotamia and Syria (cf. Schachermeyr 1979, 14-15. old.)

Under such circumstances, however, we must also assume with a very high degree of probability that, as a linguistic projection of this cultural homogenization and the also highly probable state-like formations, a considerable degree of linguistic equalization must have occurred in the area of the Halaf cultural circle between the originally apparently different idioms of the tribes and smaller tribal groups; that is, a more or less intelligible communicative language — lingua franca — also developed, which, however, was undoubtedly based on the dialects of the two most active groups of the cultural circle, primarily the Halaf and secondly the Samarra ethnic group, and consisted mainly of a mixture of these, but with a certain degree of Halaf dominance - considering that after all, the Halaf culture also spread in the former Samarra areas.

This linguistic state, by analogy with archaeological nomenclature, can most appropriately be called the (Samarra-) Halafian linguistic stage, and the period of its general use can be placed between ca. 6. 5500/5000-4500 BC. The boundaries of its distribution are the core areas of the Halaf cultural circle, i.e. the Syrian coast of the Mediterranean Sea and Cilicia in the west, and the Diyarbakir circle in the north-709

===== PAGE 699 =====

and Lake Van, to the east the Zagros Mountains and the Kermanshah region, and to the south the heights of Baghdad, the middle section of the Euphrates to the great bend of the river (east of Aleppo), and from there back to the Syrian coast.

At the same time, however, we must also reckon with the relatively uniform separate language of the Eridu-Ubaid 1-2 settlers in southern Mesopotamia, and even there at a much more developed level than in the north, since we already know that the establishment and continuous maintenance of irrigated agriculture is unimaginable without effective central control and common understanding. The bases of this southern linguistic equalization were obviously the Samarra dialects of the first settlers of the Eridu-Ubaid 1 period, which were then expanded with a considerable amount of Halafi elements in the Eridu-Ubaid 2 period (cf. the early Eridu-Ubaid civilization with its initially mainly Samarrafi and later Halafi connections; 1. above, p. 689). Thus, even at that time, when the early Eridu-Ubaid linguistic community in southern Mesopotamia was still relatively insignificant compared to the contemporary Halafi, limited to a much smaller area, they must undoubtedly have contained many common features, given that both ultimately arose through the mutual equalization of the smaller tribal languages of the Samarra and Halafi groups; only the northern linguistic community may have been based more on Halafi, and the southern primarily on Samarra dialects.

Starting from the middle of the 5th millennium, in roughly the same former Samarra-Halaf areas, but already including Southern Mesopotamia, and extending to the entire western half of Iran and a further strip of Southeastern Anatolia (up to Malatya), we find the Erida-Ubaid 3-4 cultural koines, in the northern region called the Northern Ubaid cultural circle, in which, however, Southern Mesopotamia clearly plays the leading role. At the same time, the find complexes of this cultural circle testify to a much greater degree of cultural equalization than can be demonstrated in the previous Halaf period. In other words, in the case of this civilization, we are talking about the cultural dominance of the extremely culturally active Eridu-Ubaid 3-4th century southern Mesopotamian population -- which, according to all available archaeological data, was already Sumerian long ago -- in a culture that spread far and wide: this culture was everywhere superimposed on various smaller and larger local cultures, and even on the considerable Halaf cultural circle, and unified the cultural image of the aforementioned vast region. Later, when we introduce the more distant extensions of the Eridu-Ubaid culture, we will see that this almost incredible level of dynamism was primarily due to the demographic pressure of the enormous population growth resulting from the developed irrigated agriculture of the southern Mesopotamian plain, as well as the raw material-poor southern Mesopotamia.

710
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a constant and ever-increasing diversity of cultures that are becoming more differentiated

_hyers can be explained by its demand for materials (metals, wood, precious stones, etc.): it can be shown point by point that the characteristic late Ubaid and then Uruk period find-complexes (ceramics, seal stamps, clay figurines, etc.) from Eastern Iran to the Aegean region, and even as far as Transylvania, are almost always parallel to the contemporary appearance and flourishing of metalwork (1. below, in the next chapter).

However, we have repeatedly pointed out that the development of the Eridu-Ubaid civilization and its expansion to the whole of Southern Mesopotamia can only be imagined within the framework of effective central control, a state-like power organization covering larger areas, which also presupposes a common language. Well, this civilization spread after 4500 throughout almost all of East Asia, and with it the language of the bearers of the Eridu-Ubaid culture undoubtedly became the dominant "lingua franca" in the aforementioned vast area.

This linguistic state represents the late Eridu-Ubaid linguistic stage of the Proto-Asiatic language koine, and can be dated roughly to between 4500-3700/3600 BC. However, considering that the population that developed and carried the Eridu-Ubaid culture was very likely Sumerian or Proto-Sumerian (in the sense of the term "proto-Hungarians"), it seems more correct to speak of a late Eridu-Ubaid Proto-Sumerian (pre-Sumerian) linguistic stage.

Then, starting around 3700/3600, the expansion of the already demonstrably Sumerian Uruk culture can be detected from archaeological finds, which proceeded in the same way as the former in all respects, and clear signs of its massive dominance are also present from the northeastern coast of the Mediterranean to Western Iran (Uruk-Gaura culture). The lingua franca of this cultural koine was undoubtedly Sumerian, and the Sumerian language remained the dominant "lingua franca" of East Asia, the "international" cultural, cultic and commercial language, until 2000 BC. The name of this linguistic state would therefore be the Uruk-Gaura period Proto-Sumerian linguistic stage, ca. BC. starting in 3700/3600, i.e. roughly since the earliest beginnings of pictographic writing, the invention of which modern research places in the middle of the 4th millennium (cf. above, p. 685).

In these paleolinguistic questions, however, we must pay close attention to another very important circumstance, which we have already touched upon. Namely, the fact that the colonists who appeared in southern Mesopotamia at the beginning of the Eridu-Ubaid period, according to all available archaeological and cultural-historical data, came from the area of the Samarra-Halaf cultural circle and migrated south as a result of the overpopulation there (cf. above, p. 675). Let us recall that earlier canal systems were observed in the settlements of the Samarra culture (Tell es-Sauwan,

711
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TSoga Mami; 1. pp. 675-676), i.e. the population of this culture first reached the technical level with which they could already begin the agricultural exploitation of the dry southern Mesopotamian plain. It is striking that the settlement of the Elamite plain (Susiana) from the southern Luristan region, not far east of TSoga Mami, from the region of Tepe Guran, Ali yyy, and the spread of irrigated agriculture there began at about the same time. The already mentioned close parallels of the earliest Elamite pottery (Susiana a and b wares) with the pottery of the Eridu-Ubaid 1 and 2 periods indicate very close connections (cf. e.g. Narr 1975, p. 153 and Hrouda 1971, pp. 69-70).

All these data therefore make it clear that the lingua franca of the late Eridu-Ubaid period developed from the linguistic community of the early southern Eridu-Ubaid period through a direct development. Sumerian linguistic stage in question already originally contained many common elements, but it is significantly modified by the northern (Samarra-) Halaf period. Thus, the spoken language of the pre- because this makes it very likely that there was a very significant difference between the spoken languages of the (Samarra-) Halaf and the late Eridu-Ubaid period.

We must reckon with linguistic connections and/or cross-overs, i.e. a certain degree of ~ at least partial — paleolinguistic continuity.

The same is true of the Uruk-era "lingua franca" - certainly Sumerian - and the late Eridu-Ubaid-era linguistic stage - even if we were to conditionally accept that the southern Mesopotamian population of the late Eridu-Ubaid period did not speak Sumerian. For if the Sumerians had indeed only migrated to Mesopotamia around the beginning of the Uruk period, we would not be able to find their previous homeland anywhere other than somewhere in the immediately adjacent northern or northeastern mountainous region: in northern Mesopotamia, in the Armenian Highlands or in the Zagros region; so in the end, it would be the older Samarra-Halaf, then northern Ubaid areas.

Here we note that the earlier ideas that the Sumerians mostly migrated to Mesopotamia from distant regions (e.g. Central Asia or India, etc.) have now lost all credibility as a result of the demographic considerations and cultural-historical facts to be explained in detail below. In the meantime, it has become clear that the Sumerians — no matter when they migrated to Mesopotamia — could only have arrived from the immediate vicinity, because at that time there was no such advanced culture anywhere further away as the people of Mesopotamia had demonstrably possessed from the very beginning...

Thus — y the immigration of the Sumerians during the Early Uruk period — the Sumerian language in this case must have originally contained many elements from both the Halaf period and from the spoken language of the late Eridu-Ubaid period. That is, all signs indicate that all three

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linguistic stage (Halaf, late Eridu-Ubaid, Uruk) - no matter how we look at the question + they were in a very significant paleolinguistic connection with each other.

However, as we have shown, based on the clear evidence of archaeological finds, it is practically certain — to the extent that we can speak of any certainty at all in such ancient times — that the bearers of the Eridu-Ubaid civilization were Sumerians (pre-Sumerians).

From all this it follows that the existence of three consecutive, but more or less interconnected, widely used, commonly understood, lingua franca-like communicative languages — or linguistic stages — in the central areas of East Asia, at the focal point of the cultural development of the Chalcolithic and then the Early Bronze Age, roughly in the region between the Mediterranean Sea and the Zagros Mountains, starting from the end of the 6th millennium BC, is in all probability much more than a mere hypothesis.

713
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ETHNO-CULTURAL RADIATIONS OF THE NEOLITHIC AND EARLY METAL AGE PRO-ASIAN CIVILIZATIONS IN WESTERN EURASIA

In the following pages we will discuss in more detail the cultural influences and migrations originating from East Asia, which we have already briefly summarized in Chapter 1 of our Book I. The subject matter of this chapter is therefore not new to our readers. The reason why we are returning to these questions is primarily that these migrations have an eminent significance in the cultural, ethnic and linguistic development of the whole of Western Eurasia, to such an extent that their recognition would at once render untenable all the prehistoric and paleolinguistic ideas that European science had hitherto represented during the 19th and 20th centuries. The fact of these mass migrations creates a fundamentally new situation in the field of prehistory and linguistic studies, because it proves that the development of almost the entire Western Eurasia — not only in cultural terms, but also in ethnic and linguistic terms — is largely a function of East Asia. Secondly, it is necessary to bring up these migrations again and present them in more detail, because in this way, all those who have carefully followed the explanations of our previous studies will now find it much easier to navigate the complex connections that arise, and thus we can, as it were, set out on the path together, together we can uncover the most important guiding threads of the former cultural, ethnic and paleolinguistic relations of Western Eurasia.

The Anatolian Neolithic civilizations and the emanations of the Hassuna-Samarra-Halaf cultural circle
As we have seen above, the food-producing cultures that developed in the Middle East roughly during the 9th-8th millennium BC already formed a connected cultural zone in large areas by the 7th millennium BC, from Western Anatolia to the Zagros Mountains, practically spreading to the entire regions of East Asia that could be cultivated without irrigation. Also during the 7th millennium ~ after sporadic beginnings in the 8th millennium (Mureybet, Gang Dareh Tepe) - pottery appeared in many places: in Western Anatolia in the caves near Antalya, in the area of the Konya Plain in Central Anatolia (Catal Hüyük), in Cilicia (Mersin, Tarsus), in the Amug Plain, in Syria (Ras Samra), in Palestine (Jericho, Beidha), and in the Iraqi (Tell Bugras, Jarmo) and western Iranian parts of the Zagros Mountains (Tepe

714
===== PAGE 704 =====

Guran, Tepe Sarab, Ali Kos, etc.), and even on the southeastern coast of Käspi-H6 (Ghar-i Kamarband, Hotu Cave).
This is the earliest, still very crudely worked, coarse-grained pottery with an uneven blackish-reddish 10108 1 ware, usually called Altmonochrom (synonymous names in the English-language literature: archaic burnished, neolithic burnished, archaic coarse, dark-faced burnished). Towards the end of the 7th millennium, the beginnings of pottery painting can also be observed, in the form of hastily made zigzag lines,

The connections of this most ancient pottery have not yet been clearly clarified. However, it is certain that on the one hand the painted ömonochrome ware of the more western sites (Karain, Beldibi, Belbasi, Catal Hüyük, Mersin, Amug A, Ras Samra) and on the other hand the oldest painted pottery of the Zagros region (Jarmo, Tepe Guran, Tepe Sarab, Ali Koş) form an organically related group, which, however, are also to a certain extent interconnected (cf. e.g. Hrouda 1971, pp. 45-47). In the intervening northern Mesopotamian areas, pottery appears somewhat later, at the end of the 7th millennium. Exactly corresponding to this temporal shift, the ca. The forms of Ömonochrome ware dated to 6100 BC show parallels with the younger stages of development of Anatolian and Cilician pottery (Hrouda 1971, p. 44).

In the same period, starting from the second half of the 7th millennium, parallel to the general spread of ceramics, the improvement of their quality, and the enrichment of their decoration, a rapid cultural development can be observed at almost every site. Instead of the earlier, rather miserable one-room huts, we now find real, solidly built houses, often consisting of several rooms (1. e.g. the floor plan and reconstruction of the manor house from the Hassuna IV layer, dating from about 5600, Hrouda id. m. p. 46).
Amulets appear — including those in the shape of double axes and bulls' heads, which later became widespread — seal stamps, characteristic painted terracotta figures, mainly depicting female figures — which are also found throughout the Near East region — and multi-colored wall paintings and wall reliefs (Catal Hüyük, Can Hasan).
Likewise, the beginnings of metalworking can be observed in the entire region in question as early as the 7th millennium: cold-hammered copper tools in Cayönü around 7000, melted copper ingots in Suberde around 6750, copper beads in Ali Kos in the middle of the 7th millennium, cast ýýý and lead beads in Catal Hüyük, etc.
Also starting from the turn of the 7th/6th millennium, the gradual cultural equalization of the central regions of Near East (Northern Mesopotamia and the directly adjacent areas) becomes apparent, as do the spread of characteristic find assemblages of certain regional archaeological cultures, which were initially still clearly distinguishable, over larger areas. During this process, in the first stage, from the 6th year to 715

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In the first half of the 1st millennium BC, the already painted Hassuna pottery (from the Hassuna Ib layer upwards) became uniform almost throughout the whole of present-day Northern Iraq. Its area almost exactly covers the distribution zone of the pottery and other typical finds of the Samarra culture (ca. 5600-5000) that was directly connected in time, and the Samarra ware itself appears as an organic development of the Hassuna forms, as a result of which many researchers speak of a Hassuna-Samarra culture or period. We have already mentioned that, roughly simultaneously with the Samarra culture, around 5500, slightly further west, in northern Syria, southern Celtic Anatolia and the westernmost regions of Iraq, the Tell Halaf culture developed, which then expanded to include the cultures of the entire mentioned region around 5000, to the extent that the Halaf forms became dominant everywhere, superimposed on the deeper layers of the settlements of the surrounding cultures (Samarra, Western Syria, the Amug plain, Cilicia).
In line with the large-scale cultural boom outlined above, since the turn of the 7th/6th millennium, the very first wave of colonization that crossed the narrower borders of East Asia can be clearly demonstrated archaeologically: the westward expansion of the Anatolian Hacilar — Catal Hüyük — Can Hasan cultural circle, which was at the forefront of Neolithic development according to our current knowledge, towards the Aegean and southern Balkan regions.
The earliest European Neolithic settlements are found in Thessaly at the beginning of the 6th millennium (Nea Nikomedea, proto-Sesklo). All their pottery varieties are deceptively similar to the early Hacilar ware (around 5900). In the slightly later Magulica stage, on the one hand, we find red, glazed pottery, which in turn parallels the ware of the middle layers of Hacilar, but also the so-called impressed pottery appears, in which the decorative patterns were usually pressed into the walls of the vessels with the shell of the cardium (heart shell) (cardium pottery), and sometimes simply with a nail (nail-impression pottery). The earliest occurrences of this decorative method (around 6000) are known from the Syrian coast and Cilicia (Ras Samra, Byblos, Mersin, Tarsus), and it evidently spread from there to Greece in the middle of the 6th millennium, and not much later, step by step, to the coastal regions of almost the entire Mediterranean region: to the Dalmatian and Italian coasts of the Adriatic, to Sicily, to southern France, and finally to Spain, with the groups of the Cardium pottery culture (cf. e.g. Müller Karpe 1968, p. 271).

In the following period, at the end of the 6th millennium (the classical Sesklo stage in Greece), the red painting of the pottery remains, and the anat6-ýý influences, parallels with the upper layers of Hacilar in particular, continue to dominate (see, e.g., Schachermeyr 1976, pp. 47-48).
The first Neolithic colonists also appeared on the island of Cyprus around 6000. The population came from northern Syria or Cilicia (cf. Propyl. Kunstgesch. XIII. kot. 57, p.).
716
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Neolithic culture also spread to Crete from East Asia, also at the very beginning of the 6th millennium. Most parallels point to the Cilician origin of the settlers (1. e.g. Schächermeyr 1979, p. 51; Platon 1968, p. 137).
The Neolithic colonization of Crete, however, proves the surprisingly early beginnings of deep-sea navigation. But the same is supported by the fact that the raw material of the large number of obsidian tools found in the Cretan Neolithic settlements, according to mineralogical studies, undoubtedly came from the island of Melos from the very beginning. Moreover, a number of Melosian obsidian objects were found among the Mesolithic finds from the 8th-7th millennium BC in the Franchti Cave in the Peloponnese (both data are given in 1. e.g. Propyl. Kunstgesch. Vol. XIII. p. 60.; Narr 1975, p. 60.).

Starting in the second half of the 6th millennium, new migrations, this time involving much larger masses, began from the western part of East Asia, again involving mainly Anatolian settlers, but also to a lesser extent northern Syrian settlers. These colonists made their way through the Bosphorus and (or) the Dardanelles, and by the end of the 6th millennium they had practically invaded the eastern and northern regions of the Balkans, but they also reached the Carpathian Basin plain and Transylvania. They developed the Karanovo or Kremikovci culture in Bulgaria, and the Starcevo-Körös culture in the area of present-day Yugoslavia, southern Hungary and Transylvania (1. Map 3). These cultural circles cannot be derived from the earlier Thessalian Early Neolithic, but are the consequences of direct Anatolian (Catal Hüyük, west*, Hacilar, Can Hasan) influences and immigrations that arrived via Thrace (cf. e.g. Müller-Karpe 1968, pp. 90-91; Propyl. Kunstgesch. vol. XIII, pp. 96-99; Condurachi-Daicovicu 1972, pp. 28-31).

When viewed in the light of these mass population movements, the phenomenon, which is surprising in itself, is easily explained by the fact that most of the Central and Western Anatolian Chalcolithic settlements became depopulated around the turn of the 6th/5th millennium. Thus, life ceased in the flourishing, populous settlements of Hacilar, Catal Hüyük, Can Hasan, and in all these areas of highly developed Anatolian civilizations of the 7th-6th millennium, apart from a few sites — such as Mersin, Tarsus in Cilicia or Beycesultan in Western Anatolia (which latter settlement, however, was probably founded by a part of the population that left Hacilar or the Hacilar cultural circle at the beginning of the 5th millennium) ~ for most of the 5th millennium and the first half of the 4th millennium, there is a lack of cultural continuity, development stagnates, and the population is in all probability very rare: only one or two insignificant, scattered agricultural settlements consisting of a few houses, with a rudimentary culture, have been excavated so far from this one and a half millennium period in the central areas of Anatolia. Only in the second half of the 4th millennium does the cultural development of this region begin again, and its population replenishment, but by then it is already demonstrably
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It came from the east, ultimately as a result of cultural influences and migrations of Mesopotamian-Sumerian origin (more on this later).
On the other hand, with the depopulation of large areas of Anatolia, we can observe an ever-rapid increase in the population of the Balkans and the Carpathian Basin from the beginning of the 5th millennium; agricultural and livestock-breeding settlements are gradually emerging, which proves large-scale immigrations from East Asia — states, for example, Randa already in 1954 (Handbuch der Weltgeschichte), adding that the Aegean region, the Balkans and the southern areas of Central Europe were in the ich. ý ted from Anatolia and the

Agricultural colonists from the Halaf cultural circle area populated the area. Randa's conclusions have since been fully confirmed, only today this process is dated to about half a millennium earlier.

The popular movements that led to the development of the Starévo-Körös culture were therefore no longer limited to Anatolia, but also to the consolidating and expanding regions of the region at that time. The area of the Halaf cultural circle was also significantly affected. In other words, the reason for the migrations should not be sought in Anatolia alone, but the overpopulation of the Samarra-Halaf region, which had reached a high level of development by then, obviously played a significant role in them, which in the second half of the 6th millennium was a real demographic explosion by all indications. The aforementioned westward migrations at the end of the millennium 82 The slow expansion towards Anatolia began at the same time, in the Hassuna period, in the form of Hassuna parallels appearing in some Anatolian settlements. Then in the second half of the 6th millennium, clear Samarra-Halaf influences appear in Anatolia (e.g. in Can Hasan), which reached as far as Hacilar in western Anatolia around 5000: Hacilar II. In the ceramics of the layer (ca. 5000), several Samarra-Halaf characteristics can be observed (cf. e.g. Hrouda 1971, p. 60; Propyl. Kunstgesch. vol. XIII, p. 38). Moreover, even in the finds of the Starcevo-Körös culture, very strong Halaf parallels appear: the female clay figures, the seal presses are almost identical versions of the corresponding finds of the Halaf cultural circle (cf. e.g. Moortgat 1945.; Childe 1949; Heine-Geldern 1950), and close parallels of the characteristic Halaf ceramics can also be found along the Lower Danube and in Transylvania (see Oppenheim 1931).

However, during the same period, the finds reveal the far-reaching expansion of the Hassuna-Samarra-Halaf culture to the south, east and northeast. We already know that the southern Mesopotamian plain was also colonized by settlers from this cultural area; cleinte= the In the 110856 centuries of the 1st millennium -- mainly the Samarra civilization, and in the first half of the 5th millennium, in the yyyýý Mohammed stage, the direct influences of the Halaf culture we could notice (1. 689, p.). 718 ===== PAGE 708 =====

To the east, some traces of the older versions of Hassuna pottery appear in the earliest layers of the Tepe Sialk I period (c. 5500-4600 = according to others 5300-4500) in the central Rann, and not long after, also in period I, the influences of Samarra, and especially Halaf pottery, can be detected, which become increasingly strong in Sialk II (c. 4600-4000) (1. e.g. Hrouda 1971, p. 56).

However, the pottery of Sialk II, mixed with Halaf parallels, soon appears much further north, around Tehran (Ismailabad, Kara Tepe, Chesm-i Ali, etc.), with completely identical forms (1. Amiet 1977, p. 572). Moreover, the earliest Neolithic settlements in southern Turkmenistan in the 5th millennium (Dzeitun or Chagallı culture, Mondjukli period) can also be largely evaluated as northern versions of the Hassuna and Sialk I cultures (cf. e.g. Propyl. Kunstgesch. vol. XIII, p. 168). It seems that with the development of the Samarra culture, starting around 5600/5500, some Hassuna groups moved further east and north, and then when the Halaf culture expanded into the Samarra area around 5000, some of the population of the latter culture also moved from their previous residences and settled further east and north, thus pushing the Hassunai who had previously settled there even further away; thus some Hassunai groups reached southern Turkmenistan at the beginning of the 5th millennium. In order to make the connections clearer, we would like to note in advance that, in parallel with the northern expansion of the Eridu-Ubaid culture (northern Ubaid cultural circle), from about 4500 BC onwards, the Halaf tribal population itself was apparently forced to move further north and east. It is known that the Halaf culture originally (between 5000 and 4500 BC) only extended directly to the Lake Van region (Tilki Tepe) and the Rovanduz area (Gird Banahilik), i.e. it just reached the southern borders of the Lake Urmia cultural group (Haggi Firuz, Hasanlu, Dalma Tepe, etc.). Well, in the second half of the 5th millennium, after the emergence of the northern Ubaid culture in the Halaf areas, Halaf pottery appears at Tegut (near Esmiadzin), at Kultepe near Nakhichevan, and elsewhere in northwestern Azerbaijan (see Burney-Lang 1975, p. 77), and in the east — as we have just seen — in the same period, the pottery of the Tepe Sialk II period shows very strong Halaf parallels.

At the beginning of the 4th millennium, however, the typical ceramics of the Lake Urmia cultural circle, close counterparts of the Dalma ware, appear at /Ilani Tepe in northern Azerbaijan (1. Burney-Lang 1975, pp. 80-81), and even in the Namazga Tepe I period (ca. 3800-3500) in southern Turkmenistan, as well as in the lower layers of Anau of the same age (1. Hrouda 1971, pp. 57-58), after somewhat earlier, around the turn of the 5th/4th millennium, the influences of the northern Ubaid culture increasingly appeared in the core areas of the Urmia culture (cf. Burney-Lang 1975, p. 81).

719 ===== PAGE 709 =====

All this again indicates that the expansion of the northern Ubaid culture, which had been increasingly superimposing itself on the Halaf circle since 4500, also triggered a chain reaction: it pushed the individual groups of the Halaf culture further north and east. In turn, they pushed the population of Urmia-t6 and Sialk, partly of Hassun-Samarra origin, to even more distant areas (Asserbeidsän, Tehran area, southern Turkmenistan). Later, around 4000, the Ubaidists themselves appeared in the more northern areas, e.g. on the banks of Lake Urmia.

Here we interrupt our presentation and briefly refer to the fact, which is not insignificant for our purposes, that the earliest finds of the so-called comb-pit pottery come from Ilani Tepe in northern Azerbaijan, dated to the beginning of the 4th millennium (1. Burney-Lang 1975, p. 81, but also cf. our book I, p. 33), while the comb-pit ware of the 3rd millennium can be traced back to the pottery of Anau and Namazga I (cf. Tolstow 1953). Now, however, we have seen that both the pottery of Ilani Tepe and Anau I and Namazga I ultimately originate from the typical pottery of the Chalcolithic circle around Lake Urmia, and can be considered its later provincial version. It can therefore be assumed with great probability that we must count on two main directions of radiation from this field of culture; one led to the northern regions of Transcaucasia (Ilani Tepe) at the beginning of the 4th millennium, from where comb-pit pottery soon reached the Pontic-Dnieper region (Mesolithic settlements of Iğren, Sulayev, etc. Comb-pit pottery@val-Dövebben 1 later), then in the middle of the 3rd millennium the Transcaucasian population also appeared en masse in southern Russia (so-called "late pit-grave culture"), and the earlier population — partly moving further north — took comb-pit pottery with them to the forest belt. The other route was towards southern Turkmenistan, also at the beginning of the 4th millennium (Anau I, Namazga J), and the Keltiminarians then took over comb-pit pottery from the agricultural cultures there in the first half of the 3rd millennium. their pottery, which they in turn transmitted northward to the eastern regions of the Ural Mountains. The same southern Turkmen civilizations expanded into the Altai-Yenisei region in the second half of the 3rd millennium (Afanashevo culture).

This can clearly explain the numerous striking parallels in the findings of the Afanasyevo and Late Pit-Grave cultures — which in both cases testify to a Proto-Asian origin. —, but most of all, the otherwise incomprehensible anthropological fact that the population of the Afanasyev and late Gödörsfro cultures was several thousand km Despite the distance, we find practically identical varietal characteristics (cf. our book, pp. 29-30 and 41-42, 04). However, from the data just presented, we can also simultaneously become acquainted with one of the very characteristic, almost time-independent features of colonial civilizations of all times: the introduction of settlers to virgin lands by migrants 120 ===== PAGE 710 =====

In the Läban, everywhere and at all times, they preserve the cultural state of their original homeland for centuries, sometimes even for entire millennia, when the stone "migration" has wiped them out. The further cultural achievements of the central areas only reach them with a considerable phase delay, mostly with the arrival of a new swarm of settlers. In this way, concentric circles of culture developed around the Proto-Asian-Mesopotamian core over time, the level of development of which gradually became lower culturally, in direct proportion to their distance from the center, and preserved older intellectual and material-technical traditions. When, for example, at the turn of the 5th/4th millennium BC, the advanced Late Stone Age Eridu-Ubaid 3-4 culture flourished in the areas of Mesopotamia and Syria, civilization had already reached the threshold of the beginnings of urbanization, with large-scale metal processing, irrigated agriculture, advanced division of society, the formation of larger, distinctly state-like organized communities, etc. etc. — until then, the directly adjacent areas were still dominated by the Early Chalcolithic traditions and forms of the Halaf period (Sialk II, Transcaucasia, Eastern Anatolia, etc.), and in the more distant peripheral regions (Northern Iran, Southern Turkmenistan, Aegean, Balkans, Carpathian Basin, etc.) we find the Late Neolithic stage of Hassuna or Hacilar — Catal Hüyük. This situation did not change in the following periods, only the radiations, trade relations and successive colonizations spread to increasingly distant regions, and thus the cultural "steps" between the center and the most distant peripheral areas multiplied, and the difference in culture became ever greater.

We only notice exceptions to this "rule" when the colonists settled in the deposits of important raw materials (ores, precious stones, noble wood species, sought-after spices or other luxury goods). In such cases, there were close and permanent contacts with the mother country from the very beginning, and the "cultural level" was considerably lower; — as we can prove in more than one case, for example, in the area of the azure deposits in Afghanistan (Mundigak — 1. later), or in the areas of Sialk and Tell-i Iblis in Iran, where significant copper deposits can be found, and even in the areas of the Transylvanian and northern Balkan ore deposits (1. below). We can trace this gradual cultural backwardness and conservatism particularly clearly in cases where it is not the cultures of colonists originating from East Asia, but rather the cultural goods taken over by the indigenous population of the northern regions from the neighboring settlers of East Asia, i.e., secondarily created, "induced" cultural circles. Thus, through the above-mentioned example of the comb-pitted ceramics of various Finno-Ugric groups, we can gain valuable cultural-historical insights into the nature and manner of ancient cultural relations.

721 ===== PAGE 711 =====

The 5th millennium Dalma pottery, which corresponded to the late Neolithic and early Middle Bronze Age stage of development and satisfied its needs in every respect, gave rise to the pottery of Ilani Tepe, Anau I and Namazga I, which were at the same cultural level at the time, at the beginning of the 4th millennium. This pottery was adopted in Europe by the Mesolithic groups of North Pontus (Iğren, etc.) in the middle of the same millennium, then by the sub-Neolithic Dnieper-Donetsk culture around 3000 (cf. Müller-Karpe 1968, pp. 165-166), and in Asia in the first half of the 3rd millennium. half of the sub-Neolithic Keltiminar. From these cultures, in the second half of the 3rd millennium, it spread to the former predecessors of the European Finno-Ugric peoples (but not only to them, but also to the Baltics and Scandinavia), and to the fisherman-hunters of the Ob-Ural region, who in turn continued to use it for millennia: namely, until their cultural development greatly exceeded the sub-Neolithic-Neolithic conditions - to which this ceramic type corresponded in all respects.

These would therefore be the real, archaeologically demonstrable cultural-historical connections of comb-pit pottery, of the pottery craft on which our prehistoric research mainly bases the thesis of the Finno-Ugric people and language family (cf. e.g. Fodor 1975, or the introduction to the book in Chapter 3 of our Book II). It is obvious that this pottery — in the light of the above — cannot characterize either a people or a language family, but only primitive, roughly equal cultural states. It was used by the predecessors of the Scandinavians and Lapps, the Balts, the later Slavs, several Paleo-Siberian people groups and — along with them — of course by the ancestors of today's Finno-Ugric peoples. However, we emphasize that

that in the times in question (the second half of the 3rd millennium) no connection whatsoever can be demonstrated between the European and the later "Finno-Ugric" groups of Northwest Asia. The clay of the comb-pitted pottery, which is said to be so characteristic, was thinned, for example, with shell powder in Europe, with soapstone or sometimes with gravel in Asia, and the two methods of production are never mixed: no pottery found west of the Urals mixed with soapstone or gravel is known (cf. László 1961, pp. 183-184). The custom of slimming with shell powder was passed on from the Dnieper-Donetsk culture and the late Tripoli culture to the more northern peoples (cf. Müller-Karpe 1974, 340-350. yyy-).

Let us now review the The further development of Neolithic activities in these areas at the end of the 5th millennium and the first half of the 4th millennium. The 5th millennium is marked by the Starcevo-Körös and its period of consolidation and slow expansion of the parallel Bulgarian Karanovo (Kremikovi) cultures. These cultures -- as we have seen -- cannot be directly derived from the Early Neolithic of Thessaly, but rather from Anatolian (late Hacilar, Can Hasan, etc.) and partly from northern Syria, which arrived via Thrace. CSamarra-Halaf-SYanno had many works - see p. 717).

722

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Around 4000 BC, the Neolithic Bandkeramik or Linearband-KORK culture appears from the Balkans and the Kárpát Basin further north and east, extending from the Netherlands to Celtic Europe. Modern research primarily views the emergence of the Körös culture and the simultaneous Starcevo-facies in the northern Balkans as the creation of agricultural settlers of pre-Asian origin, and on the other hand, it explains it -- to a lesser extent -- with the appearance of new pre-Asian colonial groups. This is evidenced by the massive southern parallels of the finds of the Bandkeramik culture (cf. e.g. Müller-Karpe 1968, p. 272; Propyl. Kunstgesch.

Vol. XIII. pp. 99-100.), The autochthonous European origin of the band-decorated pottery culture -- as some claim (cf. e.g. Pittioni: Der urgeschichtliche Horizont der historischen Zeit; Propyl. Weltgesch. Vol. I) -- there is no evidence. On the contrary, the survival of the Tardenoisian-type Upper Paleolithic -- Mesolithic primitive population (fisherman-hunter stage!) alongside the Neolithic population with ribbon decoration and in larger patches among them (in areas that were not suitable for farming) can be demonstrated in several places for a long time. In addition, the two types of population remained completely isolated from each other (cf. Narr. 1975, p. 327). The culture of ribbon decoration pottery is therefore demonstrably a direct extension of the Starcevo-Körös culture (see Narr 1975, p. 330). This fact, however, clearly

It has also been proven that the agricultural settlers of Pre-Asian origin of the Starcevo-Körös cultural circle also looked towards the more northern regions of Central Europe (cf. our book I, pp. 24-25). We find the same situation in the East. For example, in Western Ukraine, the beginnings of the Neolithic can be first grasped in the so-called Southern Bug culture, and in its lower layers there are close parallels to the female clay figurines, ceramics and other finds of the Körös culture (I. Müller-Karpe 1968, p. 164). From these beginnings, the Romanian-Ukrainian Cucuteni-Tripoli culture developed in the middle and second half of the 4th millennium, also the creation of agricultural settlers from the Carpathian Basin and the northern Balkans. It expanded step by step from the southwest to the east and northeast, as can be clearly seen from the layers of the sites (cf. Müller-Karpe 1968, p. 167). In the cult, customs, and art we find a very significant number of features of Anatolian origin, but anthropological studies also prove the southern, ultimately Proto-Asian origin of the population: the Tripolitarians were partly Mediterranean-Europids and partly short-headed Armenoids (I. Narr 1975, pp. 222 and 225).

However, we have already gone far beyond our current goal, because at present we can only identify the pre-Asian radiations of the Samarra-Halaf period, industrial-723

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We wanted to present the matosfás. From a didactic point of view, however, it seems advantageous if, when discussing the cultural picture of each period, we occasionally briefly refer to the pre-Asian connections of the following era, as we did above when we touched on the influences of the northern Ubaid culture on Azerbaijan and southern Turkmenistan. In this way, the unbroken continuity of the massive pre-Asian influences that can be observed in the cultural development of each area is much more clearly highlighted than in the successive periods. in archaeological periods, and our presentation becomes more transparent, not fragmented into the separately considered connections of each era. Therefore, we occasionally use these brief previews in the following ones, consciously counting on the fact that we will be forced to repeat them several times. The emanations of the late Eridu-Ubaid civilization Let us now consider the Mesopotamian connections in more distant areas after the emergence of the northern Ubaid cultural circle (c. 4500). We already know about the late Eridu-Ubaid civilizations that this was the first cultural koine in the true sense of the word in Near East, directly extending over vast areas, whose direct and massive unifying effects can be seen from Cilicia (Mersin) to Eastern Iran (Tell-i Iblis) (I. p. 690). A thorough examination of the more distant emanations of this cultural circle is of paramount importance, since we saw above that, in the light of the latest results of archaeological research, the bearers of the Eridu-Ubaid civilization were, with a probability approaching certainty, already Sumerians (pre-Sumerians) (cf. pp. 699-706).

We have already mentioned that the Eridu-Ubaid 3 civilization, expanding northward, pushed some groups of the Halaf cultural circle further north and east, starting in 4500. We also became acquainted -- following S. Lloyd -- with the flourishing Sumerian colonial settlements of Eridu-Ubaid 3rd period on the Arabian coast of the Gulf of Pescara. Somewhat later, from the end of the 5th millennium, however, the further expansion of the northern Ubaid culture is already directly evident, e.g. in the very strong Eridu-Ubaid 4 and Susa A (also known as Susa I) parallels of the three lower layers of the Tepe Sialk III period (ca. 4000-3500). 5 these characteristic Ubaid-Elamite features also appear at the beginning of the 4th millennium - already in the Tepe Hissar I period (ca. 4000 onwards) on the southeastern shore of the Caspian Sea in northern Iran. It is important to note that in the graves of Hissar I. period, male bodies clearly dominate: in layer IA, eleven women and twelve men were found, in layer IB, two women and five men, and in layer IC, twelve women and forty-five men (I. Müller-Karpe 1968, p. 540 and Amiet 1977, p. 555). This distribution of finds strongly supports the conclusion that in Hissar I. period

724

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Life there had a typical colonial character; it is well known that in the Gionnan y areas there is a large shortage of women, and we find a predominance of men. This was the case, for example, in North America, This is the case until the end of the 19th century, and in Australia and New Zealand almost to the present day, so the Tepe Hissar settlement was obviously founded by southern colonists at the very beginning of the 4th millennium. In the same period, clear northern Ubaid parallels also appear in the cultures of southern Turkmenistan. We have convincing evidence for direct Mesopotamian connections with Anau I, Namazga I, Karatepe (all between ca. 3800-3500): several female clay figurines, for example, are almost identical to the statuettes found in Mesopotamia of the Eridu-Ubaid 4 period (I. Propyl. Kunstgesch. Vol. XIII, p. 168). As a result of these similarities and several other clear Mesopotamian parallels, modern research considers the aforementioned Chalcolithic cultures of southern Turkmenistan to be the products of colonization from Mesopotamia via northern Iran. However, we saw above that settlers also arrived in southern Turkmenistan from the Chalcolithic cultural circle around Lake Urmia, which is mainly reflected in the parallels between the pottery of Anau I and Namazga I and the wares found at Dalma Tepe. Moreover, these 4th millennium settlers had already found earlier Hassun, Samarra and Sialki groups in Turkmenistan (I. pp. 718-719), and they evidently mixed with them. It is clear from this that in these colonizations originating from East Asia we are often faced with very complex, multi-level layered ethnic and cultural processes, the systematic exploration of which is still in its early stages.

According to recent studies, the first Mesopotamian settler groups reached Turkmenistan in the first half of the 4th millennium. They were followed around 3000 by a second wave of colonization, this time on such a large scale that concrete evidence of this can be found in the anthropological finds: the anthropological composition of the southern Turkmen population of the 3rd millennium is very close to the Mesopotamian bone material of the same period (cf. e.g. Jettmar et alii 1966, Part I, afterword). We will return to these latter connections when discussing the emanations of the Uruk-Jemdet Nasr period.

Agriculture also appears in Afghanistan in the first half of the 4th millennium (Mundigak I). The finds testify to a foreign people who immigrated, who brought with them knowledge of copper and the slow-turning potter's wheel. The shapes and decorative motifs of the ceramics point to Turkmen settlers, but the appearance of the slow-turning potter's wheel proves that colonists did not only come from Turkmenistan to Afghanistan: in Turkmenistan, the potter's wheel was not used until much later. This circumstance is therefore clearly of Mesopotamian origin-725

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It refers to the presence of needle groups in Afghanistan (the Eridu-Ubaid civilization was already familiar with the slowly rotating potter's wheel in the 5th millennium BC!), and their appearance must undoubtedly be linked to the rich deposits of lapis lazuli there, which semi-precious stone had a very important cultic role in Sumerian Mesopotamia (and even much later): with its deep blue color, it served as a symbol of the "egocedn", transcendental powers, and divinity (we know from Herodotus that the sanctuaries on the top floor of the ziggurats were adorned with a deep blue color even in the middle of the 1st millennium BC).

We have already mentioned that at the turn of the 5th/4th millennium BC, strong northern Ubaid influences also appeared in the settlements of the Lake Urmia cultural circle: in the contemporary ceramics of Pisdeli Tepe and Geoy Tepe -- in addition to Elamite parallels -- the Eridu-Ubaid 4 traditions became increasingly widespread (v6. Burney-Lang 1975, p. 81). The path of Mesopotamian-Elamite influences towards Lake Urmia can be observed in the contemporary finds of Tepe Giyan in western Iran. Close parallels of the ceramics there -- corresponding to the Eridu-Ubaid 3-4 wares -- are found at Lake Urmia, especially at Dalma Tepe, Pisdeli Tepe, Hasanlu in layers IX and VIII, and at the Gird Hasan Ali settlement (cf. Hrouda 1971, p. 72). All these connections, as well as the above-presented emanations of the Halaf cultural circle around Lake Urmia and in northern Azerbaijan in the second half of the 5th millennium (I. p. 719), testify to the fact that since the last centuries of the 5th millennium there had been continuous close contacts with the Transcaucasian regions, in which an early Transcaucasian civilization with a uniform appearance over large areas (Kura-Araxes culture) developed in the last quarter of the 4th millennium -- as a more modest northern counterpart to the Sumerian urban high culture that had flourished somewhat earlier, and in whose assemblages of finds we find massive Mesopotamian-Sumerian parallels even in later times. Small extensions of the northern Ubaid culture can be traced eastwards to Tell-i Iblis in eastern Iran. At this settlement, founded around 4500, Eridu-Ubaid 3 and 4 pottery soon appears, accompanied by characteristic Ubaid chin figurines (I. Hrouda 1971, p. 37). The pottery of the V/B layer of Tepe Jahja (c. 3700-3400) also in eastern Iran is very similar to the somewhat earlier Susa A and Eridu-Ubaid 4 wares (I. Amiet 1977, p. 573).

However, the massive influences of the late Eridu-Ubaid cultural circle reached far regions in the northwest and west.

As already mentioned when discussing the emanations of the Halaf culture, this culture also extended to Eastern Anatolia and Cilicia, and we can even detect clear Halaf influences in the upper layers of Can Hasan in Central Anatolia and Hacilar in Western Anatolia at the turn of the 6th/5th millennium. We have also seen that all these parallels are of enormous size. were partial manifestations of migrations, which undoubtedly took place in the Hassuna-Samarra-Halaf area-726

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During these migrations, the &Sak-Mesopotamian-Srian settlers first colonized southern Mesopotamia, and then eventually reached southern Turkmenistan (Dzeitun), the Aegean (Seskio), the Balkans (Starévo) and the Kárpát Basin (Körös). Well, in the northern Ubaid period, ca. 4500-3700, we can observe a westward expansion of all kinds, after 4500 the northern Ubaid culture replaces the Halaf culture everywhere, not only in the Emenek tribal areas, but also further afield. Between 4500 and 3700/3600 BC we see the overwhelming spread of Eridu-Ubaid pottery in Anatolia, over increasingly larger areas (cf. Propyl. Kunstgesch. vol. XII, p. 38). In the central region of Anatolia — due to the lack of finds at that time — we cannot directly demonstrate the emanations of the northern Ubaid culture, but in Southeastern and Southern Europe, the Aegean region and the Balkans, close pre-Asian parallels clearly appear in this period, which testify to repeated immigrations. The characteristic pre-Asian mother goddess cult and bull cult, the obvious pre-Asian parallels of ceramics, clay figurines and other finds were also generally widespread throughout Southeastern Europe during the 5th-4th millennium (cf. Propyl.

Kunstgesch. Vol. XIII, pp. 96-99). In addition, at the turn of the 5th/4th millennium, typical Halaf pottery appears in Thessaly and on the Aegean islands, and shortly thereafter, in the first half of the 4th millennium, late Ubaid pottery appears, and immediately with very developed forms, which indicates immigration — writes, for example, F. Matz (Crete, Mycenae, Troy). He then emphasizes that in the 4th millennium, a population of the same anthropological type, of pre-Asiatic origin, lived on the Aegean islands, Crete and the Greek mainland, which was not Indo-Germanic. Modern research explains the dominant effects and expansion of the late Eridu-Ubaid civilization over vast areas by saying that the consolidation and generalization of advanced irrigated agriculture in southern Mesopotamia from the middle of the 5th millennium BC led to a massive increase in population again, similar to the enormous population growth that took place in the millennia following the development of the Neolithic, as a result of which the already known demographic explosion occurred in the second half of the 6th millennium BC.

In Mesopotamia, the regularly irrigated lands were harvested twice, and sometimes even three times, a year. Thus, by the second half of the 5th millennium, the population density had increased dramatically again, and thus, as a result of the continuous and relatively rapid cultural and technical development that could be observed at the same time, the basic prerequisites for economic, cultural, and even political equalization covering larger areas were also given.

727
===== PAGE 717 =====

At the same time, the separation of individual social strata, the division of labor, and the development of various specialized branches of occupation -- industry, trade, etc. -- also took place, which led to the development of the Varosian high culture in the tribal areas: Southern Mesopotamia and Elam in the following Uruk period (cf. Amiet 1977, pp. 50-51).

The above demographic circumstance and the simultaneous rapid cultural and social development can therefore explain — quite obviously — the high degree of dynamism and surprisingly rapid expansion of the Eridu-Ubaid 3-4 civilization into the aforementioned vast areas.

Distant emanations of the Sumerian culture of the Uruk-Jemdet Nasr period The Uruk period began around 3800 BC, and appears in archaeological finds as an unbroken continuation of the last phase of the Eridu-Ubaid civilization in southern Mesopotamia. In the stratigraphic sections of both of the more thoroughly examined sites—Eridu and Uruk—the gradual transition between the two periods can be clearly observed.

We emphasize again — as it is of primary importance in the question of the ethnogenesis of the Sumerians — that, for example, in Uruk layer XIV, the characteristic Uruk ware appears alongside the Eridu-Ubaid 4 pottery of Addi, and gradually becomes dominant over three layers, while the late Ubaid pottery occurs together with it, diminishing from layer to layer, but only disappears completely in layer XI (cf. e.g. Lloyd 1981, p. 44; Amiet 1977, p. 538). Similarly, in Eridu, Eridu-Ubaid 4 and Uruk ware occur together for a while.

— The unbroken tradition of temple architecture and cults — which also testify to ethnic continuity — has already been discussed in detail above (cf. pp. 685-687 and 701-704).

The characteristic Sumerian high culture developed in southern Mesopotamia during the Uruk period. From this cultural center, powerful impulses spread to neighboring areas from the very beginning of the era, and Sumerian culture became a model for the civilizations of almost the entire Near East, with which no other culture could compete. This strong centralization necessarily led to the insignificance of more distant areas. However, after a short time, technical development, especially the acceleration of transportation (chariots, horseback riding), bridged greater distances, and the Near East civilizations adapted to each other, but in such a way that the Mesopotamian center was always accepted as the driving force (see Hrouda 1971, pp. 76-77).

The strength of early Sumerian civilization and its dominant influence reaching far-flung areas are proven, among other things, by the occurrences of seal cylinders from the Jemdet Nasr period in Iran, Syria, Egypt, Anatolia, the Aegean, and even Transylvania.

728
===== PAGE 718 =====

Sumerian economic relations - the acquisition of raw materials covered the whole of East Asia (cf. Müller-Karpe 1968, p. 277). The Sumerian pictograph developed the Elamite writing system, as well as the Egyptian hieroglyphic writing. This can hardly be doubted today, since pictographs identical to the Mesopotamian ones have been found in such distant regions as Romania (Tatarlaka) on the one hand, and Baluchistan (Tepe Jahja) on the other (cf. Lloyd 1981, p. 77). The spread of the characteristic artefact assemblages of the Uruk culture to the core areas of the preceding northern Ubaid culture has already been described (l. pp. 693-698). In addition, however, the powerful emanations of the Uruk culture can be detected in many places in more distant areas, of course — as we have already experienced almost regularly with the expansions of the cultures of earlier times — again with a corresponding phase delay. At the very beginning of the Uruk period, therefore, the emergence of only northern Ubaid forms indicates — indirectly — that the expanding population of the Uruk culture first displaced certain groups of the northern Ubaid culture from their previous places of residence. We must consider such indirect effects as, for example, the aforementioned late Ubaid parallels in southern Turkmenistan (Anau I, Namazga I), Afghanistan (Mundigak 1) and in these places in the second quarter of the 4th millennium, i.e. actually already at the beginning of the Uruk period.

So, once again, the well-known, repeatedly repeated process took place: the gradual expansion of a newly formed, developed, dominant cultural community with great cultural activity in the central areas of East Asia into the surrounding areas initially partially displaces the former inhabitants of these closer regions to the periphery, to even more sparsely populated regions, and then later appears itself in more distant areas.

For example, in Iran, the late Ubaid parallels of the first half of the 4th millennium are gradually replaced by clear signs of direct emanations of the Uruk culture from the middle of the millennium onwards. In layers I-3 of Tepe Sialk II/I, as we have seen, the Eridu-Ubaid 4 forms still dominated to a large extent, but in layer II/I, around 3500, epoch-making changes took place almost without a transition: large-scale metal casting began, the use of seal presses of Mesopotamian origin became common, and the rapidly rotating pottery wheel also appeared — surprisingly early — the first remains of which we already know were found in layer VII of Uruk, around 3600-3500. All this is a sign of a leap in technical and economic development (cf. Hrouda 1971, p. 72; Amiet 1977, p. 572). Shortly afterwards, in layer III/I, between ca. 3400-3300, typical Uruk pottery also appeared, and then in the IV. In the period (from 3300 onwards), the characteristic vessel forms of Uruk ware ("flower pots", jugs with slanted spouts, etc.) become increasingly common, and soon Mesopotamian-729

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Mian seal cylinders, as well as clay tablets with Elamite-Sumerian pictorial inscriptions from around 3000 BC, also appear (cf. e.g. Amiet 1977, p. 572). Elamite-Sumerian pictograph tablets (economic records) from the same period (c. 3200-3000 BC) were found in the V layer of Godin Tepe in western Iran, together with typical grayish-black Uruk vessels brought by a colonial population of Mesopotamian or Western Elamite origin (cf. Amiet 1977, p. 552). Archaeological research suggests that Godin Tepe was a walled Sumerian-Elamite colonial trading center at this time (cf. Strommenger 1978, p. 18).

In the V/A layer of Tepe Yahya in eastern Iran (ca. 3400-3200), Uruk pottery ("flower pots") also appears, along with larger-scale copper processing and even arsenic bronze. In the following IV/C layer (ca. 3200-2900), the immigration of Elamite groups can be dated: Elamite-style pictographic tablets and seal cylinders appear, but accompanied by typical Mesopotamian Jemdet Nasr pottery (cf. Amiet 1977, pp. 573-574). It should be noted that the simultaneous appearance of the characteristic Jemdet Nasr ware most likely indicates that in addition to the Elamites, we must also count on Sumerian colonists. This is strongly supported by the fact that the city of Tepe Yahya, which was founded at the same time as the IV/C layer and is located much further east, Several Sumerian seal cylinders from the Jemdet Nasr period were also found in the lowest layer of the settlement of Sahr-i Sokhta, and the pottery is very similar to the Mesopotamian-type wares from the IV/C layer of Tepe Yahya (cf. Propyl. Kunstgesch. Vol. XIII, p. 49). But anyway: the Elamite culture of the Uruk and Jemdet Nasr periods is entirely dependent on the contemporary Sumerian culture - so much so that it is often impossible to decide from the finds whether they were made in Mesopotamia or Elam.

These distant eastern Iranian settlements were intermediate bases on the southern route of the lapis lazuli trade (cf. also pp. 27-28 of our book), which is probably why Elamite and Sumerian groups appeared in both places. However, Tepe Yahya was also an important raw material production site because of the soapstone-like chlorite mined there: objects made of chlorite, which were easy to work with and attractive (vessels decorated with relief carvings, boxes, amulets, seal stamps, etc.) were very popular both in Mesopotamia and Elam, and in the later Indus culture,

At the third significant site in eastern Iran, Tell-i Iblis, we find a situation identical to the previous ones. While late Ubaid pottery occurred in layers 2 and 3, dating from 3800-3400 (Eridu-Ubaid 3-4), in the following layer 4, around 3300, a single-colored glazed pottery very similar to Uruk ware appears - just like in the previously known sites, here too there is a parallel with the widespread spread of metalworking: mosan: layer 4 was practically full of the remains of metal casting crucibles

730
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(cf. Hrouda 1971, p. 37). B evidence suggests that Tell-i Iblis was a very important center of early metalworking in Iran (cf. Hrouda 1971, p. 73). However, this early, large-scale metalworking, which can already be called a distinct metal industry, which suddenly, without transition, appeared simultaneously in highly developed forms and on such a wide scale that it far exceeded the metal needs of the population of the settlement, obviously did not originate in Tell-i Iblis, where it had no precedent in the preceding layers.

It is not found in the Uruk pottery, but undoubtedly arrived there with the colonists who also brought with them the close parallels of the Uruk pottery. These groups were probably Mesopotamian-Sumerian ore-mining and metalworking settlers who exploited the deposits in the area, smelted the mined ore immediately on site and partly processed it, and then transported the semi-finished goods to Mesopotamia (cf. Rehork 1980, p. 324). Since the vast majority of metal objects - starting with the preceding Eridu-Ubaid period - were found in the Mesopotamian region (north and south), and even during the Uruk period a large-scale metal processing and metal casting industry can be found in Uruk (cf. e.g. Hrouda 1971, p. 79), modern research places the beginnings of the systematic, professional practice of metalworking in the area of the late Eridu-Ubaid and then the Uruk culture, and sees the Tell-I Iblis metallurgy as a colonial ore smelting settlement serving Mesopotamian needs.

We have already pointed out the parallels between the pottery of the Tepe Hissar I period (ca. 4000-3000) in northern Iran and Sialk III and, through this, partly late Ubaid and Susa A. We have also mentioned that in the graves of the beginning of this period there are many more male skeletons than female ones, which is a clear sign of colonization (cf. p. 725). In addition, seal presses and cast copper occur since the founding of Hissar (cf. Amiet 1977, p. 555). All this points to settlers of southern origin, coming from Sialk and ultimately from Mesopotamia-Elam. After these antecedents, at the beginning of the Hissar II period, between ca. 3000-2900, a single-colored glazed dark gray pottery suddenly appears without any transition, which is strikingly similar to the gray version of the Uruk ware (cf. Hrouda 1971, p. 79). At the same time, this type of pottery was widely spread throughout northern Iran (Sah Tepe, Turang Tepe, Jarim Tepe, etc. — cf. e.g. Hrouda 1971, p. 99). Research has not yet been able to agree on the origin of this pottery. Some associate it with the presumed appearance of ethnic groups immigrating from the northeast, in which they believe the appearance of the first Indo-Iranians can be discerned. However, the archaeological contexts that can be counted show something completely different: namely, the southern, ultimately Urukian origin of this pottery type. Thus, e.g. Hrouda also states - in dispute with the above ideas - that since before the appearance of this North Iranian product at Godin Tepe

731
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We find typical Uruk pottery (in layer V; between 3200-3000; = f. above), one of the variants of which is also made of grayish-black glazed yyyyy, it seems very likely that the Mesopotamian Uruk pottery of the 4th millennium and the monochromatic glazed dark gray Iranian ware of the 3rd millennium are somehow related (cf. Hrouda 1971, p. 99). Apart from Godin Tepe, which is the only one mentioned by Hrouda, we have already seen the same situation at Tepe Sialk; there, as early as 3400-3300, some typical Uruk vessel shapes still appear, and starting from 3200, Uruk pottery is found in large quantities, together with Mesopotamian seal cylinders from the Jemdet Nasr period and Elamite-Sumerian pictographic tablets. It is quite obvious - and international research also takes this position - that these phenomena are evidence of the expansion of the Sumerian high culture of the late Uruk and Jemdet Nasr periods, and not the remains of some obscure Indo-Iranian tribes, which - incidentally - can be first detected in Iran only more than 2000 years later.

But the finds from Turkmenistan also testify to the opposite direction of population movements. During the Namazga II period of the Orttan, between about 3500-3000, agricultural settlements were already widespread from Turkmenistan through Afghanistan to India. The development of culture gradually moved from the southwest and west, from northern Iran and southern Turkmenistan towards the aforementioned areas (northern lapis lazuli route; cf. our book, p. 27). The most characteristic motif of ceramics, amulets, and seal stamps in the entire region is the stepped pyramid (cf. Propyl. Kunstgesch. Vol. XI, p. 169). In Turkmenistan, the population increases significantly around 3000, and in the following period (Namazga III - ca. 3000-2600) the direct effects of Sumerian urban culture also appear: at the end of the period, urban settlements arise, and soon we find citadels and monumental buildings (cf. Propyl. Kunstgesch. vol. XIII. p. 170). At the same time — at the beginning of the 3rd millennium — colonial settlements of southern Turkmen cultures appear one after another in the neighboring northern and eastern regions, starting from the Aral Sea along the Amu Darya up to Fergana (v6. e.g. Jetmar et alii 1966.).

All these phenomena were the consequences of the previously mentioned mass immigrations from Mesopotamia that occurred around 3000. In other words, if we also take into account the Turkmenistan finds just outlined, then it can be practically considered proven that the massive Uruk parallels that can be observed in Sialk since 3400 and in Godin Tepe two centuries later must also be viewed as partial phenomena of colonization originating from Mesopotamia. It is also obvious that groups of settlers had to leave Mesopotamia at the end of the Uruk period, before the colorful Jemdet Nasr pottery came into vogue, otherwise they would have already

732
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They would not have brought with them the gray glazed wares of the Uruk period, they apparently reached Tepe Sialkot around 3400, and they reached the southeastern coast of Kāspi-t6 (Hissar and the aforementioned settlements) and southern Turkmenistan around 3000. We can therefore see that around 3000 archaeological finds in the region in question clearly show migrations from the southwest to the northeast, and not the other way around, as some orthodox Indo-Germanists assume without any factual basis. The earliest expansion of the Uruk culture to the north and northwest can be traced to the former northern Ubaid areas, where, as we have already shown, Uruk pottery replaced northern Ubaid wares almost everywhere between 3700 and 3500. Beginning around 3500, we have identified typical Sumerian colonial cities in Syria in the Habur River region and along the middle Euphrates. At the same time, we have also pointed out strong Uruk emanations extending to the Levantine coast (cf. pp. 693-698). Further northwest, towards Anatolia and Southeastern Europe, the effects of the expansion of the Uruk culture into Syria and Southeastern Anatolia can also be observed already in the early Uruk period, around 3700-3600, but in these more distant areas at first only indirectly: in the emergence of late Eridu-Ubaid parallels on the Aegean islands and in Thessaly - developed forms immediately appear, which indicates immigration from Anatolia (cf. p. 727). By the middle of the 4th millennium, this new wave of Proto-Asian settlers had already reached the Northern Balkans and the Carpathian Basin. The vast majority of researchers attribute the emergence of the Vinca-Tordos culture, which emerged around 3500, to the result of the mass immigration of Proto-Asians, since the finds of this cultural circle again show numerous contemporaneous Proto-Asian parallels and very strong Near Eastern cultural influences (cf. e.g. Propyl. Kunstgesch. vol. XIII. p. 99). The Vinéa-Tordos culture appears roughly in the former Starévo-Körös areas, but cannot be traced back to 6

' from a cultural circle: it was established by colonists from Anatolia — states ' the cited manual by KJ Narr. Although the route of immigration cannot be . demonstrated directly, this is strongly supported by the fact that the closest Anatolian parallels can be demonstrated in the oldest period (Vinéa A), and moreover in the region of culture furthest from the Near East, in Transylvania. In the southern parts of the Balkans, it appears only in later times, after the turn of the 4th/3rd millennium BC. 65 y | culture forms of finds, but this is a secondary phenomenon, and is only a consequence of the fact that in the later stages of this culture it strongly influenced the Balkan region, up to Thessaly (cf. Narr 1975, pp. 175- | 179). F. Schachermeyr also showed the same, adding that the early Vinéa culture was exclusively monochromatic, black, dark grey

733
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or reddish-brown glazed pottery very likely indicates that there was significant activity in the Northern Balkans and Transylvania as early as the second half of the 4th millennium There must have been a metalworking center, nor was his conclusion - which he published as early as the 1950s - largely confirmed by the recently discovered 4th millennium copper mines in the vicinity of Belgrade (cf. Schachermeyr 1976, pp. 66-67). — Thus, international archaeological research assumes that metalworking was fully developed in the Northern Balkans and the Carpathian Basin in the second half of the 4th millennium (cf. e.g. Whitehouse 1976, p. 157 - 1. also map no. 4). Once again, we find grayish-black or reddish monochromatic glazed ceramics, strongly reminiscent of Uruk ware, together with a greater development of metalwork. This striking phenomenon — at such an early time, at such a great distance from Mesopotamia, and together with so many other pre-Asian parallels in the finds of the Vinca-Tordos culture — clearly suggests that in the middle of the 4th millennium, pre-Asian colonists indeed arrived in the Northern Balkans and Transylvania, and soon exploited the ore deposits there and established the local metallurgy. The immigration seems to have taken place mainly by water — at least this is indicated by the striking lack of related finds in the Southern Balkan region, as Narr also specifically draws attention to (cf. Narr 1975, p. 178). The Tisza culture appears as an unbroken continuation of the Vinéa B stage (somewhat before 3000), with also striking Anatolian parallels, which testify to a repeated pre-Asian expansion, this time around the turn of the 4th/3rd millennium (cf. Narr 1975, pp. 181-182). The same is demonstrated very convincingly by Nándor Kalicz (Clay Gods, pp. 36-43). These strong Anatolian influences manifested in the Tisza culture also appear in Dobruja and east of the Iron Gate in the same period (cf. Narr 1975, p. 185). — These latter data, however, are again only signs that these immigrants also arrived by water — via the Black Sea, the Danube and its tributaries.

Narr also highlights that in the middle stage of the Neolithic, the Coreneolithic ancient Balkan groups were largely replaced by new ethnic groups of pre-Asian origin, and these settlers from Anatolia formed the majority of the population in many places during the Late Neolithic period in the 3rd millennium, such as in the Vinca CD, Gumelnifă, Salkufa, Tiszapolgár, Butmir cultures (Narr 1975, p. 190). G. Childe has clearly recognized these close Proto-Asian-European connections for several decades. He convincingly argued about the Vinéa-Tordos culture and the Tisza civilization, showing that they were the creations of new waves of Proto-Asian immigrants, who significantly developed the older — also Proto-Asian settlers at the time = Coreneolithic civilizations, The Vinya-Tordos and the Tisza stage

734
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Childe summarized the Danube yŷ culture as Neveri (the earlier tape a decorated cultural circle (Danube I culture), and he points out in a graphic way that According to archaeological evidence, the northern and western European areas remained at the Coreneolithic stage of the previous Danube I culture even in the Danube II period, significantly lagging behind the Balkan Carpathians. from the cultural level of the basin region, which is due to the dominant cultural influences and influences that newly arrived in the Vinéa Tordos areas from the southeast. clear evidence of distortions. He also noticed that the Danube II cult The population in the region of ra increased significantly at the end of the 4th millennium, and certain signs suggest a regular exchange of goods between the Danube region and East Asia (see 1. Childe 1949).

The Cucuteni-Tripolitanian culture emerged in Moldavia and somewhat later in Western Ukraine in the middle of the 4th millennium, Childe continues, mainly as a product of settlers from the Vinéa-Tordos culture and the northeastern Balkan regions, and gradually expanded to the heights of the Ukrainian forest steppe. The cultural image of the Tripolitanian settlements is fully consistent with the characteristic finds of the Danube II culture and earlier Eastern Balkan cultures (e.g., the Boian culture) (1. Childe 1949.).

Recent research has also confirmed these findings of Childe. It has been proven that the Tripoli culture did indeed spread from the southwest to the northeast (the lower layers of the Moldavian settlements are older than the Ukrainian ones), and its finds are very close parallels with the slightly earlier Carpathian Basin and North Balkan cultures; and that these correspondences are ultimately of Proto-Asiatic origin. But anthropological studies have also revealed Mediterranean-Europids and Armenoids (cf. p. 723).

All this means in Hungarian that we are once again faced with the well-known phenomenon of colonization: the Proto-Asiatic settlers who appeared in the northern Balkans and Transylvania around 3500 initially partly pushed the earlier Transylvanian-northern Balkan groups north and northeast (early Cucuteni-Tripoli), and later - apparently under the pressure of the second Proto-Asiatic wave that arrived around 3000 - they themselves appeared in the Moldavian-Ukrainian region (mature Cucuteni-Tripoli).

These clear pre-Asian connections are organically integrated with the pictographic signs known for a long time from the Vinéa-Tordos culture, as well as the Tatar pictographic tablets 15, which, as a result of the above, cannot be viewed as isolated phenomena.

Thus, "Propyläen Kunstgeschichte" only interprets the general opinion of international research when it emphasizes that the Tatar tablets are clear evidence of the wave-like immigration of Proto-Asian agricultural settlers to Southeast Europe, in this case the 4/3.

735

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around the turn of the 4th millennium. In fact, we must also count on such immigrants in the first half of the 3rd millennium, during the development of Polish culture, and then a few centuries later, during the development of Baden-Pécel culture (cf. Propyl. Kunstgesch. Vol. XIII, p. 99).

The path of cultural influences and migrations originating from East Asia in the last centuries of the 4th millennium can only be outlined based on scattered finds limited to certain keystone settlements in the intervening areas of Asia Minor, the Aegean, and the southern Balkans. The Mesopotamian finds of the second half of the 4th millennium (late Uruk and Jemdet Nasr periods) can only be directly traced to the Syrian coast, the Amug plain and the middle Euphrates (Uruk pottery, Sumerian seal cylinders, pictorial tablets, etc. — 1. pp. 696-698), and the continuity of finds is interrupted in the interior of Anatolia (although demographic data — the sudden increase in population starting at the end of the 4th millennium and the appearance of grayish-black or red monochromatic ceramics throughout Anatolia — indirectly indicate mass immigration from the Mesopotamian-Syrian direction; more on this later). Only one Sumerian seal cylinder from the Jemdet Nasr period is known from the layer called Alisar 8M (M-mounts; hill) in central Anatolia. But since layer 8M belongs to well after 3000, to the first half of the 3rd millennium, this find is not unambiguously related to the end of the 4th millennium (cf. e.g. Goetze 1957, p. 38).

Looking further west, however -- surprisingly at first glance -- the

Typical finds testifying to the expansion of the Sumerian culture of the 861 Uruk and the Jemdet Nasr periods appear again on the islands of the Aegean Sea and on the western coast of Asia Minor. There are not a few of them (see map no. 5 for the following).

A Sumerian seal cylinder from the Jemdet Nasr period was discovered in a tomb dated to about 3000 BC on the island of Amorgos (eastern Cyclades), not far from the coast of Asia Minor (cf. Propyl. Kunstgesch. Vol. XIII, p. 64). The seal cylinder belongs to the so-called brocade style, which only appeared towards the end of the Jemdet Nasr period and became common during the early Sumerian dynasties at the very beginning of the 3rd millennium. For this reason, some date the Amorgos cylinder somewhat later (v6, e.g. Müller-Karpe 1974, p. 155).

We can also gain very important clues regarding the temporary stay of the proto-Asian immigrants who appeared in the Northern Balkans and Transylvania around 3000 BC from the finds of the oldest layers at Fikirtepe, located directly south of the Bosphorus on the eastern shore of the Sea of Marmara, Kumtepe near Troy, also on the coast, and Thermi on Lesbos. The lower layers at all three sites date from before the founding of Troy (c. 3000 BC) (cf. e.g. Goetze 1957, pp. 22-24; Alkim 1968, pp. 80-81), i.e. they belong to the last centuries of the 4th millennium, precisely to the Jemdet Nasr period. 8 Moreover, each settlement was located on the Dardanelles > Sea of Marmara - Bosphorus passage, which was of prime commercial - but also strategic - importance. guards and rules.

736

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In Thermi, in the lowest cultural layer directly above the soil, i.e. at the level of the last centuries of the 4th millennium BC, several metal melting crucibles and casting samples were found (cf. Müller-Karpe 1974, p. 427), which is clear evidence of local metal processing, but at the same time it also testifies that under such circumstances of the finds we can in no way speak of local antecedents of metal casting: the colonists who founded the settlement undoubtedly arrived with knowledge of metal casting from their previous homeland, and immediately began metal processing after their settlement. However, we have data on advanced metalworking - namely industrial metal casting - during the 4th millennium BC only from the Early Ázsái, more specifically from the core areas of the Uruk culture, or about the surrounding regions strongly influenced by this culture (cf. what was said above, especially pp. 729-731).

We can only be strengthened in this opinion by the further important connection that the oldest pottery of the Thermi settlement is a dark-gray or blackish, sometimes reddish, single-colored glazed ware, which can also be detected in the Troy I period, starting from the founding of Troy, that is, from about 3000 (cf. Goetze 1957, pp. 23-24). In other words, this pottery shows significant parallels with the similarly dark-gray or red single-colored glazed Uruk ware.

At the same time, the oldest pottery from Fikirtepe and Kumtepe, dating back to the end of the 4th millennium, is similar to the wares found in Thermi, but more importantly, close parallels can be found on the island of Chios (Emporio), in the Cyclades, in Thrace, and even along the Danube, in the area of the Vinéa-Tordos culture, as UB Alkim repeatedly emphasizes (cf. Alkim 1968, pp. 81 and — again — 82).

Another very important piece of information for our current research is that on the island of Lemnos (again, just before the entrance to the Dardanelles!) in the settlement of Poliochni, which dates back to the first half of the 3rd millennium BC. It was a real city, half surrounded by walls (cf. Narr 1975, p. 508), bronze was already being cast around 3000, and a Mesopotamian seal cylinder in the brocade style was also found there (cf. Müller-Karpe 1974, pp. 874 and 155). In addition, in the lowest layer of the aforementioned Thermi, in addition to casting patterns and crucibles, a genuine bronze pin dating from before 3000, containing 13.1% tin, and a ring made of pure tin were found (cf. Müller-Karpe 1974, pp. 116 and 427). Similarly, in the Sitagroi V. layer, located near the Néstos River in eastern Macedonia, which is contemporaneous with the lowest layer of Thermi, tin bronze can also be dated (L. Müller-Karpe 1974, p. 427). From a somewhat later period, but well before the middle of the 3rd millennium, we also know of genuine tin-bronze objects from the island of Amorgos: needles and a bracelet with a tin content of more than 13% (cf. Müller-Karpe 1974, p. 153).

Since tin bronze was known only to the Sumerians at that time (v6. e.g. Müller-Karpe 1974, pp. 425-426), they were all-737 in the Aegean region.

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So far, the only truly early - from the 4th millennium - bronze finds (tin bronze only appears in the Trojan-Aegean-Cretan cultural circle a good half a millennium later, in the middle of the 3rd millennium), indicate with great probability - if not with absolute certainty - that at the end of the 4th millennium and the very beginning of the 3rd millennium we must reckon with Sumerian commercial and metal-processing colonial settlements on several Aegean islands and on the coasts of Asia Minor in the foreland of the Dardanelles. This is also evidenced by the ring made of pure tin from Thermi: at that time even the rich could only afford such a thing in a metal trading center.

The findings listed so far therefore allow us to conclude, without exception, that the Tatar pictographic tablets, which are practically identical in writing style and linework to the Sumerian pictographs of the Jemdet Nasr period, must obviously be associated with Mesopotamian-Sumerian ore-mining and metal-processing groups that arrived by sea. The geographical distribution of the parallel findings; ell. The striking concentration of metals (or ores) in the Dardanelles region suggests that the transport of metals (or ores) was mainly carried out by water, and the intermediate bases of the ships — but also, apparently, at least partly, the ore smelting workshops (cf. the crucibles, casting patterns found in Thermi and the bronze foundry of Poliochni) — were located on the islands before the entrance to the Aegean Sea of the Dardanelles (Lemnos, Lesbos), on the Trojan coast (Kumtepe, and then Troy I from 3000 BC), on the coast of the Sea of Marmara near the Bosphorus (Fikirtepe), and further south on some eastern islands of the Cyclades (Amorgos). However, the home ports of the metal-transporting ships must in all probability be sought on the eastern shores of the Mediterranean: on the Amug plain, around Ugarit and Byblos; — in the Levantine regions to which the Sumerian culture of the late Uruk and Jemdet Nasr periods directly extended in the last centuries of the 4th millennium. In this regard, it should be noted that many researchers already count on the existence of a Sumerian empire extending to the Mediterranean Sea in the late Uruk and Jemdet Nasr periods (L. later).

In addition to the Tatar tablets, other Sumerian-type finds found in Transylvania and the Banat are organically linked to these clear archaeological data. The number of these finds is increasing and they show with increasing certainty that at the turn of the 4th/3rd millennium BC, a considerable number of Mesopotamian-Sumerian groups were present in the territory of the Vinéa-Tordos culture, including groups that explored ore, processed metal, transported metal products, and provided military support for mining, smelting, and trade routes.

The Tarárlak tablets are by no means unique finds, discovered almost by chance. It has been known since the excavations of Zsófia Torma in the 1880s that numerous pictographic signs, mainly scratched on clay vessels, have been found in the Late Neolithic and Early Copper Age settlements of the Tordos culture in Transylvania.

738

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were found, which are now dated to between ca. 3200-2800 BC. In the first half of the 20th century, such pictographic signs were again found during excavations carried out at the archaic settlement of Vinjá near Belgrade, 8 and it turned out that the Tordos culture was nothing more than the Transylvanian variant of the Vinca culture. Thus, this cultural circle was named the Vinda-Tordos culture as a summary name. At the same time, the stratigraphic observations of the Vinda excavations, which were repeated and expanded several times, made it possible to record the culture in time: it was established that the 68-101608 V1y culture began somewhat before the middle of the 4th millennium, flourished between ca. 3500 and 2500, and was divided into four periods (Vinéa A, B, C is D).

By the middle of our century, with the ever-increasing number of archaeological finds, it became increasingly likely that East Asian settlers played a primary role in the formation of the Vinéa-Tordos culture. Thus, we saw above that G. Childe was able to demonstrate with great probability in 1949, from the context of finds known at that time, that from the middle of the 4th millennium onwards, repeated waves of immigrants arrived from East Asia to the northern regions of the Balkans, to Transylvania (Vinéa-Tordos culture) and to the banks of the Tisza (Tisza culture). Moreover, he also pointed out emphatically — long before the discovery of the Tatar tablets! — that at the turn of the 4th/3rd millennium BC there seemed to be a regular exchange of goods between East Asia and the Danube region.

Similarly, R. Heine-Geldern established in 1950 that the writing symbols of the Tordos culture are clearly of Sumerian origin, and explained their appearance with a large-scale migration from Northern Mesopotamia or Eastern Anatolia in the second half of the 4th millennium, which, according to him, was closely related to the Sumerian expansion of that time — the Uruk era — (cf. Heine-Geldern 1950).

In the 1950s, finds in this direction continued to increase, and in 1955, F. Schachermeyr managed to demonstrate with scientific method that in the second half of the 4th millennium, in the northern Balkan-Transylvanian region, in the area of the Vinéa-Tordos culture, single-colored glazed clay pots appeared en masse, which were the so-called

"metal shock" signs, i.e. they prove that somewhere in the area there was already a significant metal processing center in the second half of the 4th millennium, and potters began to imitate metal vessels to an increasing extent (cf. Schachermeyr 1955).

In connection with the monochromatic glazed pottery, we are reminded again of the also monochromatic glazed Uruk ware, similar ceramics to which appeared in the second half of the 4th millennium BC in the aforementioned Aegean and western Asia Minor coastal settlements, precisely on the most likely route of the Sumerian ore-seeking colonial settlers towards the northern Balkans and the Carpathian Basin. And most importantly: this characteristic pottery appeared in all these sites together with surprisingly early concrete evidence of advanced metalworking in the area in question,

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This was more or less the archaeological situation when the archaeologist N. Vlassa from Cluj-Napoca found the Tatarla tablets (1961). In other words, from the aforementioned earlier works - the list of which could be extended with completely similar findings - it is clear that the Tatarla finds can in no way be considered isolated, self-contained things, which, abstracted from all other archaeological data, without antecedents and connections, would have appeared in Transylvania unexpectedly and inexplicably. On the contrary: we have shown above that even before their discovery, researchers had a lot of very concrete evidence available to them that the Neolithic civilizations of the Balkans and the Carpathian Basin in general, from the very beginning (Sesklo, Starcevo, Körös, Kremikövc), ultimately originated in East Asia, that they arose through repeated mass immigrations of Near Eastern agricultural settlers in several waves, and that, more specifically, the formation of the Vinéa-Tordos civilization must undoubtedly be attributed to one of these East Asian colonial waves.

The Tatarlak tablets, therefore, no matter how much of a sensation they caused among the public interested in prehistoric issues, were no surprise at all to experts in the archaeology of the Balkan-Transylvanian region. In fact, based on the already known and systematized findings, it was to be expected that something similar, clearly and indisputably Mesopotamian, would sooner or later be discovered in the Vinca-Tordos culture. Thus, the appearance of the Tatarlak Sumerian pictographic signs was only the last mosaic stone in the cultural history picture that could already be sketched with considerable certainty. As Hrouda, for example, states: the Tatarlak tablets provided only the final, unambiguous evidence that the Sumerians' trade relations extended to Transylvania around 3000 (cf. Hrouda 1971, p. 103). Or, as the "Propyläen Kunstgeschichte" also states, the Tartárlak tablets confirm the assumption, which could already be supported with great probability, that the Vinéa-Tordos culture was also the creation of pre-Asian colonists (cf. Propyl. Kunstgesch. Vol. XIII. 99, p.).

In accordance with this situation, studies on the pre-Asian connections of the Vintéa-Tordos culture naturally intensified, starting with the discovery of the tablets. It is not an everyday thing in prehistoric research that such clear evidence from such early times is available to scientists, such as the Tatar script,

Thus, the discoverer of the tablets, N. Vlassa, also collected and examined more thoroughly numerous finds that had been excavated previously but had not been published or taken into account. He searched for and evaluated the long-forgotten "y" that had often been gathering dust for decades in the museums of Cluj and other Transylvania.

740
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natkoz6 finds", primarily the more than 10,000 pieces of the collection of Zsófia Torma. At the same time, he closely followed the excavation of the Transylvanian and Banat sites of the Vinta-Tordos culture, which excavations repeatedly brought new important finds to light. He published the detailed results of his researches in several scientific papers (1. e.g. Vlassa 1970.). We can now dispense with the individual presentation of these, because he summarized his conclusions very clearly and in a concise manner in a statement to 5. Saveanu, a colleague of the magazine "Magazin". For this reason, and because this article is also available in Hungarian, Vlassa would like to thank our readers for this Bucharest

We present the question based on his statement, mostly adhering to the text of the Hungarian translation verbatim (cf. Vlassa-Saveanu: Did the Sumerians visit us? ~ Hungarian Primitive Studies, Buenos Aires 1977, Vol. II, No. 6; in Romanian: Magazin, 19 Dec. 1970, Bucharest).

Vlassa immediately highlights in the introduction of his statement as the fundamental requirement for examining the issue the same thing that we have already emphasized above, namely that the inscriptions on the Tatar tablets cannot be considered an isolated phenomenon. "I am thinking of the numerous writings found on the bottom of one or another vessel of the Tordos finds, which have been known for a long time," he notes in this regard, and then continues: "In our country, the vessels of the Tordos Neolithic culture are related to those from the famous Vinéa culture in Yugoslavia... The re-evaluation of some older finds in the collection of the Historical Museum of Cluj-Napoca and the recent discovery of a rich series shed unexpected light on the deep and significant cultural community of the peoples associated with the Tordos-type Neolithic culture and the population of the Middle East."

But new finds also point in this direction. Recently, Vlassa reports, archaic ceramics were found during the excavation of the foundations of a transformer house in the main square of Cluj-Napoca. The salvage excavation revealed that the vessels date from the Tordos culture. One of these Tordos-type pottery fragments shows a surprisingly accurate incised sketch of a Cycladic-type rowing-sailing ship. The middle mast with its triangular sail and the positions of the rowers can be clearly distinguished in the image. The age of the find can be estimated at ca. 2800. — This ship drawing is clear evidence that at the turn of the 4th/3rd millennium BC there must have been lively contacts between the Aegean region and Transylvania — as Vlassa also emphatically emphasizes below.

In his first scientific works dealing with the Tatar tablets, Vlassa asserted that the population of the Tordos culture probably knew rudimentary pictographic writing. He pointed out that this hypothesis would also solve the long-debated question of where these peoples came to Transylvania from. He emphasized that the Tordos site could contribute greatly to clarifying the question.

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A thorough examination of the writings on the fragments of objects found in type settlements and on the finds kept in the Cluj-Napoca Museum, which have now increased to more than 300 signs (1).

Further research has confirmed the correctness of Vlassa's assumption. It has been known for decades that a clay disk with 26 strange, engraved signs on both sides of it is found in the old collection of the Cluj-Napoca Museum. Until recently, no particular significance was attributed to it. Later, it turned out that many of these signs on the Tordos clay disk are similar to the signs on the famous so-called Blau tablets kept in the British Museum in London, which date back to the early dynasties of Mesopotamia, notes Vlassa.

It is imperative to point out here that Vlassa is wrong in dating the Blau tablets, because they are much older than the so-called early Sumerian dynasties (ca. 3000-2450): their writing character, the linework of the signs reflects a transitional state between the pictographic signs of Uruk IV. and III. layers, that is, the tablets belong to the borderline between the Uruk era and the Jemdet Nasr period (1. Reallexikon der Assyriologie vol. II. under the heading "Blau'sche Tafel", which corresponds in time to ca. 3200 BC. But cf. also Hrouda 1971, p. 95, who also dates the Blau tablets to the Jemdet Nasr period.

In this sense, the most interesting piece — Vlassa continues his account — which moves the debate from the level of assumptions to that of certainty, is a fragment of pottery found in Tordos, which is now in our museum, in a record of almost a century. The extraordinary significance of the fragment of pottery has escaped the attention of scientists. The clay tablet bears the imprinted series of signs of pre-Sumerian incised cylinders ... This fragment also bears the traces of an animal-shaped coat of arms, as well as the details of the decorative gate of an ancient Mesopotamian temple. It is a truly surprising fact: the most perfect analogies of the fragments of pottery lead again — as in the case of the Tatar tablets — to the time horizon of the VI-IV. layers of Uruk, and towards the settlement of Chafadje in Iraq, i.e. again towards the 2800s BC.

We must note again that Vlassa is wrong again, and this time twice. First: we have already seen above that Uruk VI-II. layers, as well as the lower layers of Chafadje from the late Uruk and Jemdet Nasr periods, do not point to 2800 BC, but deep into the second half of the 4th millennium, Uruk VI III layer corresponds to about 3400-3000. Secondly: the seal cylinders of the late Uruk period are not "presumerian", but early Sumerian finds, since international research has defined the entire Uruk period as Sumerian from the very beginning, starting from the XIVth Uruk layer, that is, from about 3800, and in fact, many renowned experts go back much earlier in dating the origin of Sumerian culture (1,699,703 pages). On the other hand, the seal cylinders in question are not even 742

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can be called "pre-Sumerian cylinders" because all researchers, without exception, clearly attribute the invention of the seal cylinder to the Sumerians. That is, pre-Sumerian Talking about ("presumer") seal cylinders is a contradiction in itself.

s, An unexpected discovery was made in a quarter of Timisoara, Ronac, Vlassa continues. - In a settlement of the Tordos-Vinéa type, a clay vessel was unearthed, and a relief of a fantastic creature was attached to it: the lion-headed eagle, a distant copy of the pre-Sumerian deity Imdugod (newer reading "Anzu") (this is not "pre-Sumerian", but early Sumerian; its occurrence can be dated to the Uruk III layer, i.e. the Jemdet Nasr period for the first time; cf. e.g. Hrouda 1971, p. 93 — note from us). Vlassa also highlights: I would like to emphasize that the eagle with the lion's head on the fragment found in Timisoara shows a strangely accentuated deep eye socket, and this circumstance brings this representation very close to its eastern origin."

Regarding the assumption that an ancient people of the Sumerian type arrived in the territory of Transylvania at the turn of the 4th/3rd millennium BC, or that the ancient tribes of Mesopotamia moved from their original places of residence, Vlassa notes the following. "It has long been known that certain Neolithic cultures in our country, as well as in the Balkan region, show close kinship with the cultures of the Aegean, Anatolian and Asia Minor Neolithic world. However, new discoveries seem to confirm that these kinship ties go beyond simple cultural influence (emphasis added) and allow the assumption that certain Asian groups had previously settled in the Balkans, and even in our country. This applies primarily to the two pure representatives of the southeastern related culture, the older settlements of Körös and the subsequent Tordos-Vinca culture. A series of inhibiting factors intervene in our modern perspective. Today, we sometimes have difficulty imagining events that are not very distant. This includes, for example, the historical fact that is now little known, namely that only 150 years have passed since the time when the Transylvanian shepherds reached Adrianople (Edirne) by grazing their sheep. On the other hand, we know that the pre-Sumerian peoples (again, incorrect terminology; — our note) also had sailing ships and thus, in addition to their connection with the islands of the Aegean Sea or the Bosphorus region, it could not have been an insurmountable obstacle for them to reach our homeland."

This would therefore be the current position of international archaeological and prehistoric research on the issue of the finds from Tartar. We have seen that the above-mentioned manuals, scholars — and with them the archaeologist himself, Vlassa 15 — base their conclusions on extensive, complex investigations, on a careful comparison of the relevant Mesopotamian, Asia Minor, Aegean and Balkan finds, and thus arrive at the well-considered conclusion that the Vinéa-743

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In the area of Tordos culture, at the turn of the 4th/3rd millennium BC, we must very likely assume the presence of Mesopotamian-Sumerian ethnic groups.

We also note that the probability of the Mesopotamian colonists arriving by water is supported to a great extent by the findings of the West Asia Minor (Lemnos, Lesbos, Amorgos, Kumtepe, Fikirtepe) and their parallels in the Danube region, which have been presented in detail above, and which are clearly maritime in nature, and by the drawing of a Cycladic-type sailing-rowing ship found in Cluj-Napoca by Vlassa, as well as by the connecting links that have been missing from the interior of Anatolia and the southern regions of the Balkans, i.e. the striking lack of connectable finds from this intervening land area. Makkay J. has come to practically the same conclusion, although he very cautiously only considers some Sumerian traders who escaped to Transylvania (1. Makkay 1970.).

We must once again refer to the eminently important fact that recently copper mines that were already being worked in the second half of the 4th millennium BC have been found in the vicinity of Belgrade, which strikingly and indisputably confirmed F. Schachermeyr's previously formulated "fem-shock" thesis: that in the second half of the 4th millennium BC there was indeed copper mining and metalworking in the northern Balkan-Transylvanian region. From this it follows clearly that the striking concentration of clearly and unmistakably Sumerian-type finds (Uruk-era seal cylinder impressions, Jemdet Nasr-era pictographic tablets, Imdugud relief, etc.) in the region of the very first European center of copper mining, in the immediate vicinity of the copper deposits around Belgrade and in Transylvania, can most obviously only be explained by the presence of a significant number of Mesopotamian-Sumerian ore prospectors and metalworking groups.

Let us now review the cultural state of the Anatolian-Transcaucasian areas during the late Uruk and Jemdet Nasr periods.

We have already mentioned that in the inner, inland areas of Anatolia in the last quarter of the 4th millennium, the direct effects of Mesopotamian Sumerian culture cannot yet be detected — meaning that clearly Mesopotamian artifacts are still missing from this period, with the exception of the single Jemdet Nasr era seal cylinder, which was found in Alisar in a much later environment than the end of the 4th millennium. However, it is highly probable that we must take into account strong Mesopotamian cultural influences as early as the Uruk and Jemdet Nasr periods, and even the settlement of smaller Mesopotamian groups in Anatolia.

The B-radiation can be traced most clearly through the combined study of ceramic types and the larger spread of metalwork. Namely, the characteristic dark gray, blackish, or reddish monochromatic glazed ceramic products, which are often used as vessels.

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In their form they also show close parallels with the Uruk ware that preceded the Yalam, appearing in almost identical form throughout the entire northern Near Eastern region, from Western Anatolia to Northern Iran, between about 3200-3000, 8 while a simultaneous boom in metalwork can be observed everywhere. Hrouda particularly emphasizes that the uniformly glazed surface of the Uruk pottery and the clay vessels of these northern regions compared with it, as well as the forms of the pottery, correspond to the Mesopotamian metal vessels of the same period (cf. Hrouda 1971, p. 79).

Since the appearance of pottery in Eastern Anatolia (at the end of the 7th millennium), the culture from Mersin to Lake Van was roughly uniform, the first phase of which (between ca. 6200-5500) was characterized by the so-called Old Monochrome, the second (between ca. 5500-4500) by Halaf pottery, and after 4500 the northern Ubaid ware and its local imitations dominated (1. e.g. Propyl. Kunstgesch. Vol. XIII, p. 55). This is approximately the same region that appears in Sumerian sources from the 3rd millennium under the name Subir-ki or Subar-ki (Subir or Subar land), and in the 2nd millennium as a Hurrian territory, or in Akkadian under the name Subartu (cf. pp. 34-38 of our book).

The pottery of these areas is also characterized by the monochromatic glazed, dark brownish-reddish or black ceramics from around the turn of the 4th/3rd millennium, from about 3200. This ware, whose center is to be found around Lake Urmia, around Janik Tepe, is obviously related to the dark gray monochromatic ceramics of northern Iran to the east, which — as we have already seen — appears there somewhat later, around 3000 (cf. Hrouda 1971, p. 100, note I). Originally — around 3000 — it is found westward to the upper reaches of the Euphrates, and then in the first half of the 3rd millennium — by all indications as a result of population movements — it gradually spreads to Central and Southern Anatolia, Syria, and even Palestine. On the Amuq plain, solid red or black pottery with a glazed surface, often with relief decoration, appears as early as 2700 (cf. e.g. Alkim 1968, p. 113).

It is best known in the literature from its first site in Palestine as the Khirbet-Kerak ware (cf. e.g. Hrouda 1971, pp. 102-103).

This pottery from the Urmia region and eastern Anatolia is, however, nothing more than the typical ceramics of the so-called early Transcaucasian culture (Kura-Araxes culture), which shows a surprisingly uniform picture in the vast area between Malatya and the Caucasus, and between Erzingan and Lake Urmia from the end of the 4th millennium to the last centuries of the 3rd millennium (cf. Burney-Lang 1975, pp. 95-96). Moreover, since its beginnings around 3250, and even earlier, the area in question had very close and intensive cultural relations with the neighboring Mesopotamian-Sumerian culture to the south (cf. e.g. Narr 1975, p. 515), and the methods of production and forms of the metal finds found there are practically identical to the Mesopotamian metal objects of the same period. At the same time, this culture has characteristic ceramics

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The routes of its later spread correspond almost exactly to the main directions of the Subir-Hurrian expansion that began in the first half of the 3rd millennium BC (cf. Burney-Lang 1975, pp. 101-104; also pp. 33-35 of our book 1).

In Central Anatolia, after a cultural stagnation of over a millennium (between ca. 4800 and 3300, only a few scattered, small settlements in the primitive Neolithic stage are known; cf. above, pp. 672 and 717-718), higher — urban — civilization begins in the second half of the 4th millennium. The layers of all three important known Central Anatolian settlements — Alisar, Alaca Hüyük and Kültepe — founded in the last centuries of the 4th millennium between ~ 3300 and 3000 (Alisar, layer 19M-12M, Alaca Hüyük IV period, Kültepe layer 14-13) are again characterized by the well-known gray or black monochromatic glazed pottery. The connections with the already mentioned very similar North Iranian ware, which appeared around 3000, are still unclear, but this Central Anatolian pottery probably has a lot to do with the previously discussed, also monochromatic glazed pottery of the same period from Azerbaijan and the Urmia Lake area — which, in turn, is clearly related to the North Iranian gray ware (1. above, p. 745) (cf. e.g. Hrouda 1971, pp. 99-100).

According to these, at the turn of the 4th/3rd millennium BC, we find extensive cultural — and presumably even ethnic — contacts from Northern Iran to Central Anatolia in the northeastern, northern and northwestern fringes of the Late Uruk — Jemdet Nasr Sumerian cultural circle: dark-gray, monochromatic glazed Uruk-type ceramics were generally widespread in Western and Central Iran (Godin Tepe, Tepe Sialk), Transcaucasia and Central Anatolia between 3400 and 3200 BC, and by around 3000 BC they had already appeared in Northern Iran.

But we can go even further. In Western Anatolia, we can also detect the beginnings of urbanization in this period. The earliest pottery of the Northwestern Anatolian cultural circle, the Troy-Jortan group, which appeared around 3000, is also a dark gray, blackish or sometimes reddish-brown monochromatic glazed ware (v6. Hrouda 1971, p. 102), which apparently originated from the Mesopotamian-Sumerian colonial settlements of the late 4th millennium BC on the mainland of Asia Minor, before the entrance to the Aegean Sea through the Dardanelles - already described in detail (1. pp. 735-738). Its completely identical precursors are the lowest, ca. They were found in the layers dating from 3200-3000 BC (cf. e.g. Goetze 1957, pp. 23-24), but the pottery of Kumtepe, located directly near Troy, on the coast, and Fikirtepe, near the southern entrance to the Bosphorus, from the end of the 4th millennium BC, is also very similar to that of Troy. It therefore seems that the founding of Troy I is closely connected with these settlements on the coastal and near-coastal islands - by all indications ultimately of Mesopotamian Sumerian origin, primarily the Kumtepe circle, and represents an organic development of the latter (cf. e.g. Narr 1975, p. 508):

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over time, a fortified urban center became evidently necessary.

zasa, which is undoubtedly of a coastal nature (like the other yyyyyy/y settlements without exception), but is not located directly on the coast, if not a few kilometers inland, no doubt because of the unexpected sea

be protected against birds. This characteristic method of protection is used by the

In recent times, it is found uniformly throughout the Mediterranean region (more details in 1. below).

From the above brief overview, it can be established again that, as we have already observed in the more distant small areas of Halaf and Eridu-Ubaid pottery, the parallels of Uruk ware in more distant areas have also been largely completed long ago.

appear with a "phase delay", mostly around the turn of the 4th/3rd millennium, when the Uruk period in Mesopotamia itself

These phenomena can be explained most logically by the presence of Sumerian ore-exploring and metal-processing colonists in the region in question. It is well known that ores, or other raw materials that were very important to the Sumerians, are nowhere to be found in the core areas of Sumerian civilization. The highly developed Sumerian metalwork, which was already equipped for mass production at that time, and many other industries, were therefore completely dependent on the import of raw materials, and the need for metal and other raw materials had to be continuously ensured, otherwise the entire socio-economic organization would have collapsed. It also necessarily follows from this fact that the task of organizing mining and metallurgy in remote ore deposits and ensuring the regular continuity of work had to be solved in some reassuring way. Under the conditions of the time, this goal could obviously only be achieved by sending out large-scale colonizing groups and militarily adequately equipped ore exploration and metal processing squadrons.

In contrast to the raw material poverty of the Mesopotamian plain, the ore deposits of Anatolia, Transcaucasia, and Iran were known since the beginning of the Pre-Asian Chalcolithic, and the early Uruk culture, and even the preceding Eridu-Ubaid civilization, covered their metal needs from them. We recall, for example, the Ubaid and especially Uruk-era ore deposits of Tepe Sialk in Iran, the developed ore deposit of Tell-i Ibilis, also of the Uruk-era, which undoubtedly produced for export, and the simultaneous multifaceted cultural connections of this settlement with Mesopotamia; the ore-richness of Tepe Gaura in northern Iraq and its equally massive Uruk parallels; in Anatolia, the appearance of northern Ubaid pottery in the Malatya and Erzingan regions, directly around the ore-rich eastern Taurus Mountains; etc. Moreover, we have also seen that the Balkan and Transylvanian ore deposits of the Vinca-Tordos culture 15 were, with a very high degree of probability, demonstrably discovered and exploited by Mesopotamian Sumerian ore miners.

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The Sumerian cultural parallels of the Uruk and Jemdet Nasr periods, which extended to distant territories⁶ and in many cases were evidence of conscious colonization (providing raw materials), clearly show that we need to fundamentally revise the generally accepted views on the beginnings of Sumerian high culture and, with it, the formation of states and empires. Thus, until recently, research held that the Sumerian kingdom emerged in the times after the Jemdet Nasr period, at the beginning of the 3rd millennium.

That is why the period following the Jemdet Nasr stage, from about 3000 to 2450, is still called the period of the early Sumerian dynasties. However, in recent times, more and more

They emphatically pointed out the misleading nature of this designation, since in the light of the results of recent excavations, it seems beyond doubt that kings (sacred princes) ruled the Sumerian city-states as early as the Uruk period, and even the existence of larger organized empires in the second half of the 4th millennium, during the late Uruk and Jemdet Nasr periods, is highly probable.

P. Amiet, for example, states that the Sumerian kingdom — contrary to the previous opinion of researchers — had obviously been established long before the era of the so-called "early dynasties", and in the surviving Sumerian royal annals, traditions dating back to the Uruk era were mixed with later historical data (cf. Amiet 1977, p. 78). According to H. Schmökel and his co-authors (Kulturgeschichte des Alten Orients), there are many signs that a Sumerian empire covering large areas with its seat in Uruk existed as early as the Uruk and Jemdet Nasr periods.

L. Woolley also emphatically points out that the kings of the First Uruk Dynasty (around 3000 BC according to the long chronology) had a historical right (!) claim to fiefdom over Elam. During the Uruk and Jemdet Nasr periods, Sumerian power expanded to increasingly distant territories to the north and northwest, he writes, and Sumer only broke up into numerous vārosāllams at the beginning of the so-called early Sumerian dynasties, after 3000 (L. Woolley 1961, p. 15). E. Strommenger goes even further in this direction. As early as 1962, he called the Jemdet Nasr period the "6th dynastic period" (1. Strommenger 1962), and more recently he has shown that we must definitely count on effective central leadership even before the middle of the 4th millennium, that is, before the development of Sumerian high culture in the narrower sense, since without such a thing, the irrigation systems, temple buildings and defensive installations of the size of Eridu, which we already know from the earliest colonization of southern Mesopotamia, would not have been possible (see Strommenger 1978, p. 14). However, in the second half of the 4th millennium, there was undoubtedly a large-scale,

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The existence of empire-like state organizations is attested by the massive cultural influences that can be detected simultaneously over vast areas of the early Sumerian period (late Uruk and Jemdet Nasr periods), Strommenger continues. The direct emanations reached deep into the heart of the Iranian highlands in the east, to Godin Tepe, where a walled Sumerian-Elamite trading colony has been excavated, to the north to Nineveh, to the northwest to Carchemish and Habuba Kabira — the latter also with a large Sumerian colonial city fortified with a tall tower — but strong Sumerian cultural influences are also found on the Syrian coast. In the south, new finds have been discovered on the coasts of 1 y and on the island of Bahrain. they also testify to the dominant emanations of early Sumerian civilization, which ultimately reached Egypt and played a significant role in the development of high culture there. . The question of how such a large-scale unification could have developed in these vast areas from the local, diverse cultural groupings of earlier prehistoric times has not yet been decided. In any case, we must already reckon with certain imperialistic aspirations that went far beyond the borders of individual city-states" — concludes Strommenger's above-outlined discussions (cf. Strommenger 1978, p. 18).

Let us spend some time on the Egyptian finds of Sumerian origin just mentioned by Strommenger, which several experts consider to be so important that they even see the immigration of smaller Sumerian groups to Egypt as a leading stratum that created culture and achieved the political unification of Northern and Southern Egypt (cf. e.g. Müller-Karpe 1968, p. 279). In addition to shedding light on the nature of Sumerian cultural influences, these finds also provide chronological lessons of paramount importance, since in the case of absolute Egyptian chronology we stand on much more solid ground than in Mesopotamia. The Mesopotamian finds of late Uruk and Jemdet Nasr origin found in Egyptian sites that can be dated with sufficient accuracy therefore allow us to determine the absolute dates of the late Uruk and Jemdet Nasr periods with practically the same certainty.

A carved knife handle found near Gebel el-Arag shows a characteristic, unmistakable depiction of a Mesopotamian prince from the late Uruk period, as well as both types of early Sumerian ships (low and high-set bows and sterns) well known from Mesopotamian finds, with crescent-shaped military insignia. Ships with high bows and sterns are typical of the early Sumerians, and the crescent-shaped military insignia (on the lower ships) is also characteristic of the late Uruk period: it often appears on seal cylinders of that time. However, the fact that naval campaigns were already conducted in the Uruk period is proven by a recent Uruk seal cylinder impression found in 13020 Mis (all this in 1. Propyl. Kunstgesch. Vol. XIII. 234-235. 749 ===== PAGE 739 ===== p.). The sea route from Mesopotamia to Egypt ran through the Persian Gulf and along the southern coast of Arabia towards the Red Sea. Good evidence of this is the Mesopotamian pottery of the Jemdet Nasr period found in Oman and Abu Dhabi (cf. Propyl. Kunstgesch. Vol. XIII, p. 49). However, more chronologically important than the above knife handle - which was not found during an authentic excavation - is the fact that several Mesopotamian seal cylinder imitations were found in the tombs of the so-called Nagada II period (also known as the Gerzean period), which are generally dated by research to between 3400-3200 BC (Propyl. Kunstgesch. Vol. XIII. p. 240. fig. 16a-e). Some of these - e.g. those shown in Figs. 166 and 16d - clearly belong to the tin brocade style, which we already know (l. p. 736) to have first appeared in Mesopotamia only at the end of the Jemdet Nasr period, and to have become general at the beginning of the period of the following early dynasties (cf. e.g. Hrouda 1971, p. 96; Lloyd 1981, p. 77; Propyl. Kunstgesch. Vol. XIV, p. 216).

These new insights have led to the previously commonly used so-called "relative" approach being rejected by most "short" and "middle" Mesopotamian chronology — especially for these early times researchers. Previously, they assumed that the Mesopotamian finds from the late Uruk and Jemdet Nasr periods found in Egypt were [/ They date from the time of the change of the 2nd Egyptian dynasty, around 2800-2700 (cf. Margueron 1970, p. 180). However, recent studies have corrected this parallelism in two stages. On the one hand, it became clear that the beginning of the 3rd Egyptian dynasty (Pharaoh Djoser) can be placed — with considerable certainty — not at around 2780, and that the 1st dynasty began at least around 3000, but rather even earlier (cf. Montet 1975, pp. 4 and 12-13). On the other hand, it was proven that the Egyptian finds in question, showing Mesopotamian parallels from the Uruk and Jemdet Nasr periods, indisputably date from before the establishment of the 1st dynasty, namely from the predynastic Nagada II. period, i.e. from around 3400-3200. Consequently, we must place Uruk IV. and III. layers, i.e. the end of the Uruk period and the Jemdet Nasr stage, between 3300 and 3000 (cf. Propyl. Kunstgesch. vol. XIV. p. 19). The earliest seal cylinder found in Egypt comes from the beginning of the Nagada II stage, and according to its style it belongs to the very end of the Uruk period (Uruk IV). However, since the beginning of Nagada IIe can be dated to around 3300, Uruk IV. layer can be dated to around 3350-3250, and the beginning of the Jemdet Nasr period to the generally accepted 3100.

instead of ("middle" chronology) we should date it between 3250-3200 (cf. Propyl. - Kunstgesch. vol. XIV, p. 216). From these very certain Egyptian chronological parallels, it is therefore clear that only the so-called "serious calculation", according to which the Lord "long" Mesopotamian chronology can be taken

790

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with period ended around 3250/3200, and the Jemdet Nasr period lasted from about 3250-320061 to 3000 (cf. e.g. Strommenger 1978, p. 68; Kramer 1978, p. 276). Notes on the question of Mesopotamian chronology This is an opportunity to address the still unresolved issues of Mesopotamian absolute chronology. The debate in the literature about the pre-Asian chronology of the 3rd and 2nd millennia BC continues to this day, and the uninitiated reader is often puzzled by the often contradictory time definitions found in works on the prehistory or art of Mesopotamia. The main reason for this is that the absolute date of Hammurabi's reign has not yet been determined with sufficient precision. The only thing that is certain is that we cannot in any case place his accession to the throne after 1728 BC, or before ca. 1951 BC (v6. e.g. Hrouda 1971, p. 23; Müller-Karpe 1974, p. 68).

As a result of this uncertainty, three types of chronology have emerged in the literature (with several sub-variants, which we cannot discuss in this brief summary). The so-called short chronology places Hammurabi's accession to the throne in 1728, the medium chronology in 1792, and the long chronology in 1930. These dates have specific astronomical reasons, and are ultimately related to the 64-year cycles of the planet Venus. We do not have the opportunity to go into detail here. We need only mention one astronomical data, which is the most important in every respect, since ultimately all three mentioned chronologies are based on it. From the 6th year of the reign of Hammurabi's 4th successor, Ammisaduga, texts from the same period have survived, which record the relationship between the heliacal rising of Venus and the time of the new moon at the beginning of December of that year. From these records, it can be calculated using modern astronomical methods that the Venus-new moon constellations given in the texts could only have existed at the beginning of December in ca. 1577 BC, or around 1800 BC, but never in the intervening period (cf. e.g. Hrouda 1971, p. 23). Since it is known that 151 years passed from Hammurabi's accession to the throne until the 6th year of Ammisaduga, this means that Hammurabi could have ascended to the throne around 1728 at the latest and around 1951 at the earliest.

According to this, the middle chronology (Hammurabi 1792; l. above) could be completely ruled out, since the astronomical phenomena mentioned could not have existed 151 years after 1792, i.e. around 1641 (cf. e.g. Hrouda 1971, p. 23). Nevertheless, many experts used this chronology until recently, which can be explained by the fact that on the one hand, the short chronology — as we will show below — practically includes all contemporaries or

751 ===== PAGE 741 =====

It is in direct contradiction with the source data that is close in time, and on the other hand, many researchers believe that the 4 long chronology excessively extends the so-called period of the middle of the 2nd millennium "dark" BC, from which very few single-circuit wellheads have survived. Thus, the medium chronology is actually a compromise, a kind of average provisional measure to facilitate orientation, which, however, has a right to exist only until more precise data from the middle of the 2nd millennium BC are available to us. Moreover, to a certain extent, the long chronology itself can be called a compromise, since we saw earlier that it places Hammurabi's accession to the throne in 1930, although according to the aforementioned astronomical calculations, we should place Hammurabi around 1951. This reduction, however, results from the fact that the representatives of the long chronology base their calculations on the traditional data of contemporary Babylonian and Assyrian inscriptions and the king lists that are close in time. Neither of these sources very likely state that Hammurabi ascended the throne around 1930.

We have already seen that, with regard to the beginning of Mesopotamian historical times, recent studies have clearly confirmed the dating of the long chronology, since, based on the Mesopotamian finds from the late Uruk and Jemdet Nasr periods found in Egypt, we must date the end of the Uruk period to approximately 3250/3200, and the Jemdet Nasr period to between 3250/3200 and 3000 (1. pp. 750-751). Well, for decades now, followers of the long chronology have placed the Jemdet Nasr stage in exactly these centuries, while the medium chronology dates it between 3100/3000 and 2900/2800, and the short chronology dates it between 2900-2700 (cf. e.g. Soden: Propyl. Weltgesch. Vol. I, p. 584 and Hrouda 1971, pp. 23 and 24).

So let's see into which chronological scheme the surviving contemporary and close-in-time sources about the events of the 2nd millennium BC can most likely be fitted.

Going back in time to 747 BC, we have astronomically accurate data calculated to the year (1. e.g. Hrouda 1971, p. 22). Up to 909 BC, the absolute chronology can be determined from the Assyrian eponym registers (the "eponyms" were officials in Assyria who were comparable to the Roman consuls of the imperial era and who changed annually) with a maximum error of 10 years (+5), and then, going back to about 1450, the dates of historical events can also be deduced with considerable certainty based on the Assyrian and Babylonian king lists (cf. e.g. Hrouda 1971, 22-23. 016.).

A great help is provided by the collection of cuneiform tablets found near Amarna in Egypt, which contain the diplomatic correspondence of the last pharaohs of the 18th dynasty with the kings of various pre-Asian states in the Akkadian-Babylonian language, primarily from the time of Amenhotep III (1413-1375) and Amenhotep IV (1375-1358), but with multiple references to earlier periods.

752
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diplomatic and dynastic marriage relations, as well as since the Egyptian absolute chronology can be accurately determined with an error of barely 3-4 years up to 1991 BC (with the help of the so-called Sirius periods; cf. Müller-Karpe 1974, 17-18, pp.), we can also compare the absolute dates of the Babylonian (Kassu in Azidoben), Mitanni-Hurrian and Assyrian kings mentioned on the Amarna tablets with the precisely known years of reign of the pharaohs of the 18th Egyptian dynasty mentioned with them. Thus, for example, one of the Amarna letters mentions that the Kassu-Babylonian king Karaindaš sent envoys to Egypt in the 33rd year of Pharaoh Thutmose III. Since III. The 33rd year of Thutmose (1502-1448) falls in 1470, which places the latest possible date for the accession of this Kassite king to 1471.

Let us stay for now with the Kassite dynasty, whose 36 kings ruled for a total of 576 years according to the Babylonian king lists. We know the end of this dynasty quite precisely: its fall occurred at the earliest in 1171, at the latest in 1155, i.e. on average around 116348. In other words, we must place the beginning of the Kassite rule in Babylon — based on the king lists — around 1739+8. The Kassites, in turn, replaced the Hammurabi dynasty, which — also according to the king lists — ruled for a total of 304 years with 11 kings. Hammurabi was the 6th king of this dynasty.

Well, the years of the reigns of some of the kings of the Hammurabi dynasty are known from contemporary records, completely independently of the king lists: a total of 300 years can be dated from the sources. This means that the king lists are only 4 years off — if they are at all wrong, since the 300 years that can be dated are based on the year names found so far for the kings of the Hammurabi dynasty, and by no means mean that a few more year names will not be discovered. So if the king lists provide practically an almost year-accurate figure for the total duration of the Hammurabi dynasty centuries earlier, we can be absolutely certain that they are not wrong about the duration of the much later Kassuz period, which is much closer to the time when the lists were compiled.

But what does all this mean in absolute terms? It can be precisely stated that from the time Hammurabi ascended to the throne, his dynasty ruled for another 198 years. That is, if the Kassites — as the king lists state — really succeeded the Hammurabi dynasty with an accuracy of one year, then the date of Hammurabi's accession to the throne can be placed at the maximum around 1945, and at the minimum around 1929 (1171 or 1155 is the end of the Kassite dynasty, 576 years for the Kassites and another 198 years for Hammurabi). However, if we take into account the interregnum of a few years between the two dynasties — which is very likely — the beginning of Hammurabi's reign would be between about 1950 and 1934, i.e., with approximate accuracy to the astronomically calculated date mentioned above.

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mftott date, around 1951. However, let us not complicate the already unclear chronological confusion even more, and let us stick to the date of Hammurabi's accession to the throne, which is generally accepted in today's long chronology, 1930. It is essential to mention, however, that in the 1930s, international orientalist research - apart from the higher dates of some experts - almost unequivocally placed Hammurabi's accession to the throne in 1955 or 1947 (cf. Reallexikon der Assyriologie - hereinafter RLA --Vol. 1, pp. 241 and 371, or Vol. II, pp. 134-135). Today, however, the followers of the long chronology reckon that the first Kassus king (Gandaš) initially ruled for a few years in parallel with the last king of the Hammurabi dynasty, Shamsuditana, before he conquered all of Babylonia, and thus arrive at the date of Hammurabi's accession to the throne in 1930. — We can therefore see that the total period of the Hammurabi and Kassus dynasties handed down to us in the king lists can be fitted into the framework of the long chronology with an accuracy of almost a year.

But how do we stand with the medium and especially the short chronology? In the latter, the end of the Hammurabi dynasty is placed in 1531. Well, we have already mentioned above that yyyyyyyyyy, who according to the lists was the 15th king of the Kassian dynasty, was already ruling in 1470, so according to the lowest possible dating he ascended to the throne in 1471. His predecessor, Agum III, on the other hand, ruled for about 20 years; the name and years of the 13th Kassian king are not known exactly, but according to many he was called Ulamburias and ruled for about 5 years, and the 12th king, Castilias III, for about 30 years (cf. Soden: Propyl. Weltgesch. Vol. I, p. 69). All this means that the accession of the 12th Kassian king to the throne is at least (!) around 1526. In other words, according to the short chronology, the 11th or possibly the 10th Kassus king would have conquered Babylonia around 1531. On the other hand, an inscription from the 8th Kassus king, Agum II, survives in a later copy, in which he calls himself "Kassi and Akkad" and the king of Babylon, and also reports that he had the Babylonian temple of the god Marduk called Esagila rebuilt (!. RLA vol. I, p. 43). In other words, this data clearly excludes the short chronology, and this is ultimately the reason why most researchers accept the middle chronology, which places Hammurabi's accession to the throne one Venus cycle (64 years) earlier, in 1792, and consequently the end of his dynasty in 1595,

Learning from this failure of the short chronology, which can be traced back to the fact that the followers of this scheme completely ignore the data of the king lists, the middle chronology accepts in principle the traditional 576-year period of the Kassite dynasty, but argues that these 576 years should be counted from the time when the Kassites first appeared on the borders of Babylonia, and they probably also set foot in some eastern territories. Since the earliest source of information about the Kassites is Hammurabi's son, 754
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It dates from the 9th year of Samsuiluna, when this king reports on a Kassuz attack, but the 9th year of Samsuiluna falls in the middle chronology in 1741, so at first glance this line of thought undoubtedly seems very obvious, since we saw above that the beginnings of the Kassuz dynasty -- calculated over 576 years -- do indeed date back to around 1739+8. The credibility of this argument is reinforced by the fact that the king lists in more than one case show as successive dynasties that, according to contemporary sources, actually ruled in some areas of Mesopotamia in parallel. According to the middle chronology, the Kassites would have ruled all of Babylonia only starting with the aforementioned Agum II, the 8th king, after the Hammurabi dynasty was overthrown by the Hittite invasion around 1595. However, the dating of Agum II's reign around 1595 also fits perfectly into the order of the Kassite kings, since we have seen that Agum II's fourth successor, the 12th Kassite king, ascended the throne at least around 1526.

Upon closer examination, however, this line of thought of the adherents of the middle chronology cannot be maintained either — for a similar reason as we learned earlier in connection with the short chronology. It can be stated that the first Kassite king of the king lists, the aforementioned Gandaš, had a temple built in the city of Babylon in honor of the god Enlil, the founding charter of which has survived in a later copy (see RLA vol. III, p. 138). The author of the Reallexikon also explains this data by saying that the temple in question was "certainly outside the city of Babylon", but this is obviously only a modern interpretation of the followers of the middle chronology (the RLA works with the middle chronology), since Gandaš is clearly mentioned in the document. The argument for the partial simultaneous reign of the Hammurabi and Kassite dynasties can therefore only be accepted in the sense in which we He calls himself "lord of the four worlds", "king of Sumer and Akkad, and of Babylon" (1. RLA, vol. III, ibid.).

find it in the long chronology: Gandaš first
His reign apparently ran parallel to the last years of the 11th king of the Hammurabi dynasty (Samsuditana).
The long chronology is also confirmed by the total period of 368 years given in the king lists for the so-called Sealand dynasty. The beginning of this dynasty is known: the "Sealand" (Del-Babylonia, the former Sumerian tribal area) became finally independent in the 28th year of the aforementioned Samsuiluna, son of Hammurabi, and we also know that the Sealand dynasty was ended by the brother of the 12th Kassu king, Ulamburias, presumably during his brother's lifetime, whom he later probably succeeded on the Kassu throne (cf., e.g. Saggs 1975, p. 126). The reign of the 12th Kassu king, 111th Kassu, can be placed at least between ca. 1526-1496. The fall of the Sealand dynasty therefore also occurred during this period. In the long chronology, Samsuiluna's 28th year falls in 1860,

755
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Thus, the conquest of the Mediterranean, calculated according to the 368 years of the records, dates back to 1492, i.e. already during the reign of the successor of III of Castile. On the other hand, it is highly probable that also that the first king of the Sealand dynasty, Ima, may not have counted his reign from 1860, but from 13-18 years earlier, from the 10-15th year of Samsuiluna, during which period, according to Samsuiluna's own inscriptions, all of Babylonia was in flames, and he only managed to suppress or at least localize the uprisings with great difficulty. According to this possibility, the fall of the Sealand dynasty would also be 13-18 years earlier, between about 1510-1505, that is, indeed to the time of Castile III.
The above, however, means in the middle chronology that — since the 28th year of Samsui- luna falls in 1722 in this scheme — more than a third, about 138 years, of the 368 years traditionally passed down from the Sealand dynasty would have to be disputed, and the total duration of the dynasty would shrink to only about 230 years (from 1722 to about 1492). However, since the supporters of the middle chronology know well that such a reduction is highly unlikely, because such a large discrepancy is not found in the case of any other dynasty between the datable years and the lists, they argue that on the one hand the beginning of the dynasty should be placed about 18 years earlier, as we mentioned earlier, and on the other hand the end should be placed in the second quarter of the 15th century, between 1475 and 1450. Thus, for example, HWS Saggs speaks of "before 1450" (cf. Saggs 1975, p. 126), and W. von Soden writes exactly 1474 (cf. Propyl. Weltgesch. Vol. II, p. 69). In other words, they attempt to reduce the 138-year difference - alternately by researcher - by about half a century.
This correction is also indispensable because we have a land grant deed from Der in northern Babylonia by the 6th Sealand king, Gulkisar (see RLA vol. I, p. 371). There is no doubt, however, that the Sealand dynasty could only have extended its sphere of power (temporarily, until the consolidation of Kassite rule) to Der in the far north during the turbulent years immediately following the fall of the Hammurabi dynasty. Now, according to the king lists, GulkiSar 120

He died years before the fall of the Sealand dynasty, i.e. — if we count the end of the dynasty as 1492, 111. 1510 — around 1612 or 1630, while the middle chronology places the fall of the Hammurabi dynasty in 1595. We can see that Soden calculated very accurately: 1474 is the earliest possible date for GulkīSar to have granted an estate in Derb in the last year of his reign, one year after the fall of the Hammurabi dynasty, in 1594, Why does the eminent expert W. v. Soden reckon with the earliest possible date of 1474? Because the necessity of the temporal reduction of the fall of the Sealand dynasty, caused by the GulkīSar deed of donation, on the other hand, puts the followers of the middle chronology in the forced position that Karaindaš, the 15th Kassu king, ascended the throne in 756.

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The Amarna tablets can indisputably be dated to a minimum date of 1471, and therefore the beginning of Karaindad's reign in the middle chronology is simply placed around 1445 without any justification (6. Soden: Propyl. Weltgesch, vol. II, p. 69). It is obvious that if the fall of the Sealand dynasty -- which can be dated to the time of the 12th, or at most the 13th Kassuz king -- is placed in 1474, then the 15th king could not have reigned since 1471 either -- at least according to this upside-down logic, according to which contemporary written data can be ignored if they contradict the ideas of some modern researchers.

Of course, followers of the short chronology find themselves in an even greater dilemma in this regard: since in this system the 28th year of Samsuiluna falls in 1658, the Sealand dynasty — counting back to 1474 — has only 184 years left, exactly half of the 368 years in the lists. Finally, let us also look at which chronology we can best reconcile with the other main source group that has not been mentioned so far, namely the Assyrian King List, the so-called Assyrian-Babylonian Contemporaneous Chronicle, and the Assyrian-Babylonian Contemporaneous King List. The reign of the 39th king of the Assyrian kingly annals, Samšiadad I, can be compared with the years of Hammurabi with an accuracy of almost a year, using contemporary sources completely independent of the lists. It can be established that he ascended the throne 21 years before Hammurabi, and a text dated to the 12th year of Hammurabi last mentions him as king of Assyria. This also confirms that the 33 years of reign given for Samšiadad I in the Assyrian kingly annals are completely true, i.e. these lists are based on concrete data that go back at least to the time of Hammurabi. In other words, we must reject outright the modern interpretation of this

"source criticisms" that classify the data in the Assyrian king lists as an ordinary compilation or a list of fictitious rulers — as is mainly shown in the short

We can experience this in more than one case among those who follow the chronological order, and we will immediately see why.

The different versions of the king lists give a minimum of 1021 and a maximum of 1044 years of reign from the accession of the aforementioned Samsiadad I to the death of Salmanasar V (Sulman-ASared), who is classified as the 109th king, i.e. 722 BC (this date still belongs to the period that can be calculated astronomically to the year; cf. above, p. 752), but the years of the 66-67th and 84-85th kings are illegible (1. RLA vol. VI, pp. 102-115). That is, without the four kings who reigned for an unknown period, the accession of Samsiadad I to the throne would be between 1743-1766 BC at the latest, even in this first rough approximation. However, since we know from the contemporary Assyrian-Babylonian chronicles that A&SSurbeliniSeSu, the 69th Assyrian king, concluded a border treaty with the already mentioned Kassu-Babylonian king Karaindaš,

757
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who reigned at least between 1471-1430, so Assurbelinišesu's 9-year reign — also calculated at least — falls at the latest between ca. 1433-1424 (cf. e.g. Soden: Propyl. Weltgesch. Vol. II, p. 69). From this it can be established that the 84th and 85th Assyrian kings reigned — again minimally — for a total of at least 15 years: without these two kings, taking only the legible years of the lists, ASSurbelinišesu would be 15 years later. In other words, with the accession of Samšiadad I, we arrive at a minimum between 1758-1781.

It would be a stretch to determine the reigns of the 65th and 66th Assyrian kings. Here too, the contemporaneous chronicle provides information: it states that the 61st Assyrian king, ūy. Puzurassur, also held border negotiations with a Kassite king, Burnaburias I, who is in 10th place among the Kassite kings. We have already seen above that the 12th Kassite king, III. He ascended the throne of Castile — in this case too we have calculated minimally — cannot be placed after 1526 (1. 755. p.). Counting only a few years for the 11th Kassian king (his name and date are unknown), this means that I. Burnaburias died around 1530 at the latest, i.e. III. PuzuraSSur could not have ascended the throne after about 1533. — We emphasize again that in each case of parallelism that has arisen so far — both for the Assyrian and Kassian kings — we have started our considerations from the lowest possible dates. Then two variations are possible. PuzuraSSur III is one of the few rulers for whom the data in the king lists fluctuates; for him, 14 or 24 years are given. If he reigned for 14 years, i.e. between about 1533 and 1519, then the missing time of the 65th and 66th kings amounts to 48 years, since the lists are from the accession of ASSurbeliniSeSu (1433) and III. The period between the death of PuzuraSSur, which in this case would be 86 years, is given as only 38 years. If, however, PuzuraSSur III reigned for 24 years, i.e. until about 1509, then about 38 years remain for the two kings in question. Thus the 23-year fluctuation of the lists, which we have so far taken into account in the minimum dating of the accession of Samšiadad I, is reduced to 13 years, because looking back only from PuzuraSSur III. The date of PuzuraSSur's accession to the throne, which can be dated to around 1533, is important, not whether he reigned for 14 or 24 years. The minimum date of 1758 given above therefore increases by 48 years to 1806, but the maximum date of 1781 only increases by 38 years, since in this case PuzuraS3urra III was originally calculated to be 24 years old. That is, the date of Samšiadad I's accession to the throne, after the highly probable determination of the years of the four kings who reigned for an unknown period, falls between around 1806-1819. Since the short chronology places the beginning of Hammurabi's reign in 1728, according to its scheme, the accession of Samšiadad I cannot be before 1749, since we know that he occupied the throne of Assyria 21 years before the first year of Hammurabi. However, this completely excludes the short chronology. 758

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We can conclude from our further investigations that it proved to be completely unrealistic in light of the data of both the Kassus and the Tangeröldi dynasties, as well as the Assyrian royal annals.

In the middle chronology, however, in which the accession of Samšiadad I to the throne is placed in 1813 (Hammurabi 1792), the above dates would again at first glance fit satisfactorily, as we have already seen in relation to the Kassite dynasty. But again only apparently. For upon closer examination of the question, it soon becomes apparent that the Assyrian kingly records are far from complete, while the followers of the middle chronology - although this fact is by no means unknown to them (see Soden's statements 1. below) - consistently take into account only the mere dates of the records in their chronological considerations. After the already well-known son of Samšiadad I, ISmedagan I, for example, in most list versions, ASSurduġul follows, although it can be dated from contemporary texts that for a few years I. ISmedagan's son, Asinum, ruled, followed by a king named Puzursin, who dethroned Asinum. We also know of an original inscription from this Puzursin, but the records are silent about him (cf. e.g. Soden: Propyl. Weltgesch. Vol. I, p. 597). Instead, together with the aforementioned AS&Surdugul, six other kings are listed, all of whom were "senkihāzi" (literally "mar la mamana" = "son of Senki", i.e., his father is unknown), and they allegedly competed with each other for the throne for 6 years. The last of these seven kings is Adasi, who founded a dynasty again, and according to the lists, the later Assyrian kings descended from him. — In a list fragment, however, after ISmedagan I, a certain ruler named "...033077 (or "...askur") follows, followed by Rimus (v. Remu5) (cf. RLA vol. I, p. 246).

As a result of these uncertainties, Wv Soden, for example, emphasizes that the king list apparently includes only a few names of those who claimed the throne of Assyria in the turbulent centuries after the death of Hammurabi. During this period — up to the beginning of the 14th century — the Assyrian kings were vassals first of the Hurrians, later of Mitanni, and in many cases among the names listed in the king lists we must undoubtedly include governors appointed by foreign lords, who, however, are deliberately omitted in the lists, since on the one hand they try to shorten this inglorious period of Assyria as much as possible, and on the other hand they were compiled for obvious dynastic interests on the basis that in Assyria from the Old Babylonian times, starting with the aforementioned AdaSi — apart from a few illegitimate usurpers — until the 14th century BC. The same dynasty ruled until the 1st millennium (cf. Soden: Propyl. Weltgesch. Vol. I, p. 597).

Well, we can actually prove these deliberate omissions, and we can also prove that the Assyrian kings of the 1st millennium BC were much more precise.

===== PAGE 749 =====

sabb ~ obviously intended only for internal court use — had information about the actual reigns of their predecessors in the 2nd millennium, as we can read from the public regnal lists intended to prove the continuity of the dynasty. For example, in one of Esarhaddon's inscriptions he mentions that II. Samsiadad (the 57th king of the lists) ascended the throne 586 + 434, i.e. 1020 years before him (yy. RLA I. kot. p. 246). Since ASarhaddon reigned between 6. 680-669 BC, II. Samšiadad's accession to the throne is placed at 1700. At the same time, the regnal lists cover the period from the beginning of II. Samšiadad's reign to the accession of the already known III. PuzuraSSur, i.e. approx. Until 1533, only 79 or 83 years are given (the time of the 59th king varies between 15 and 19 years), which brings us only to 1612-1616. We can therefore see that more than 80 years have slipped away from the lists here, and precisely from the period when Assyria lost all independence and was an ordinary Hurrian province. It is also very characteristic in this connection that from the above-mentioned Puzursin — about whom the lists are also silent — until about 1550 we do not know of any inscription from any Assyrian king. 111. With PuzuraSSur's predecessor, ASSurnerani I (about 1559-1533), royal inscriptions appear again (cf. RLA vol. I, p. 247; Soden: Propyl. Weltgesch. vol. I, p. 597). It is therefore clear that around 1550, when the Hurrian empire, which had been more or less unified until then, was finally divided into several parts with the independence of Mitanni, Assyria also acquired a certain degree of autonomy, relative independence, at least to the extent that it was no longer governed by foreign governors, but by native nobles who came to the throne — although undoubtedly still under Hurrian-Mitanni dependence. Let us now count back in time from Samsiadadi II, using the data of Sarhaddon in 1700 as a basis, for the time being only to the accession of the already known son of Adaši, Belbani, to the throne.

In the king lists we find 124 years for this period, i.e. with Belbani we arrive at 1824, 11 years earlier than the beginning of the reign of Samšiadad I in the middle chronology. Whereas — considering only the data of the lists — before Belbani, Adaši and his six companions fought for the throne for 6 years, before them, I, ISmedagan reigned for 40 years, his father, I. Samšiadad and 33 years. This is a total of 79 years, so Samšiadad I ascended the throne around 1903, about 90 years before the Middle Chronology. However, if we add the unknown but datable reigns of Asinum and Puzursin — for which two kings Wv Soden himself, one of the most famous representatives of the Middle Chronology, calculates 12 years (cf. Soden: Propyl. Weltgesch. Vol. I, p. 584), we arrive at the accession of Samšiadad I to the throne around 1915. And in this calculation we have not yet taken into account that again, according to Soden, the period of the seven kings who competed for 6 years in the lists must be greatly extended, not including six of the seven kings, Adaši, among them ~ himself about 30 years

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(v6, Soden, Propyl. Weltgesch. Vol. I, 584. jyy.), If we apply this consideration of Soden to our above number, 8 to the aforementioned date of 1915 we add 30 years for the six kings in question, then 15 58088080 ascended the throne around 1945, i.e. with an accuracy of almost a year we have arrived at the dating of the long chronology, according to which Hammurabi began to reign in 1930, and 1. 580518086 in 1951,

Moreover, even if we do not accept the date of ASarhaddon - as the supporters of the middle chronology also consider to be an error in the 1612-1616 --, but we remain IL Samšīadad ascended the throne date calculated from the king lists - the beginning of the reign of Samšīadad I would still be only 84-88 years after 1945, i.e. still almost half a century before the dating of the middle chronology - and this is precisely based on the above considerations of Soden, who works with the middle chronology. Finally, it is essential to mention that the information in the Assyrian-Babylonian contemporaneous king list (not the chronicle!) according to which IL Sam. Siadad was a contemporary of the 2nd king of the Kassite dynasty, I. Agum, can only be correct if we give credit to ASarhaddon and place IL Sam. Siadad between 1700-1694 (he reigned for 6 years). I. Agum's reign — whether we take the long or the medium chronology — is definitely around 1713-1691 (+8) (we have already seen that the first Kassus king, Ganda\$, ascended the throne around 1739+8; after his 26-year reign, Agum I followed 22 years), with the only difference being that according to the long chronology he ruled over all of Babylonia, while according to the medium chronology he ruled only in the northeastern territories (cf. above, p. 754). However, if we take the reign of Samšīadad II, which can be calculated from the king lists, i.e. around 1616-1610 or 1612-1606, he does not come even close to Agum I in any chronology.

Ultimately, a more thorough examination of the Assyrian king lists and other sources led us to the same conclusion, which was already clearly evident after reviewing the data on the Kassus and Sealand dynasties: all these sources, which are close in time, can be reassuringly fitted only into the framework of the long chronology.

After this, we can move on — moving backwards in time — to determining the chronology of the Isin-Larsa period preceding Hammurabi, the IIIrd Uri dynasty, the Gutian rule, and the Akkadian period, now only taking into account the dates of the long chronology, which can be considered the only realistic one. Establishing the absolute chronology of this period of more than half a millennium is much less difficult than fixing the events of the middle of the 2nd millennium, the so-called "dark ages". The reason for this is that, in a strange way, we have much more contemporaneous data from these much older times than we do for the Kassite, the Sea-born and the Assyrian dynasties.

===== PAGE 751 =====

in the 2nd millennium BC. This striking disproportion can be explained mainly by the fact that the primitive, systematic and unmethodical excavations carried out in the second half of the 19th century without any stratigraphic observations, the aim of which was primarily treasure hunting in the strict sense of the word, i.e. the extraction of as many attractive or exotic museum pieces as possible, have now irreparably disturbed the upper layers extending to the middle of the 2nd millennium at many Mesopotamian sites, and thus the scientifically satisfactory excavations of the 20th century have mostly found only the older layers undisturbed - and this is precisely at the most important sites, in the largest cities, because the treasure hunters of the 19th century naturally stuck their picks into the most significant rubble mounds, since they expected the most plentiful spoils from these.

In short, the internal chronology of the period from the Akkadian period to the end of the Isin-Larsa period can be clearly determined with only a margin of error of a few years. For example, we know that the first king of the Isin dynasty, Ibierra, ascended the throne about 224 years before Hammurabi (with an error of 2-3 years), and Naplanum, the founder of the Larsa dynasty, 9 years before ISbierra. On the other hand, it is known that ISbierra already called himself king in the 11th year of the last king of the III. Ur dynasty (Ibbisin). Since Ibbisin reigned for 24 years, it can be easily calculated from this that the III. Ur dynasty fell about 211 years before Hammurabi ascended the throne, that is, according to the long chronology, about 2141. The total duration of the Ur Dynasty II varies by 16 years in the various versions of the king lists (between 105 and 121 years). However, since only 108-109 years can be given from contemporary sources, research almost universally estimates 108 years for this dynasty. Thus, the accession of the first king, Urnammu, to the throne can be dated to ca. 2249. Before him, Utuhegal, the only king of the Uruk Dynasty V, ruled for 7 years, after he had driven the Gutians out of Mesopotamia. The end of Gutian rule is therefore dated to ca. 2256. The total duration of the Gutian dynasty — and even the number of kings — also varies in the lists. According to one version, 21 kings ruled for a total of 125 or 124 years, in other texts, 23 kings for 99 years. However, if we add up the legible names and reign years of the list group mentioning 21 kings individually, we get not 124-125 years, but 91-93 years for 20 kings (the name and reign of one king are illegible) (the reigns of two kings vary by one year). It therefore remains questionable whether the Gutian dynasty had 21 or 23 kings. Based on the total reign of 91/93 years for the 20 kings, each king reigned for 4-5 years, or about 21 kings. 96/98, 23 would be about 105/107 years. Thus, research generally places the duration of the Gutian dynasty at around a century (cf., e.g. RLA III, vol. 713, p.), and the kings are not real rulers, but military leaders re-elected every 3-4-5 years. Thus, the beginning of the Gutian dynasty is placed around 2356.

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0 165 However, the question of whether the reign of the first king of the Gutian dynasty coincides with the beginning of the Gutian Mesopotamian hegemony is already known; in several cases the Kirdly lists show dynasties that were partly ruling simultaneously as succeeding each other. This is especially common in the 3rd millennium, 8 this is also the case with the Gutian and Akkadian dis nastia.

Before explaining this, however, we must briefly outline the relevant data from the Sumerian king lists. The lists include eleven kings in the Akkadian dynasty: 1. Sarrukin (Sargon) 56 years, 2. Rimus 9 years, 3. Manistusu 15 years, 4. Naramsin 37 years, 5. Sarkaligarri 25 years, 6-9. Elulu, Igigi, Iri, Nanum, who competed for power simultaneously for 3 years ("who was king, who was not king?" - the lists ask), 10. Dudu 21 years (a total of 181 years). Then - in the warriors of the lists - Akkad was defeated by the sword, the kingdom passed to Uruk". Then five Uruk kings follow, a total of 30 years (the IV Uruk dynasty), and again the scheme: Uruk was defeated by the sword, the kingdom passed to the Gutians".

On the other hand, we have contemporary and close-to-time sources that the Gutians — and the Hurrians in the north — were already repeatedly attacking in the last years of Naramsin, the 4th Akkadian king, and Naramsin could only defend himself against them with difficulty. In other words, the disintegration of the Akkadian empire had already begun at that time (cf. e.g. Hrouda 1971, p. 130). According to the Naramsin legend, which originated shortly after the Akkadian period, for example, this king sent three armies (180,000, 120,000 and finally 60,000 warriors) one after the other against an enemy alliance, and all three armies were completely destroyed. Likewise, the prophetic texts — which are very reliable in this respect, since in the view of the time they were the basis for the most accurate prediction of future events — also rank Naramsin among the unfortunate rulers (cf. Soden: Propyl. Wetgesch. Vol. I, p. 550).

All this suggests that the Gutians had already established a foothold in the northeastern regions of the Akkadian empire in the last years of Naramsin, and that at about the same time, but no later than the death of Naramsin, Uruk also managed to gain its independence (the IVth dynasty of Uruk in the records). These data are strongly confirmed by the results of recent research, according to which the Gutians invaded the Akkadian empire during the reign of Naramsin's son, Sarkalisarri, and at that time they also destroyed the capital, Agade (cf. Lloyd 1981, p. 177). Although this attack resulted in the collapse of the Akkadian empire and the end of the Akkadian hegemony over Mesopotamia, it did not mean the end of the Akkadian dynasty. Sarkalisarri — apparently extending to the northwestern territories of the former empire — continued to exist for many years.

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He maintained himself through his sons, similarly to the last Assyrian kings who, after the capture of Assur and Nineveh (614 and 612 BC), ruled the Syrian region for another 6-8 years, with Harran as their seat. This is exactly what the surviving name of one of Sarkalisarri's reign years indicates: "year in which Sarkalisarri defeated the western countries in Bashar". At the same time, two further names of Sarkalisarri's years testify to the independence of Uruk, and indeed of all of Southern Mesopotamia: "Year in which he fought against Uruk and Naxu" — note: "fought" and not "defeated them"! --, and: jyy, in which Sarkalisarr marched against Sumer" (again just "marched"!). The year names are found in 1. RLA Vol. II. p. 133.

From a chronological point of view, however, the data that is also known from one of the year names of SarkaliSarri is much more important than all this, and it attests to the partial contemporaneity of the Akkadian and Gutian dynasties: "in which Sarkalisarri subdued Sarlag, king of Gutium" (I. RLA vol. II. p. 133). It is obvious that this Sarlag is identical with the 3rd Gutian king of the lists, Jarlagaba (variations: Zarlagaba and Sarlagaba), or possibly with the 4th king, Jarlagass. This therefore proves that the first kings of the Gutian dynasty ruled simultaneously with SarkaliSarri.

But we can go further. We mentioned that according to the king lists, after the death of SarkaliSarri, four "Akkadian" kings vied for the throne for 3 years. Well, one of these was named Elulu, and this king is in all probability identical to the 6th king of the Gutian dynasty, Elulu-meS (cf. e.g. Müller-Karpe 1974, p. 44). The first four Gutian kings ruled for 21 or 22 years according to the lists. The name and time of the fifth king are not known (illegible), but since only one of the twenty known Gutian kings ruled for more than 7 years, and the total duration of the dynasty also indicates an average reign of about 4-5 years, we can rightly expect 4-5 years for the fifth king as well. That is, by the time ElulumeS\$ ascended the throne, about 25-27 years of the total duration of the Gutian dynasty had already passed. Elulumes himself ruled for 6 (or 7) years, and if he was only for 3 years, it undoubtedly means that he was king of Gutium for 3-4 years "Akkadian" king (according to the lists), this before Sarkalisarri's death, and in his last 3 years, after Sarkalisarri's death with whom Sargon's dynasty died out — he also acted as a pretender to the Akkadian throne. This consideration results in the fact that the beginning of the Gutian dynasty must be placed approximately 28-31 years before the death of SarkalsSarri (25-27 years for the first five Gutian kings and 3-4 years for Elulume' until the death of SarkaliSarri), i.e. in the last 3-6 years of Naramsin. This dating, however, coincides with the already known data of the Naramsin legend, with an accuracy of almost a year, that Naramsin lost three battles in the last years of his reign — evidently against the Gutians and their allies. In these years, therefore, the Gutians undoubtedly occupied certain eastern-northeastern parts of the Akkadian empire, and therefore the lists count the Gutian dynasty from this point on.

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a That is, we should not take the duration of the Gutian hegemony as the same as the Sssside of the dynasty, but we should deduct 28-31 years from it. This is the period during which they competed with the Akkadians for the supremacy over Mesopotamia, namely in the last years of Naramsin and during the 25-year reign of SarkaliSarri. The 30-year independence of the IV. Uruk dynasty also falls, with almost a year's accuracy, in the same period, which largely confirms the correctness of our above reasoning. On the other hand, it also follows from all this that the total time of the seven Akkadian kings following Naramsin in the lists (64 years), as well as the last 3-6 years of Naramsin, must also be subtracted from the duration of Akkadian hegemony, because in this 67-70-year period we can no longer speak of Akkadian rule in all of Mesopotamia.

Chronologically, the above results in the fact that the last 67-70 years of the Akkadian dynasty of the king lists ran parallel to the first 67-70 years of the entire Gutian dynasty. 2/3 (cf. RLA vol. VI, p. 81). That is, for the period of the Akkadian dynasty, when the Akkadian kings still ruled all of Mesopotamia without rivals, we can only count about 111-114 years out of the 181 years according to the lists — with the following restriction. In this way, Sargon ascended the throne around 2467-2470 (around 2356 — the beginning of the Gutian dynasty — minus 111-114 years). However, this date does not at the same time mean the beginning of the Akkadian hegemony in Mesopotamia, because it is well known that Sargon was initially only the king of the city of Kis for a long time, and had to fight a series of wars until — according to the Sargon legend — he defeated the then high king, Lugalzagezi, and . the kings of the city-states subject to him (cf. e.g. Hrouda 1971, p. 129). | According to these, the periodic division of Mesopotamian prehistory based on the long chronology is as follows.

ÿ 3250/3200 | Jemdet Nasr era 3250/3200-3000 ÿ . Early Sumerian Dynasties ca. 0 3000-2455 | Sargon enthroned at Kisben ca... 2470 | Akkadian hegemony ÿÿ...mann 2455-2356 . Transitional period (Akkād-Gut struggle for supremacy, | IV. Uruk dynasty) ca. 2356-2326 | Gutian hegemony -----%. 2326-2256 IV, Uruk dynasty ca. 2256-2249 ÿ UJIII. Ur dynasty ca. ...eesenesnessessessenenenunneenesenen csscscssssssssssssssssssssssssssssssssssveaes 2141-1900 | Hammurabi ascended the throne in the city of Babylon ca. 1930 | The hegemony of Hammurabi's dynasty (babylon choir) KD. 1900-1733 BC 765 ===== PAGE 755 ===== Maritime dynasty in Del-Mesopotamia ca. 1860-1492 Kassian era ca. .4-4--7-.....77--. [0-0 1733-1163 Sumerian cultural and ethnic influences during the so-called "early Sumerian dynasties" (c. 3000-2455) We saw above that Mesopotamia, poor in ores and other raw materials, was already in need of large-scale ore and metal imports during the Eridu-Ubaid period and even more so during the Uruk-Jemdet Nasr period. Now, with the continuous rapid development of culture, the growth of population and the increasingly widespread use of metals, notably bronze, in everyday life, the need for ores assumed ever greater proportions at the turn of the 4th/3rd millennium BC and after, and a continuous, uninterrupted supply of raw materials became a matter of life for the Sumerian city-states and empires — just as oil is today. This is also proven — indirectly, but with great probability — by the most ancient Sumerian epics, the heroic tales woven around the figures of the Uruk kings Enmerkar, Lugalbanda, and Gilgamesh, which almost without exception detail the vicissitudes of expeditions and caravans launched to procure various raw materials, enclosing these adventurous and dangerous enterprises heading to distant lands in an epic framework (cf. e.g. the plot of the ancient Sumerian epic "The Lord of Enmerkar and Aratta"; 1. above, pp. 695-696). It is in the knowledge of this cultural and economic background that we must view the archaeological finds of the centuries around the turn of the 4th/3rd millennium BC and after in the near and far regions surrounding Mesopotamia.

We have already mentioned that after the Uruk pottery and Mesopotamian seal cylinders of the Iranian Tepe Sialk IV period and the Godin Tepe V layer, which appeared around 3400-3200, Sumerian-Elamite pictograph tablets also appeared in both places between 3100-3000, and that the presence of Sumerian-Elamite colonists at Godin Tepe at this time can be dated (1. p. 729), then starting around 3000, plain-colored glazed gray pottery, strongly similar to the earlier Uruk ware, became dominant throughout northern Iran, from Godin Tepe to Turkmenistan, the shapes and decorative motifs of which act as imitations of metal vessels (l. p. 731 and Hrouda 1971, p. 127, note 4). We have seen that at the same time, metalworking and culture in general flourished in these Iranian areas (cf. e.g. Hrouda 1971, p. 127). In northern Iran, but also in such distant regions as Turkmenistan or Afghanistan, not long after, distinctly urban settlements arose in several places (e.g. Tepe Hissar, Turang Tepe in northern Iran; Altin Tepe, Namazga Tepe in Turkmenistan; Mundigak in Afghanistan), the use of copper and seal-printing became widespread. Research clearly shows all these phenomena.

766 ===== PAGE 756 ===== He writes about the expansion of Sumerian culture, and in part 8 he explains it with the consolidation of the lines of the lapis lazuli trade (see our book, pp. 27-28, also). However, it cannot be ignored in this Usseffigés that we do not know of any tin deposits in the immediate vicinity of Mesopotamia. The nearest tin mines are in Khorassan, around Mesed (northeastern Iran), and research generally derives this essential raw material for early Sumerian bronze art from there (cf. e.g. Müller-Karpe 1974, p. 126). Thus, tin, which soon became even more important, was also associated with lapis lazuli. Mesed lies near the Turkmen border, along the northern lapis lazuli route leading to Afghanistan, not far from Namazga Tepe and Altin Tepe.

Undoubtedly, the large-scale Mesopotamian colonization of Turkmenistan around 3000 BC can be attributed to these important trade routes, as well as the fact that close ties with Mesopotamia were not broken after this, and the slow but continuous infiltration of Mesopotamian settlers into Turkmenistan continued throughout the 3rd millennium (cf. e.g. Jettmar et alii 1966). These constant cultural and ethnic contacts eventually led to the appearance of the characteristic Sumerian stepped tower temples themselves in northern Iran and southern Turkmenistan in the second half of the 3rd millennium, e.g. at Turang Tepe and Altin Tepe (cf. p. 28 of our book). These tower temples were undoubtedly modeled on the Sumerian ziggurats, which is clear evidence of the deep intensity of cultural contacts (cf. e.g. Amiet 1977, p. 137).

However, these phenomena belong to the next era, so we will deal with them in more detail later. Further east, in Afghanistan, in the first half of the 3rd millennium, we can observe phenomena very similar to the situation in northern Iran and southern Turkmenistan. The Mundigak IV period corresponds in time to the Tepe Hissar ÿÿ/- III/A layer and the Namazga III-IV period, i.e. it belongs to ca. 2700-2350. In this period, the cultural picture of the earlier layers changes fundamentally: at such a great distance from Mesopotamia, we encounter for the first time a real urban settlement, surrounded by a wall fortified with towers and bastions. In addition to numerous spacious residential buildings, a truly monumental palace (ca. 40x40m) and a temple (ca. 30x30m) of also very impressive size were excavated (cf. Müller-Karpe 1974, Tafelband, plate 707). The built-up area increased enormously during this period: the mound of rubble created by the superimposition of earlier settlements is almost entirely occupied by the aforementioned palace alone (cf. Müller-Karpe 1974, pp. 368 and 1039-1040). All these phenomena can undoubtedly only be explained by direct emanations of Mesopotamian-Sumerian high culture, emphasizes Müller-Karpe (1974, p. 491).

767 ===== PAGE 757 ===== In other words, in Mundigak, where since the end of the Eridu-Ubaid period we could observe strong Mesopotamian influences (l. p. 725), which are obviously connected with the exploitation of the azure deposits in Afghanistan and the organization of transport, a real high culture developed at once without any transition during the early Sumerian dynasties, the former modest settlement grew into a large, walled city. There is no doubt that we must connect this surprisingly rapid cultural development with the appearance of Sumerian groups from Mesopotamia -- as Müller-Karpe clearly states. (1. above).

We have already briefly mentioned that research attributes a very significant role in the emergence of the Indus civilization (Harappan culture, between ca. 2500-1500) to the Mesopotamian impulses that came from both directions and met in the region of Afghanistan and Baluchistan (northern and southern blue-green routes; cf. pp. 27-28 of our book). And not without reason. The first discovered settlement of the Indus civilization, Mohenjo Daro, was discovered by M. Wheeler, who pointed out emphatically that the cities of the Indus culture were clearly planned, as colonial settlements. The straight streets of Mohenjo Daro, intersecting at right angles, are reminiscent of the Greek colonial cities founded in Hellenistic times. In this connection, R. Heine-Geldern showed in 1955 that such an urban culture could only have originated in Mesopotamia in the middle of the 3rd millennium. The cultural flow from the west reached the Indus region from two directions. One route led through southern Iran and along the coast of the Indian Ocean, and the other from northern Mesopotamia and eastern Anatolia reached India via the northern edge of Iran, southern Turkmenistan and Afghanistan. — Heine-Geldern recognized all this long before the discovery of the aforementioned lapis lazuli routes. Moreover, he also noticed that the cultural and trade relations of that time had assumed almost unimaginable dimensions. For example, undoubted Cretan and early Minoan (ca. 2600-2000) parallels could be found in the finds of the Indus culture — he states (the above is I. Heine-Geldern 1955.)

We will see below that these parallels between the Indus and Crete can be traced back to the fact that the Sumerian civilization had close cultural and commercial relations with both areas, and that — similarly to the Indus civilization — Cretan culture also has many features that point to a certain degree of colonial dependence on Mesopotamia in the initial phase of this civilization, called the Early Minoan period.

As a characteristic episode, we also mention that the above-mentioned planned urban architecture, which was first observed in the settlements of the Indus civilization, is still being observed today, more than 50 years after the discovery of Mohenjo Daro, in the "Hip-768"

===== PAGE 758 ===== "podamos-like city plan" is what the Eurocentric cultural historian calls it (after Hippodamos of Miletus, who lived in the 5th century BC), despite the fact that we have known such planned city plans older than the 5th century not only since the discovery of the Indus culture, but we can also read in several ancient Roman authors that the oldest Etruscan cities were built according to exactly such a plan, a good 2-3 centuries before the birth of Hippodamos. Looking further west, in the area of Lake Urmia and in Anatolia at the beginning of the 3rd millennium BC, we find connections similar to the previous ones. The expansion of Sumerian culture in these areas can also be traced by the characteristic grey or black, sometimes reddish, single-coloured glazed ceramic finds, very similar to the Uruk ware.

First, however, we must say a few words about the Anatolian about the concept of "Early Bronze Age". In reference to Asia Minor, the analogy of the Mesopotamian periodization — where Bronze Age, which we can rightly speak of since the beginning of the Jemdet Nasr period -- the term "Early Bronze Age" has long been established in archaeology, and is also counted from the beginning of the Jemdet Nasr period -- i.e. starting with 3200, 3100/3000 or 2900, depending on whether the researcher in question accepts the long, middle or short Mesopotamian chronology. However, we have shown above that a thorough analysis of the Egyptian parallels of the late Uruk and Jemdet Nasr periods has recently led to the conclusion that only the long chronology deserves credit (1. pp. 750-751). Accordingly, the Anatolian is structured as follows. "Early Bronze Age" chronology

Phase I — corresponds to the Jemdet Nasr period — ca. 3200-3000; Phase II — corresponds to the early Sumerian dynasties — ca. 3000-2450; Phase III — corresponds to the Akkadian and Ur III periods -- ca. 2450-2150. It is another matter whether, apart . The term "Early Bronze Age" starting at the same time as the Jemdet Nasr period cannot be called accurate terminology, because the bronze in Anatolia — one or two from the rare Anatolian exception, such as Alisar, where metal objects cast from copper mixed with varying amounts of tin were found from the turn of the 4th/3rd millennium (cf. Müller-Karpe 1974, 427).

p.) — became generally widespread only in the middle of the 3rd millennium. For this reason, many people — consistently — call only the III. stage the Early Bronze Age, and between 3200-2450 they speak of the Anatolian Copper Age (cf. e.g. Müller-Karpe 1974, 111-112, p.). This would undoubtedly be the correct terminology. We should also be aware that most available summary works still place the end of the Anatolian Early Bronze Age — under the influence of the previously used short Mesopotamian chronology — at around 1900 BC, and consequently, the dates of the earlier stages are also a good 2-3 centuries lower. Note however, that 769

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Some sharp-eyed researchers noticed already during the almost general rule of the short chronology that the Anatolian Early Bronze Age ended well before the turn of the 3rd/2nd millennium BC. Thus, for example, A. Goetze emphasized in 1957 that the end of the Anatolian Early Bronze Age must be dated to around 2200. Since the periods III-V of Troy still belong to the Early Bronze Age and represent a period of at least 300 years (a total of 12-13 layers!), we cannot in any case date the end of Troy II -- following K. Bittel and F. Matz -- to 2200, or even to 2300 -- as e.g. S. Weinberger -- but must date it considerably earlier, he states (1. Goetze 1957, p. 35). The Troy I period therefore began around 3000 — as the excavator of Troy, C. Blegen, also claims —, Troy II around 2750, Troy III around 2450 — concludes Goetze's chronological discussions (1. Goetze 1957, p. 36). —

We can see that Goetze's dates fit exactly with the long chronology in recent use. The oldest pottery from Troy I, Alisar, Alaca Hüyük, and Kültepe mentioned above (1. 746, p.) was still freely shaped by hand. However, not long after, in the first half of the 3rd millennium The rapidly rotating potter's wheel appeared from the southeast in the second half of the 3rd millennium (cf. Hrouda 1971, p. 102). In the same period, profound changes took place in Anatolian metalwork. The excellent quality of Anatolian metal objects from the middle of the 3rd millennium (e.g. Troy II, Dorak, Alaca Hüyük, Horoztepe, etc.) testifies to the surprisingly high level of metalwork. This is also confirmed by the forms and decorative elements of ceramics imitating metal vessels, which can be observed particularly well in Beycesultan in western Anatolia and Karahüyük (near Konya) in central Anatolia — states Hrouda (1. Hrouda 1971, p. 102). The metal wealth of Anatolia exerted a great attraction on the surrounding peoples. Primarily, Mesopotamia, which was culturally superior, exploited the raw materials. Many early Bronze Age Anatolian cities of the 3rd millennium BC owe their origin and significance to such Mesopotamian connections, such as Tarsus in Cilicia or Gedikli and Tilmen Hüyük in the Islahiye area, etc. (cf. Hrouda 1971, p. 102). In this context, the already mentioned circumstance that genuine bronze appears sporadically in Alisar at the very beginning of the 3rd millennium, with varying tin contents, sometimes reaching 10% (cf. Müller-Karpe 1974, p. 427; also I. above, p. 769) is very telling.

It has been a generally accepted position in the relevant literature for a long time that in the 3rd millennium, all of Anatolia, Transcaucasia, and even the Kuban region all received their impulses for their own development from Sumerian civilization. Moreover, in Eastern Anatolia, the strong emanations of the Sumerian center in Mesopotamia can be considered proven from the second half of the 4th millennium, as K. Bittel asserts, as early as 1950)1, Bittel 1950). Since then, we see these dominant Mesopotamian-Sumerian influences in a much more nuanced light in the light of recent excavations. Most importantly: 770

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It has been proven beyond doubt that the population of Anatolia was practically completely replaced starting from the turn of the 4th/3rd millennium BC. While the long-headed Euro-African and Mediterranean type predominated during the Neolithic and Chalcolithic periods, the short-headed Proto-Asian type became increasingly widespread in the 3rd millennium BC, during the Early Bronze Age. This anthropological picture clearly indicates that a large number of short-headed immigrants arrived in Anatolia in the 3rd millennium BC — states Alkim (cf. Alkim 1968, p. 82). But this is also proven by the fact that, contrary to the previous lack of finds, dozens of settlements founded around the turn of the 4th/3rd millennium BC and after have been excavated in Anatolia, and the population increased greatly in the Early Bronze Age (1. Alkim 1968, pp. 122-123 and ff. to p. 147). At the same time, the parallels of the finds from these new settlements clearly indicate colonists arriving from the east. Many types of finds previously thought to be of Western origin (mostly the obsession with Indo-Germanic "Herrenschicht" migrations haunted these find definitions), or at least assessed as the result of autochthonous Anatolian development, have turned out to have been brought to Anatolia from the East during the Early Bronze Age. This is the case even in the case of the most western sites. The Trojan culture, for example, arose quite suddenly, and in its finds we find many samples of Eastern Mesopotamian origin (cf. e.g. Matz 1958). Or the surprisingly accurate correspondences of the characteristic faceted vessels that appeared in the Troy II period, around 2600, appear in Hassuna and Sauwan as early as the 6th millennium, in the Hacilar VI-I layer around 5000, and then in the Early Bronze Age in the first half of the 3rd millennium in the Konya Plain. (e.g. Karahüyük) (cf. Alkim 1968, p. 116). According to these findings, the primary center of faceted vessels must be sought in the region of Northern Mesopotamia-Northern Syria (cf. RLA IH. vol. 299, p. 299).

Similarly, the so-called "depas amphikypellon" (two-handled cup) that appears in the Troy II period between 2500-2400 (layer IId), which until now has generally been derived from the west, also reached Troy from the southeast. Simultaneously with Troy II, it occurs on the northern Syrian coast in the Amug I period, in Tarsus, Mersin, Alaca Hüyük, Alisar, Kültepe, Beycesultan, etc., etc., but what is most important: in the vicinity of Islahiye, near the Syrian border, according to the results of the latest excavations, the two-handled cups are older than the Troy II period (cf. Alkim 1968, pp. 116-117).

The earliest antecedents of the Trojan "megaron" structures, previously considered to be of clearly Western origin, are also found in the east. Although they appear in Thessaly as early as the 4th millennium (Dimini culture), and in Western Anatolia they are found in the second half of the 4th millennium before the founding of Troy (e.g. in layer XXIV of Beycesultan), they can be continuously followed eastward in the Early Bronze Age layers of Karatas-Semayük and Kültepe all the way to Tell Chuera in northern Syria (cf. Hrouda 1971, pp. 100-101). 771

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Likewise, the already known Uruk temples of Tepe Gaura (around 3600 BC) are also indisputably "megaron" structures (cf. Hrouda 1971, p. 82), and in Kilikia the megaron appears already in the XXth layer of Mersin, around 5000 BC (cf. Hrouda 1971, p. 37). All these data clearly show that long-distance trade, the carefully organized and planned procurement of raw materials that continuously ensured the needs of the mother country, played a very important role in the spread of many characteristic features of early Sumerian civilization to distant areas. The finds of recent decades have proven beyond any doubt that we must completely reject the traditional views previously held about the relatively limited connections and even isolation of Near Eastern cultures, and especially of Sumerian civilization (cf. e.g. Schmökel 1963, p. 105).

On the other hand, it can be proven - also beyond all doubt - that the Sumerians operated a vast long-distance trade network in the 3rd millennium. They had regular trade relations with South Arabia, Abyssinia, Egypt, India, Anatolia, the Caucasus region and, not least, with the interior of Asia Minor, especially the Caspian and Aral Sea regions, as well as with Afghanistan (Schmökel 1963, p. 105 map). Hrouda also states that, according to the evidence of the Tatar finds, the trade relations of the Sumerians already reached Transylvania around 3000 (cf. Hrouda 1971, p. 103). Finally, we also pointed out that under the conditions of that time, the enormous demand for raw materials — primarily metals — that the Sumerian high culture of Mesopotamia constantly required could only be ensured continuously and without interruption for more than a millennium if a considerable number of colonial settlers were sent to the ore deposits, who could ensure the smooth progress of mining, ore smelting and the transportation of metals (1. pp. 747-748). Similarly, bases were needed on important sections of the trade routes — passes, river crossings, straits, transshipment ports, etc. — and — last but not least — good and lasting connections had to be established with the indigenous peoples living around the ore deposits and along the routes, so that the shipments could actually reach Mesopotamia. - These obvious things have not been given due attention by research so far, although there is no doubt that each of the listed links was an essential prerequisite for any kind of trade to develop at all in the ancient times in question, and in the absence of even one such link, organized long-distance trade would have been simply unthinkable at that time.

It is in the light of these considerations that we must examine Sumerian long-distance trade in the 3rd millennium BC, as well as the processes described below.

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striking phenomena, which can be observed in archaeological finds from the second third of the 3rd millennium, around the 27th century BC, primarily in Anatolia, but also in the Aegean region, almost the entire Mediterranean, and even on the shores of the Atlantic Ocean. Since industrial metalworking and bronze casting can be demonstrated for the first time among the Sumerians (cf., e.g. Müller-Karpe 1974, pp. 425-426), 8 because the most important issue of Sumerian civilization was the continuous provision of the ever-increasing need for metal, our attention will naturally be directed mainly to metalwork and its Mesopotamian-Sumerian parallels, as well as to bronze finds in the areas to be examined.

Let us first review the Anatolian archaeological record in the middle of the 3rd millennium (see also map 1 and 6). We have already mentioned that around the middle of the millennium there was a surprising and incredibly rapid leap in development is evident in Anatolian metalwork (1. p. 770). Moreover, if the not yet definitively published grave finds from the area of Dorak (east of Troy) prove to be authentic, then we must speak of fully developed metalwork before 2600. According to the preliminary report, statuettes made of electron (a gold-silver alloy), silver and tin bronze, artistically chiseled ornamental weapons (e.g. a silver sword decorated with engravings of Cycladic-type ships) and metal vessels were found in the Dorak tombs, and in one tomb the framed "calligraphie" (cartouche) of the second pharaoh of the 5th Egyptian dynasty (Sahu-re) was also found (cf. Hrouda 1971, p. 128). According to the short chronology, Sahu-re reigned around 2450 (1. Müller-Karpe 1974, p. 774), which corresponds to about 2650 in the recently increasingly common long chronology. However, it should be noted that Hrouda, who also calculates with the short chronology, dates Sahu-re's reign to about 2500. (cf. Hrouda 1971, p. 128). According to these, the Dorak finds date back to 2700-2600.

The most magnificent pieces of Anatolian metalwork were found in the royal tombs of Alaca Hüyük. So far, 13 tombs are known, from which a rich mass of gold, silver, electron, copper and bronze accessories was recovered. Dating the tombs was difficult for a long time, but today it is certain that they belong at least to the 27th-24th centuries BC, but probably even earlier. The age of the layer above the tombs — that is, younger — is given by two CS measurements as 2590±56 and 2250±58 BC (1. Narr 1975, p. 511). Similarly, the early dating is also proven by the Khirbet-Kerak ware found in the graves, which we already know was widespread in Anatolia, Syria and the Amug plain in the first half of the 3rd millennium, where it occurs in the Amug H stage (c. 3000-2500), since c. 2700 (1. p. 105 — but cf. Goetze 1957, p. AO. also). The royal tombs of Alaca Hüyük are shaft tombs built with stone and covered with beams. The splendor of the finds surpasses even the famous treasure finds of Troy. The metal objects are unmistakable in terms of craftsmanship, but also in their shapes.

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are closely related to the Mesopotamian ironwork of the early Sumerian dynastic period, above all to the metal accessories of the royal tombs excavated in KiS and Ur vārosā, while some features rather reflect local forms (cf. Müller-Karpe 1974, p. 123). Such local forms include, for example, the animal-shaped decorations that could be mounted on poles, generally interpreted as military insignia, and the openwork bronze discs, which could also have crowned pillars supporting canopies or been accessories of ceremonial chariots (cf. e.g. Müller-Karpe 1974, p. 698).

These close parallels with the finds from the royal tombs of 18 and Ur dating from ca. 2800-2600 are indeed very striking. They extend to numerous vessel shapes (jugs, cups, goblets, glasses), decorative motifs (e.g. grooved; "kannclure"), the combination of metals (tin bronze, electron), the tool kit (vessels, figurines, daggers, axes, hatchets, spearheads), sometimes to the similarity of form that goes as far as identity (e.g. in the daggers; cf. Narr 1975, p. 512, fig. 163), and to the techniques of metalworking (e.g. granulation, filigree technique, inlay technique; cf. Narr 1975, p. 511). Two iron spears with gold handles were also found at Alaca Hüyük, exact copies of which — one also with an iron blade — are also found in the Uri royal tombs (1. Narr 1975, p. 512, fig. 163).

Other finds, such as ivory, turquoise, lapis lazuli, and amber, also prove extensive trade relations. The nearest deposits of these raw materials are in Egypt (ivory), the Sinai Peninsula, or Iran (turquoise), and Afghanistan (lapis lazuli), while amber is found on the shores of the Eastern Sea (cf. Narr 1975, p. 511). This picture of finds clearly shows that Alaca Hüyük—and also Troy II, where very similar pieces were found in the treasure finds—was organically involved in the Sumerian long-distance trade of the 3rd millennium. Both settlements were undoubtedly intermediate stations or bases in this trade network. Amber, however, apparently reached Anatolia through the Tripoli civilization, meaning that trade had already extended to distant parts of Europe by this time — as evidenced by finds from Tartárlak and other Transylvanian-Banca finds dating back to around 3000.

Finally, it is absolutely necessary to mention — as a circumstance not uninteresting to us — that the earliest known partial animal burials were found in the princely tombs of Alaca Hüyük. In the burial chambers themselves, large empty spaces were repeatedly observed among the deposited accessories, which makes it very likely that there was decaying material there, which disintegrated without a trace — perhaps carts. This assumption is confirmed by the fact that on the roof of the tombs, on the beams covering the chamber, in several cases cattle hooves (between two and ten) and shin bones were placed jv - va- 1647100168 together with the skin of the animals (cf. e.g. Narr 1975, p. 512; Müller: Karpe 1974, p. 697-698)

This burial rite is also Alaca Hüyük

774

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proves its organic Mesopotamian connections. The earliest burials with chariots and beasts of burden are known from the rich tombs of Ur and Kis dating back to 2800-2600 BC. The precious metal finds of Alaca Hüyük are organically connected in time and form to the treasure finds from the Troy II period, which some date to between 2750-2450 (see, e.g. Goetze 1957, p. 36), others to between 2500-2300 (cf. Alkim 1968, p. 251: chronological table). In any case, it is certain that the beginnings of the rich Anatolian treasure and tomb finds must be placed before the middle of the 3rd millennium (1. e.g. Narr 1975, p. 509 — but also cf. the age of the Dorak finds mentioned above). In addition to the close parallels with Troy II, however, we also find very close correspondences to the characteristic metal finds of Alaca Hüyük to the north and east. On the southern Black Sea coast, the rich graves of Mahmatlar, Horoztepe, Kayapınar, Düdartepe and Giresun yielded essentially the same assemblages as those at Alaca Hüyük. The age of these finds corresponds to the later graves of Alaca, ca. to the 24th century (1. e.g. Narr 1975, p. 514; Alkim 1968, p. 146), and they clearly show the route by which the Central Anatolian cultural radiators reached the Kuban region between 2400-2300, where we find in the princely kurgans of Maykop many similar attachments and the same splendor as in the tombs of Alaca Hüyük (more on this later).

Further east, in the area of the early Transcaucasian civilization, in the first period of this culture (ca. 3250-2700), we find only copper objects, but in the 11th period (ca. 2700-2300), before the middle of the 3rd millennium, bronze appears here, initially in the form of arsenic bronze, with an arsenic content of up to 5-6% (1. Narr 1975, p. 518). In the upper strata of the 11th period of Kültepe near Nakhichevan, casting samples, smelting ax and arsenic bronze objects were found (1. Burney-Lang 1975, p. 143; Narr 1975, p. 518). The 2nd period of Kültepe can be dated to between 3200 and 2500 BC based on a C° measurement: a sample taken from the middle of 14 layers of the period, 8 m thick, yielded a date of 2920±90 BC (cf. Burney-Lang 1975, p. 115.).

Similarly, numerous arsenic bronze tools have been recovered from the B layer of Kvazchlebi in Georgia (see Narr 1975, p. 518), which also date back to before the middle of the 3rd millennium: the C, , layer section C° immediately preceding the B layer is dated to 2800±90 BC (1. Burney-Lang 1975, p. 94). Closer parallels with Anatolia in these areas only appear around 2400-2300, with the beginning of the III period of the early Transcaucasian civilization, i.e. exactly at the same time when strong traces of the Anatolian Early Bronze Age culture appeared at Horoztepe and the other mentioned northeastern Anatolian settlements, and even in Maykop. We will deal with this period in more detail later.

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Let us now look at the development of the cultures of the western regions, the Aegean region, and Europe in the first half and middle of the 3rd millennium, during the early Sumerian dynasties. In the northwestern regions of Anatolia during this period - as we already know > we find the Troy-Jortan culture, which reached its peak of flowering in the Troy II period, between about 2700/2600 and 2400/2300. The finds of this cultural circle are in many ways similar to, or closely parallel to, the contemporary Central and Southern Anatolian cultures. We find walled settlements everywhere, burial customs are the same in most places (so-called "pithos" tombs; i.e. the dead were placed in larger clay vessels), the megaron type of construction is widespread, and in the field of ceramics several vessel shapes are identical almost throughout Anatolia (e.g. two-handled cups, slanted-mouthed jugs, etc.). Similarly, the rapid development of metalwork can be observed in the west as well as in the east. We have already mentioned that the pieces of treasure finds from the Troy II period show several organic parallels with the metal finds from 8 Hüyük. However, these correspondences also extend to the products of Mesopotamian and Syrian metalwork of the same period, especially the metal objects from the royal tombs of Ur and the Akkadian-era Tell Brak in northern Syria. metal tools. At the same time, tin bronze also appears in the Troy II period. We have also pointed out above that this rapid cultural development was ultimately the result of Mesopotamian-Sumerian cultural radiation, systematic trade relations, and even colonization, and can be traced primarily to the Sumerian civilization's continuous, ever-increasing need for metal (1. pp. 747-748 and 772-773).

Well, these Mesopotamian-Sumerian influences, as a result of which urban civilizations developed in Central and Western Anatolia around the turn of the 4th/3rd millennium BC, soon became increasingly evident in the Aegean region. Even in the first half of the 3rd millennium BC, between about 2800-2600 BC, the so-called Early Cycladic culture (Prühkykladikum) began in the Cyclades, the so-called Early Hellenic civilization (Frühheliadikum) on the Greek mainland, and around 2600 BC we can also observe the beginnings of the so-called Minoan culture in Crete (Early Minoan period; „Frühminoikum“).

G. Childe, who possessed an almost incredible degree of insight and synthetic sense, recognized the close Mesopotamian-Sumerian connections in the case of these cultures decades ago, at a still rather rudimentary stage of archaeological research, and pointed out with his hand that all three cultural circles clearly emerged mainly under the influence of the contemporary Mesopotamian and Anatolian cultures, and — to a lesser extent — the culture of the Old Kingdom of Egypt, primarily as a raw material market for the Near Eastern cultures. It is characteristic of these Aegean cultural circles that they have no temples, no sign of social na-776

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to a greater extent, they are socially stratified, 8 that they are uniformly strongly mercantile in character, i.e. typical colonial structures (see Childe 1949). This is particularly noticeable in Crete, where almost the entire culture, including the social and economic system, was built entirely on the Sumerian model. The same Mesopotamian influences spread through Crete to Sicily, southern Italy, and even as far as the Iberian Peninsula (see Childe 1952). We will see below that more recent archaeological excavations have since repeatedly confirmed Childe's findings.

In addition to Childe, one of the most renowned experts on Aegean cultures, F. Matz, also emphasized in the 1950s that both early Hellenic and early Minoan civilizations contain a large number of parallels of eastern, ultimately Mesopotamian-Sumerian origin, which reached the Aegean region via Asia Minor (cf. Matz 1958).

The same massive Mesopotamian and Asia Minor influences were also demonstrated by the other leading Aegean authority, F. Schachermeyr, through thorough, down-to-the-minute analyses and comparisons of finds, also in the 1950s (cf. Schachermeyr 1955). After this brief historical review of research and general findings, let us look at the most characteristic Eastern parallels of the aforementioned Aegean civilizations one by one. The advantageous geographical position of the region was undoubtedly of particular importance in the development of the early Cycladic culture, which predestined the Aegean islands to be ideal bases and transhipment points for the trade of the eastern Mediterranean basin. Some important raw materials also played a major role in the cultural development, such as the copper deposits on the islands of Paros and Siphnos, or the marble mines on Naxos and Paros (cf. Narr 1975, p. 522). The Cyclades and other Aegean islands already played a significant intermediary role in the East-West maritime trade at a time when Crete and the Greek mainland were still in the Neolithic period. This is evidenced, for example, by the already mentioned Jemdet Nasr seal cylinder from Amorgos, as well as a vessel made of Parian marble in an Early Dynastic tomb in Egypt (1. Narr 1975, pp. 522-523), and the early, late 4th millennium settlements near the Aegean entrance to the Dardanelles, also already described, which were evidently intermediate bases on the sea routes to Europe (1. above, pp. 737-738). Later, parallel to the flourishing of the Early Hellenistic and Early Minoan cultures, the Cyclades gradually lost their initially prominent role, and the centers of trade increasingly shifted to Crete and - to a much lesser extent - to the Greek mainland (cf. e.g. Narr 1975, p. 527).

The pottery of the earliest stage of the Early Cycladic culture (Early Cycladic I) is a monochromatic, scratch-decorated ware that is closely related to

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on the one hand with the lower layer of Kumtepe and the Troy I kerāmājā, and on the other hand with the pottery artefacts of the Early Hellenistic Culture I stage on the Greek mainland (1. Müller-Karpe 1974, pp. 151-152); on the island of Syros, a significant settlement belonging to the middle of the 3rd millennium BC from the Early Cycladic Culture II stage was excavated in Chalandriani. The walls of the houses were built using the same technique as in Troy I and II. The settlement was protected by a fortification with projecting battlements (or bastions) (a similar fortress is also known from the island of Siphnos). In Chalandriani, several typical Anatolian two-handled cups were found, as well as metal casting samples, copper slag, an Anatolian-type silver vessel and genuine tin-bronze objects with a tin content of 4.3-6.2 9 (cf. e.g. Müller-Karpe 1974, p. 881). Metal tools, copper pins and tweezers were also found in the cemetery of the site (1. Narr 1975, pp. 523-525). In this period, the

Arsenic bronze is already very common in the Aegean region, and in addition to the aforementioned Syros pieces, we also know of several objects made of genuine tin bronze from other places, e.g. daggers and a bracelet with a tin content of more than 13% from Amorgos (cf. Müller-Karpe 1974, p. 153). Eastern connections and cultural influences can also be observed at other sites. For example, spearheads or spearheads of Cypriot, Syrian and Asia Minor origin in the tombs of the island of Amorgos (1. Narr 1975, p. 525). Similarly, copper slag and casting samples on the island of Paros provide evidence of copper mining and metal casting in the early Cycladic period (1. Müller-Karpe 1974, p. 128).

The oriental forms — evidently largely through Cycladic transmission — soon appeared on the Greek mainland. The pottery of the oldest period of the Early Hellenistic culture (Early Helladic I) — like the Anatolian ware of the first half of the 3rd millennium — is also characterized by single-colored, blackish or bright red glazed clay vessels polished to a mirror-like finish. The pottery of the lowest layers of Eutresis in Boeotia (Early Helladic I) is very similar to the oldest ware of Lesbos (Thermi) and Lemnos (Poliochni), as well as to the clay vessels of Troy I. According to two C¹⁴ measurements, the settlement in Eutresis, belonging to the 1st period of the Early Helladic culture, dates from 2670±59 68 2673±72 BC, and the subsequent and for layer 8156 of period 11, 2431±58 (1. Narr 1975, p. 530). The gold and silver vessels found on the island of Euboea belong to this latter period, which probably came there from the Cyclades or Asia Minor. This is also confirmed by the Cycladic and Anatolian-type pottery from the fortified settlement excavated near Cape Manika, also on Euboea (see Narr 1975, p. 526). Also belonging to the II period are « among others = the Attic Agios Kosmas, a considerable fortified city on the island of Aigina, and the settlement of the 1st layer of Lerna, south of Argos, surrounded by a double wall and fortified with projecting towers. The aforementioned sites are all distinctly Cycladic-Anatolian in character (16, e.g. Narr 1975, p. 530-531), 778

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Cretan & Early Minoan Period 1st phase (c. 2600-2400 BC)

3010) the pottery ka di. Narr 1975, p. 527). The high culture there appears with single-colored, black or red glazed ceramics (cf. Propyl.: Kunstgesch. XIII yyyy, pp. 63-64), and only at the end of the 3rd millennium BC, in the early Minoan period (between ca. 2200-2000), does the unique, characteristic Cretan ani appear.

In the second phase of the Early Minoan period, between 2400 and 2200 BC, development accelerates noticeably. The pottery remains largely of Anatolian type, with Mesara in southern Crete. On the plains, larger tholos-like pseudo-domed tombs appear, copper becomes common, and the two-edged axes, cultic pairs of horns, which had previously been found in Anatolia (e.g. in Troy I), as well as the cone-shaped clay pots (kernor²) and beaked jugs, also of Anatolian origin, appear. In Mochlos (East Crete), rich graves yielded extremely finely crafted gold jewelry, which — especially the gold flower petals and leaves — can be unmistakably traced back to very similar pieces from the royal tombs of Ur (see e.g. Müller-Karpe 1974, p. 150; Schachermeyr 1979, pp. 52-53). Moreover, the purpose of these flower petals and leaves is the same: in both Urban and Mochlos they belonged to women's headdresses.

In this period, there were probably also considerable immigrations to Crete from Anatolia (1. e.g. Schachermeyr 1979, p. 54). This is also supported by the fact that — in addition to the correspondences mentioned so far — the column or facullate and the cultic pairs of horns, which were repeatedly found in the early Bronze Age sanctuaries of Beycesultan in western Anatolia in the middle of the 3rd millennium, appeared in Crete in the same form and role shortly afterwards. These cultic correspondences are undoubted evidence of the dominant influences of Anatolia on the development of Cretan high culture (see e.g. Alkym 1968, p. 119-120).

According to the almost universally accepted view of modern research, the economic interests of the far-leading pre-Asian civilizations played a significant role in the emergence of the Aegean cultures of the 3rd millennium BC, above all the exploration and exploitation of ore deposits, and that in close connection with these movements, colonial groups of pre-Asian origin also appeared in many places in the Aegean region. Although these colonists did not create large empires, they occasionally established colonial-type political and economic ruling organizations in smaller or larger areas with the involvement of the local population (v.

pl. Müller-Karpe 1974, 482. old.). However, such pre-Asian colonizations can also be traced in regions much further away than the Aegean. The megalithic groups of southern Italy, Sardinia, Iberia, etc. of the 3rd millennium, where knowledge of metalworking can be found almost everywhere, were in all likelihood pre-719

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were Asian colonists who, first settling in small coastal settlements as focal points, gradually expanded the areas under their control, subjugating or winning over and organizing the local Neolithic population. These colonists eventually reached the British Isles and even Scandinavia along the Atlantic coast (see map 7). The aim of the colonization was apparently to search for and seize ore deposits (all this j. Müller-Karpe 1974, pp. 482-483.)

According to Narr's cited handbook (1975), the finds of many settlements in the Iberian Neolithic of the 3rd millennium clearly indicate that they were occupied by pre-Asian colonists who lived in fortified towns and did not mix with the local population. It can be stated with complete certainty that these colonial settlements were centres of copper mining and trade, and their inhabitants constituted a sort of ruling class among the native population. They were buried in domed tombs very similar to the tholos tombs of Crete. Their towns are found along the southern coast of Spain and in Portugal, and are mostly located near the sea, but not directly on the coast. They overlook the sea, but are not visible from the sea (1. Narr 1975, pp. 545-546). - We note that the exact same situation is true of most Cretan cities and early Hellenistic Greek settlements - the predecessors of the later Mycenaean centers. The design and construction of fortifications - continues Narr's manual - reveals a military tactical and technical knowledge that we can in no way expect from the surrounding Neolithic indigenous population. It is striking that, for —, but also in the case of Troy. example, the forts of Chalandriani belonging to the early Cycladic period or the contemporaneous Early Hellenistic Lerna were built in the same way, as were Troy 1-11. These similarities alone prove that the colonists arrived in the Iberian Peninsula from the east. But the irrigation system recently discovered at the Orce settlement (Granada province) also testifies to the same thing (l. Narr 1975, p. 547).

Copper is found in all such colonial settlements, not only as finished tools, but also in the form of drops and slag. Together with the remains of melting pots, which can also be detected, this undoubtedly proves that the entire process of metalworking took place in the settlements, from ore to finished vessels. The daggers and axes correspond completely to the early Minoan Cretan forms. On the other hand, no copper ornaments are found. This is striking because numerous copper ornaments occur in Crete and the Aegean region. Crete, the Cyclades and the Greek mainland were therefore only transit stations, not the homeland of the Iberian and other megalithic colonists. Their original homeland must therefore be sought in East Asia and Egypt (cf. Narr 1975, pp. 550-551).

The mentioned megalithic colonial settlements can be dated to between ca. 2600-1900 (cf. Narr 1975, p. 553), i.e. mostly from the Akkadian and Ur III periods. 780

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They belong to the 1st century BC, but since their beginnings date back to the time of the early Sumerian dynasties, we did not want to break the connections and presented them together. However, we must point out that these colonists migrated from Near East and Mesopotamia mainly in the Akkadian period, and in some cases even in the period after the fall of the 3rd Ur dynasty, obviously not least as a result of the chaotic conditions of the time, the massive Amorite influx and the general uncertainty of existence (more on this later).

The cultural influences and migrations from the east that led to the formation of the early Aegean cultures and megalithic groups were not confined to the coastal regions of Europe but extended to the interior of the continent. We can also trace it in the areas of Southeastern Europe and the Carpathian Basin. The Troy-Jortan cultural circle soon, at the very beginning of the 3rd millennium, expanded to the neighboring Balkan areas, primarily Thrace and the eastern parts of Macedonia (cf. Narr 1975, p. 508), and we must also consider the appearance of Polish culture in Transdanubia and the directly adjacent areas around the turn of the 4th/3rd millennium as an integral part of these migrations (cf. e.g. Propyl. Kunstgesch. vol. XIII. p. 99). Nándor Kalicz also explains the origin of Polish culture by the immigration of southern ethnic elements (cf. Kalicz 1974, p. 48). However, the influx of southerners continued into the middle of the 3rd millennium. The Tisza culture of the Great Plain developed without a break in the first half of the millennium, and then the Bodrogkeresztúr culture shortly thereafter in the middle of the millennium. The proto-European and proto-Mediterranean mixture of the Tiszapolgár population noticeably shifts towards the Mediterranean type in the Bodrogkeresztúr period. At the same time, Aegean vessel shapes also appear. These two data undoubtedly indicate the immigration of significant masses of southern origin (v6. e.g. Narr 1975, pp. 183-185).

Some chronological notes

In connection with the aforementioned cultures, we must point out that — as our informed readers have probably already noticed — in contrast to Hungarian paleoarchaeology, international research dates the Carpathian Basin civilizations of the 3rd millennium much earlier. For example, while Kalicz places the beginnings of the Tisza and Polish cultures at 2800 (l. Kalicz 1974, p. 68), the cited manual by Müller-Karpe places the Tisza civilization around 3100 and the Polish culture around 3000 (v6. Müller-Karpe 1968, Tafelband, chronological table no. 4). Similarly, the Tisza civilized civilization according to Kalicz dates ca. 2450, the Bodrogkeresztúr culture around 2250, and the Baden-Pécel culture around 2100 (l. Kalicz 1974, p. 68), but according to Müller-Karpe, Tiszapolgár and Bodrogkeresztúr were inhabited around 2700-2400 BC-781

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zé, belongs to the 27th-25th centuries AD, and Baden-Pécel to the 24th-22nd, \$căzad-, perhaps with early 25th century (cf. Müller-Karpe 1974, pp. 209 and 265). These chronological data are particularly incomprehensible when we consider that Müller-Karpe followed the middle chronology that was most commonly used at the time of writing his handbook, emphasizing, however, that this scheme cannot be considered proven at all (1. Müller-Karpe 1974, p. 39). Now, if we reduce Müller-Karpe's above data to the lowest datings found in international archaeology, the short chronology, i.e. to about 50-100 years later, we must still note with surprise that Kalicz dates it to about 150-250 years later than the short chronology.

Looking at the domestic and foreign datings of the earlier Neolithic cultures of the Carpathian Basin, we can establish differences similar to those outlined above, with the only difference being that Hungarian archaeology dates these cultures even younger than those of the 3rd millennium. Thus, Kalicz places the beginnings of the Körös culture at around 4000, that of the Bükk culture at around 3000 (l. Kalicz 1974, p. 68), although international archaeology has for some time almost unequivocally dated them to ca. He dates the emergence of the Körös culture to 5000 BC, and that of the Bükk culture to around 3800 BC (cf. e.g. Narr 1975, p. 208; Müller-Karpe 1968, Tafelband, chronological table no. 4; Propyl. Kunstgesch. vol. XIII, p. 365; Whitehouse 1976, p. 137).

All these chronological discrepancies arise from the fact that our paleoarchaeology is still clearly under the influence of the Eurocentric view of the former Indo-Germanic studies, and among foreign experts it obviously considers only the representatives of these earlier ideas as authoritative. The example just mentioned, the drastically reduced chronology of the Körös culture, is in fact — demonstrably — ultimately also based on this traditional view, the idea of Indo-Germanic or European cultural priority.

We already know that the earliest Neolithic culture of the northern regions of Central Europe, the so-called Band-decorated Ceramics culture ("Bandkeramik" or "Linearbandkeramik"), dates back to around 4000 BC, 5 and the vast majority of experts agree that this culture arose through the strong emanations and even colonizations of the StarCevo-Körös cultural circle. We have also seen that the isolated survival of the fishing-hunting indigenous population alongside and among the Band-decorated agricultural population can be detected in several places for a long time (all of this is discussed in], above, p. 721.

there is also literature), This is not the case with the representatives of the "ahistorical" trend of Indo-Germanic studies (the explanation of this term is given in our book I, p. 77). They ignore all archaeological evidence pointing south, and according to them the 782 "Bandkeramik" (or Danube culture) arose completely independently, all Pre-Asian or Baltic

===== PAGE 772 ===== without any influence from the Kanian period, i.e. an "autochthonous European" culture (cf. e.g. Pittioni: Der urgeschichtliche Horizont der historischen Zeit, Propyl. Weltgesch. I, p. 244). Regarding this, they European Neolithic culture developed in think that Europe developed independently of Pre-Asia (see, Pittioni uott, p. 233). Moreover, they directly claim that although there is no evidence, the very first count for the advanced the areas of the "Danube culture", already in the second half of the 6th millennium (!), but at that time there was still "Bandkeramik" pottery, or without it: "So then one can already sixth century with a soil construction in this area ..." (I. Pittioni uott, p. 244). In Hungarian: , Thus, in these areas we can count on farming already in the advanced sixth millennium ..." Consequently, it is self-evident — as in this "ahistorical" — that the Körös culture cannot be older than the culture of ribbon-decorated vessels, but at most contemporaneous with it, but much younger. For this reason, according to 10- gika, the beginnings of the Körös culture must be placed around 4000, so that the thesis of the autochthonous European Neolithic does not suffer.

The above brief overview of the finds clearly shows that in the first half of the 3rd millennium, in the era of the early Sumerian dynasties, from which time we already have historical — contemporaneous written — source data on several successive large-scale Sumerian empires (the hegemony of the Uruk, Kis, Akšak, Mari, etc. dynasties in the whole of Mesopotamia), some of which extended "from the lower sea to the upper sea", i.e. from the Persian Gulf to the Mediterranean Sea (e.g. the empire of Lugalzagezi between ca. 2480-2455), simultaneously with these significant, culturally far-reaching state formations, the appearance of countless smaller and larger Pre-Asian — largely undoubtedly Mesopotamian-Sumerian — colonial groups can be dated on the one hand in Anatolia and almost the entire Mediterranean region, and even on the shores of the Atlantic Ocean, and on the other hand in western Iran, southern Turkmenistan, Afghanistan and probably also in the Indus Valley Civilization area.

We have also seen that these Proto-Asian colonists, almost without exception, occupied the deposits of ores and other important raw materials, or appeared at the junctions and strategically important sections of the water and land trade routes leading to them. These phenomena can undoubtedly only be directly linked to the ever-increasing need for raw materials - primarily metal - of the Sumerian civilization, and consequently to the final development and consolidation of the centrally organized Mesopotamian-Sumerian long-distance trade network. It follows clearly from this that among these East Asian colonists - in addition to the Anatolian, Syrian, Levantine, northern Mesopotamian (Subir-Hurrian) and western Iranian (Elamite) ethnic groups - there was obviously a very considerable number of Sumerian ethnic groups. 783

===== PAGE 773 ===== We must also take this into account, since the engine of the entire organization, the main demander and consumer market for raw materials, was only Sumerian high culture at that time.

Thus, these pre-Asian, partly Mesopotamian-Sumerian colonial foci, which can be assumed with a probability bordering on certainty and can be convincingly supported archaeologically, from Western Europe to Turkmenistan and the Indus, as well as the dominant cultural and linguistic influences of these settlers on the much more backward indigenous population of the surrounding areas, clearly and reasonably explain the close Sumerian ethnographic - fairy tales, legends, mythological - and linguistic correspondences that appear en masse in the beliefs, customs and - not least - languages of the Semitic, Indo-Germanic, Uralic and Altaic peoples, some excerpts of which we have already presented in Chapter 5 of our Book I.

We can understand these phenomena even better if we remember that during the late Eridu-Ubaid cultural koine, i.e. between ca. 4500-3700, large-scale migrations and colonizations originating from the area of this cultural circle can be dated in the nearer and more distant West Eurasian region surrounding East Asia (1. pp. 724-728), and that the spoken language of the late Ubaid cultural koine, according to almost all available archaeological data, was very likely Sumerian — more precisely Elesumerian — (cf. pp. 670-706).

Traces of Sumerian cultural emanations and migrations in the Akkadian, Gutian, Ur III and Isin-Larsa periods During this period, decisive political and ethnic changes took place in Mesopotamia. From the direction of the Arabian desert and the Syrian steppe, the Semitic-speaking Akkadians apparently began to infiltrate Mesopotamia in increasing numbers from the beginning of the 3rd millennium. At first, they quickly merged into the mass of the indigenous population, but by the middle of the 3rd millennium, through constant immigration, they had become so strong that they seized political power around 2450. Sarrukin (Sargon) created the Akkadian empire, which included all of Mesopotamia, Syria, and even Elam for a short time, and, like the earlier empire of Lugalzagezi, it extended "from sea to the upper sea". the bottom

From this time on, the Akkadian, and soon the Amorite-Semitic element, increasingly established itself in Mesopotamia. Sargon and his successors abolished the autonomy of the Sumerian city-states, which had previously existed under the rule of the hegemonic Sumerian dynasties, placed Akkadians in leading positions in the political and religious hierarchy, and granted the Akkadian Hargos extensive territories in exchange for military obligations. 784

===== PAGE 774 ===== These provisions naturally provoked great discontent and resistance among broad sections of the Sumerian indigenous population. Undoubted proof of this is that the anti-Akkadian freedom struggles began already in Sargon's last years (cf. e.g. Soden: Propyl. Weltgesch. vol. 1, p. 549), and immediately after his death, huge bloody uprisings broke out throughout the empire, the proportions of which - and at the same time the extent of the Akkadian retaliation - we can form a clear picture of based on some surviving written data. In the south, for example, sources report 8,040 dead in Urban and Umma alone, and in the north, in Kazallu alone - where a Subir-Hurrian population already lived at that time - we know of ~12,650 dead (cf. e.g. Schmökel 1962).

Such general uprisings were regularly repeated at the death of each Akkadian king, and each new king had to virtually reconquer his predecessor's empire (cf. e.g. Moortgat 1950; Bottéro 1965; Soden: Propyl. Weltgesch. Vol. I, p. 549). Similarly, the Gutian attack that brought about the end of the Akkadian empire occurred in a time-coordinated manner with the mass uprising of the Sumerian city-states (cf. Lloyd 1981, p. 177).

All this is a clear sign of the extreme instability of the Akkadian empire. However, the mass uprisings also prove that in most of the conquered Mesopotamian territories the Akkadians were not yet in the majority at that time, and the overwhelmingly non-Semitic population regarded Akkadian rule as a foreign conquest. This is also confirmed, for example, by the fact that from the architecture of the Akkadian period we know almost only fortifications, and not from the border regions of the empire, but mostly from the interior areas (e.g. Tell Brak in Syria), which were obviously built in many places in Mesopotamia to keep the local population in check. However, we also have surprising archaeological evidence of organized anti-Akkadian behavior by the indigenous population. The metallurgical analysis of the metal objects from the Uri cemetery excavated by Woolley revealed that while many of the metal objects found in Sumerian graves of the first half of the 3rd millennium BC were made of genuine tin bronze, the metal objects found in the Akkadian graves of the cemetery were cast from pure copper without exception (cf. e.g. Müller-Karpe 1974, p. 426). This striking phenomenon suggests that either the local Sumerian metalworkers employed passive resistance against the Akkadian conquerors and did not reveal to them the secret of bronze making, or, on the other hand, we can also assume that, in agreement with the domestic resistance, the local leaders of the Sumerian ore-exploring and metal-processing colonial settlements, which had been exploiting distant ore deposits for centuries, consciously stopped metal shipments to Akkadian Mesopotamia (the Akkadians could have obtained copper from the Armenian and Western Iranian deposits directly bordering their empire, even by force, but tin — as we have already mentioned — was imported from Mesopotamia).

785 ===== PAGE 775 ===== does not occur near: the nearest 6nbányák are found in Eszakelet-Irán; cf. above, p. 766). This had a first-rate military-tactical significance, and obviously contributed to a large extent to the fact that the Sumerian city-states were able to shake off the Akkadian yoke after a relatively short time: weapons made of bronze were much more effective in battle than the copper weapons of the Akkadians, which soon became useless from the blows of the harder bronze battle axes and swords. However, the Sumerians also had the opportunity to withdraw from Akkadian rule. The migration routes through Elam and along the Zagros Mountains to the northeast and north were mostly free during the Akkadian hegemony. Elam only remained part of the Akkadian empire under Sargon, and the Gutian territories further north were never conquered by the Akkadians. Then, starting with the last years of Sargon, a series of regularly recurring uprisings opened the escape routes in all directions, sometimes for shorter or longer periods. After Sargon's death, the Akkadians lost most of the conquered territories outside Mesopotamia, and only Naramsin was able to partially restore Sargon's empire about 30 years later, but several provinces - e.g. He also failed to recapture Elam: we know that he was forced to conclude a treaty with the Elamite king as an equal party (the text of this agreement has survived). It can therefore be assumed with great probability that the constantly recurring

The Sumerians and other subjugated peoples of the empire fled en masse from the Akkadians, who brutally retaliated against the uprisings (cf. the number of dead above) — for example, the Hurrians from the area of Kazallu and other northern Mesopotamian areas. The core of the migrating groups was undoubtedly formed by the more culturally and ethnically self-conscious strata, who actively participated in the uprisings and rightly feared the revenge of the Akkadians: it was these people who endured the most hardships when the Akkadians, who were more uncultured, followed different customs and laws and spoke a foreign language, only relied on weapons.

These were apparently joined by simple farmers whom the Akkadian kings had driven from their fields, gardens and date palm groves in order to provide a livelihood for their own military (1. above).

The surviving contemporary and contemporary sources clearly show that these deployment orders affected a very considerable number of people. We can form an idea of the size of the mobilized Akkadian army from the already mentioned story in the Naramsin-monda that this king put 180,000, 120,000 and 60,000 men into arms within a few short years. Although these round numbers are obviously exaggerated, they cannot be called fantastic at all, especially not when we compare them with the other surviving text-786

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I get, in which Sargon himself says that "5400 warriors ate at my table every day" (cf. e.g. Soden: Propyl. Weltgesch. 1, vol., p. 548). That is, in the capital alone there were 5400 soldiers constantly in arms. We must therefore compare the size of the mobilizable Akkadian army to this number, which, in addition to these 5400 men, consisted of the garrisons of the other cities and fortresses, as well as the soldiers settled in the countryside and required to serve armed only in times of war.
| The number of the settler army is thus calculated with extreme caution- |

va -- we must put it at a minimum of 50-60,000, which meant the expropriation of the lands of at least the same number, but much more likely two or three times as many Sumerian families, since the Akkadian kings obviously could not be stingy in satisfying the needs of the Akkadian military, the sole base of their power, and undoubtedly allocated them much larger plots than the size of the land usually owned or leased by a Sumerian agricultural family. This means that already at the time of Sargon's first settlements at least 150,000-180,000 Sumerian families — that is, about 600-700,000 people — had become partly vagrants and partly slaves on their former estates.
However, this calculation only applies to the time of Sargon. We also know from contemporary written lists that his two sons, Rimus and Manistius, "bought" the lands. This has caught the eye of many researchers. It the acquisition of land (see Propyl. Weltgesch. 1, p. 549) -- It is not at all impossible that the epigraphic texts, with the motif of beating to death with seal cylinders, indicate that Rimus had his own seal-787

However, it remains highly questionable in these estate "purchases" how much was paid for the lands, whether the usual fair price was given, and finally how much was taken simply by force of words, under one pretext or another, without payment. Most estates were undoubtedly acquired in the latter way. This follows almost automatically from the essence of the matter.

In these systematically pursued land purchases — no matter how they were made — a purposeful effort to reduce the material base of the indigenous population as much as possible is clearly visible — even if the previous owner received some small compensation for the land he had taken. For there can be no doubt for a moment — if one looks at the question with common sense — that these mass royal "land purchases" did not take place on the basis of equal opportunities for the buyer and the seller, but that the indigenous owner was more or less forced to sell.

Soden also hints at this when he remarks: "Internal peace was not promoted by these acquisitions of property. Later epigraphic texts state that both rulers were murdered by their servants; Rimus was beaten to death with a bundle of seal cylinders." (I. Propyl. Weltgesch. Vol. I, p. 549) -- It is not at all impossible that the epigraphic texts, with the motif of beating to death with seal cylinders, indicate that Rimus had his own seal-787

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"His sales contracts, authenticated by his prints, did not correspond to reality or to the requirements of justice and fairness, i.e., according to the analogical beliefs generally prevalent at that time, he was punished for what he did wrong."
sons of Sargon, "his purchases of estates" therefore obviously again reduced hundreds of thousands of the Sumerian indigenous population to beggars or forced them to migrate. After the death of Mani&tsu and the when the fire of uprisings flared up again throughout the empire, 5 Naramsin's first years were filled with the suppression of these, we must also expect a significant new wave of migration.
When the Akkadian empire was overthrown — as we have already mentioned — the Sumerian city-states, by all indications, allied themselves with the Gutians to attack the Akkadians. This is also reflected in the consequences: contemporary sources prove that during the period of Gutian hegemony, the pressure of the Gutians was much less felt in the southern Sumerian territories than in the northern, mostly Akkadian regions, and the Sumerian cities remained practically independent (cf. Moortgat 1950; Schmökel 1962). For example, the city-state of Lagas enjoyed complete independence during the reign of Urningirsu and Gudea about 40 years before the Gutians were driven out, although it was located the most north-eastern of all the Sumerian city-states, i.e. closest to the Gutian homelands. We know from contemporary records that Lagas was a flourishing state during this period, with a population of about With an area of 1,600 km² and a population of 216,000, it included 17 larger and 8 smaller towns, as well as numerous villages, 40 of which are known by name (cf. Müller-Karpe 1974, p. 45).

From these contemporaneous data we can conclude that the total population of the Sumerian territories at that time (around 2290-2250 BC) was probably several million. 11678 For the rest, Lagas was considered such an insignificant városállam that its rulers were not considered worthy of mention in the Sumerian king lists; neither in the time of Gudea — who led an independent army into Elam — nor before the Akkadian period, when some Lagasian kings demonstrably ruled significant areas of Mesopotamia, e.g. Eannatum and Entemena.

The Ur III dynasty, which followed the Gutian rule, restored the political unity of Mesopotamia, again under Sumerian leadership, and this so-called Neo-Sumerian empire rivaled Sargon's former empire in size. We know that the Ur kings also conquered Assyria, Elam, and the western Gutian tribal territories (cf. e.g. Hrouda 1971, p. 142), and it can even be stated that Ebla (v. Ibla) in western Syria, which has been excavated in recent decades, and Byblos on the Levantine coast, also belonged to the III dynasty. It was one of the fiefs of the Ur dynasty (cf. e.g. Amiet 1977, pp. 139 and 486; Kempinsky - Avi Yonah 1978, 42. 010.), and Kültepe in central Anatolia was a flourishing trading colony during the Ur III dynasty, which was later, in the 788

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'After the fall of Gastia, the Dassirs took over (see e.g. Gochtze 1957, p. 66, Hrouda 1971, p. 171; Schmökel 1974, p. 88) = We have supported the Sumerian antecedents of the trading colony of B Kültepe in the 1111th century with multiple references because in most of the commonly available summary works, the colony is only listed as a colony.

In the last decades of the IIIrd dynasty of Ur, we hear more and more about the increasing pressure of the Amor-ru nomads (Amorites) from the region of Syria towards Mesopotamia. These Semitic-speaking Amorites had already established a foothold in the middle reaches of the Euphrates, around the city of Mari, at the beginning of the 24-year reign of the last Ur king, Ibbisin, which resulted in the interruption of the trade routes to the northwest, and that — undoubtedly due to the simultaneous damage to the vital canal network or the obstruction of its continuous maintenance — a lasting famine broke out in the southern Sumerian tribal areas in the center of the empire. This can be clearly stated from Ibbisin's surviving correspondence and other contemporary sources. For example, we know that in the 7th and 8th years of Ibbisin, food prices rose 50-60 times the normal level.
Rebellions broke out everywhere, most provincial governors no longer recognized the authority of the Ur III dynasty, and Ibbisin's area of power practically shrank to the territory of the former city-state of Ur. Soon after, the Elamites also began their attacks, and in the 24th year of Ibbisin, around 2141, they finally overthrew the Ur III dynasty; they destroyed Ur and deported Ibbisin along with a significant part of the population.

After the fall of Ur, Isbierra, probably of Amorite origin, the founder of the Isini dynasty — who was initially the provincial governor of Ibbisin, then changed sides and actively participated in the overthrow of the Uri III dynasty — tried to reassemble the ruins of the empire, but with only partial and temporary success, because the following
In the following decades, Mesopotamia again disintegrated into a mosaic of small city-states, in which the leadership was sooner or later taken over almost everywhere by the Amorites, who continued to flood in large numbers. As a result, general anarchy prevailed in Mesopotamia, ordinary raids between neighboring city-states were the order of the day, and security of law, property, and life ceased (cf. e.g. Edzard: Fischer Weltgesch. Vol. y. Chap. 4.).
Under these circumstances, it is obvious that already during the reign of Ibbisin we must definitely count on a mass migration of the starving Sumerian population of southern Mesopotamia. But later, in the turbulent times after the fall of the IIIrd Uri dynasty, there was no opportunity for a nationwide reorganization of production or for the repair of the canal network, so the famine and with it the migrations undoubtedly continued.

789

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This is also confirmed by the surviving texts of nearly the same period, such as the Lamentation of Ibbisin and the Lamentation of Ur, which were written shortly after the destruction of Ur, the first of the Isin dynasty. They date from the 18th and 19th centuries. In these poems, which are valuable historical sources, we can read that a considerable proportion of the population of Ur and southern Mesopotamia in general was dispersed in all directions of the compass, that many cities lay in ruins, and that the Sumerian gods also left their cities along with the population. In Book I of this series of studies, we have already pointed out that this formulation, in the language of religious imagery at the time and embedded in the ancient Near Eastern world of thought, means that the migrating population took its local gods with them (cf. pp. 44-45 of our book).

Let us therefore examine the surrounding areas, first during the Akkadian period, and then in the period after the fall of the Third Ur Dynasty, in order to see whether we can find any traces or phenomena that can be convincingly linked to the appearance of Mesopotamian-Sumerian groups. We must focus our attention primarily on the parts of Iran north of Elam, since we have already seen that in the Akkadian period the easiest routes of migration led to Iran, and these were open to refugees for most of the period in question.

In Luristan, beyond the northern borders of Elam, very similar precursors of the so-called Luristan bronzes can be found already during the early Sumerian dynasties. This has only been brought to light by regular excavations over the last 15 years. In many places, large cemeteries dating from the first half of the 3rd millennium have been found, with large family-ethnic crypts of considerable size, walled with stone and covered with stone slabs in a flat or gable-like roof, from which rich appendices have been recovered. The ceramic finds correspond to the Elamite wares parallel to the later third of the early Sumerian dynasties, but numerous Mesopotamian seal cylinders have also been found. However, the metal appendices are most interesting, as they show that Luristan already had a highly technically developed metal industry in the first half of the 3rd millennium, and artistically crafted copper and bronze objects were cast. The bronze tools were almost without exception made of arsenic bronze. The weapons and tools are identical in form and technology to the products of the Sumerian and Elamite metalwork of the same period. However, certain local characteristics, which would remain just as characteristic of the much later "Luristani bronzes", already appear at this time; the

For example, animal or human-shaped inlays appear on ornamental weapons. Particularly close Mesopotamian connections are evidenced by the Sumerian inscription on the handle of a locally made mace, which is evidence that Sumerians also lived among the Luristani (all this J. Amiet 1977, 113, and 558, pp. but also cf. Hrouda 1971, p. 127). After these antecedents, from the beginning of the Akkadian period onwards, the indigenous population: 64g (the large communal tombs were used until the end of the 3rd millennium BC -79%)

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"naltaks) in several places, e.g. in the Qaldye Nesar and Daryand regions, a new population appeared, with different burial customs; their dead were buried separately, in individual sf- toks. At the same time, the previous strong Mesopotamian connections ceased, and the connections with Elam became increasingly close. The excavator L. Vanden Berghe sees in these newcomers the Gutians, who, according to him, extended their power not only westward, but also to Luristan in the Akkadian period (cf. Amiet 1977, p. 558). It is at least equally probable, if not much more probable, that the new ethnic element that appeared in Luristan from the beginning of the Akkadian period consisted mostly of Sumerian agricultural settlers who had been made fugitives by the Akkadian kings. In any case, this is supported by the fact that, according to Vanden Berghe's own observation, from this time onward, relations were strengthened not with Akkadian Mesopotamia but with Elam, which was independent of the Akkadians. The data, which has only recently become known, that numerous flourishing agricultural settlements arose in Luristan in the following centuries (we do not know of any such settlements from earlier!) and that prosperity greatly increased (cf. Amiet 1977, p. 558) also points in this direction with a high degree of probability.

It seems, therefore, that in Luristan we have most likely found significant groups of Sumerians who migrated during the Akkadian period. This would also be of paramount importance for the later history of Mesopotamia: the Kassites invaded Babylonia from Luristan in the first half of the 2nd millennium, replacing the Hammurabi dynasty and ruling Mesopotamia for more than half a millennium. In view of the above, the idea that the Kassite ethnicity contained a significant Sumerian element cannot be dismissed. This possibility is also confirmed by the striking fact that most of the inscriptions of the Kassite kings are in Sumerian script, and in general they sought to renew the Sumerian traditions of the 3rd millennium in all areas (cf. e.g. Schmökel 1957).

In northern Iran, in the southeastern region of the Caspian Sea, as we already know, around 3000 BC, uniformly dark gray, single-colored glazed pottery appeared, the vessel shapes and decorative motifs of which imitate metal vessels. At the same time, metalwork also showed a great boom (cf. Hrouda 1971, p. 127). The route of the Mesopotamian colonists who appeared en masse in Turkmenistan around 3000 BC passed through these areas (see above, pp. 725 and 732-733). In the Akkadian period, this gray pottery is still found in the known northern Iranian settlements, including the most important site, Tepe Hissar, where the second half of the 3rd millennium is represented by the III. period with three layers (A, B, C), between ca. 2500-1900. The conditions of the IIVA layer were not sufficiently clarified during the excavation, but the II/B layer, which is contemporary with the end of the Akkadian period and the time of the II. Ur dynasty (ca. 2350-2150), is quite well known.

791
===== PAGE 781 =====

It is striking that at the beginning of the II/B layer the settlement suddenly expanded massively, became a värosiassä, and the population increased significantly. In addition to the impressive residential houses, a 27 x 13 m representative building was also excavated, in which the local ruler obviously resided. The corners of the palace pointed towards the cardinal points, as can be observed in all public buildings (temples, ziggurats, palaces) in Sumerian Mesopotamia since the Uruk period. The gate was surrounded by two towers, the walls were built in a bastion-like manner. It was reinforced by projections. Two flights of stairs led from the spacious vestibule to the upper floor, where the living rooms were evidently located, because the ground floor, apart from the vestibule, was occupied only by a huge hall (approx. 13 x 11 m) -- the roof of which was supported in the middle by two columns -- and two storerooms. Numerous ornamental weapons and metal objects, including a gold vessel, were found among the ruins of the palace (cf. Amiet 1977, p. 556). Several seal cylinders were also found in the Hissar III/B layer, as well as copper, bronze, silver and gold vessels, including an ornate copper bowl with a battle scene (a lion defeating a bull); and finally decorative staffs made of copper and bronze, crowned with human and animal figures at their upper ends, the figural decorations of which are very similar to some pieces of "military standards" found at Alaca Hüyük, which were also used pinned to poles (L. above, p. 773). It can be clearly established from the finds that Tepe Hissar at this time had many connections with Akkadian and Ur III Mesopotamia, as well as with Anatolian civilizations (Alaca Hüyük, Horoztepe, etc.) and the Kuban culture (cf. Müller-Karpe 1974, p. 367).

In light of all this, the idea that the remarkably rapid cultural development and sudden population growth in the Hissar III/B layer can be explained by the appearance of Mesopotamian-Sumerian groups that migrated during the Akkadian period deserves serious attention. This is strongly supported by the surprising It is also a phenomenon that the floor plan and layout of the palace described above are very similar to the Istar temple of the 6th archaic layer of the city of Assur, which dates back to 2500-2400. This, in turn, indicates that the population of the Sumerian colonial settlements in Assyria, which can be dated to the time of the early Sumerian dynasties, fled to the northeast from the Akkadian expansion, and in part probably settled at Tepe Hissar. Mesopotamian connections are even more evident in the rich tombs of the Hissar IIVC layer (ca. 2150-1900). At the same time, the annexes are again far-reaching. They also show parallels with the finds from the Anatolian and Cuban princely tombs already mentioned in layer II/B (cf. Müller-Karpe 1974, 355, p. 11). In other words, the continuity of the culture of layer II/B still existed in its entirety: the population of layer IIIC was exactly the same.

===== PAGE 782 =====

close ties with distant centers, with which connections were established as early as the 61626 era. It is particularly important for our current investigations that layer II/C coincides almost exactly with the fall of the IIIrd Uri dynasty and the Isin-Larsa period, It is also worth mentioning that a large amount of amber was found in the Hissar 111/02 layer, which indisputably proves that Hissar was already involved in the amber trade during the 111/02 layer. Now, since amber only appeared in the southern regions much later — in the Aegean region, for example, it begins to appear in the middle of the 2nd millennium — P. Amiet draws the conclusion from this that the Hissar 11/02 layer obviously extended to the middle of the 2nd millennium (I. Amiet 1977, p. 556). — However, in making this conclusion, Amiet not only ignored the fact that the latest phase of Hissar IV/C layer can be dated to around 1800 at the latest according to C" measurements (cf. RLA vol. IV, p. 429), but also forgot the very important fact that amber can be dated in the Near East as early as the 3rd millennium. On the one hand, amber beads were found in the archaic layers of the city of Assur (between ca. 2600-2200) (see RLA vol. II, p. 1), and on the other hand, we already know that amber jewelry was also found in the princely tombs of Alaca Hüyük (I, p. 774, but cf. also Müller-Karpe 1974, pp. 124 and 853).

It is therefore clear that these earliest Pre-Asian amber finds appear along the important route of Sumerian long-distance trade in the 3rd millennium, which followed the Tigris to Anatolia. Assur was a Sumerian colonial-hereditary center during the early Sumerian dynasties, and we have also pointed out that Alaca Hüyük was also, by all indications, one of the most important intermediate bases of the Sumerian trade network. In addition, the research clearly According to his findings, the settlement of Tepe Hissar during the yy. period had very close ties to both Mesopotamia and Alaca Hüyük; we also saw that the palace excavated in the HUB layer was modeled on the floor plan and layout of the Assyrian IStar temple. Under these circumstances, it is obvious that the large amount of amber found in the IIVC layer of Hissar does not at all suggest that this single layer should be extended for more than 600 years — such a thing is hardly conceivable from an archaeological point of view, but it clearly shows that the inhabitants of Hissar were already connected to the long-distance trade network that the Sumerians had been regularly maintaining since at least the end of the 4th millennium, and which also included Alaca Huyük and Assur. Thus, during the IIVC layer, we must again count on Sumerian immigration to Hissar, namely groups that left Mesopotamia around the time of the fall of the Ol. Uri dynasty.

793

===== PAGE 783 =====

Incidentally, the diverse Mesopotamian connections of Hissar, as well as similar find assemblages from several other sites in northern Iran and southern Turkmenistan, led the research to the realization that after the aforementioned great colonial wave around 3000 (L. pp. 725 and 732), the immigration of smaller and larger Mesopotamian groups to the regions of northern Iran and southern Turkmenistan continued throughout the 3rd millennium (cf. e.g. Jettmar et alii 1966, Part I, afterword). Thus, north of Hissar, not far from the southeastern corner of Lake Caspian, at Turang Tepe and Shah Tepe, the already known dark gray, monochromatic glazed pottery has also dominated since the beginning of the 3rd millennium, and in the second half of the millennium we find largely the same cultural phenomena as in the Tepe Hissar II period, to which Shah Tepe II and Turang Tepe III correspond in time.

In the second half of the 3rd millennium, Turang Tepe — just like in Hissar y — a surprisingly rapid cultural development can be observed. The settlement here also expands, reaches urban dimensions, prosperity visibly increases dramatically, and metalworking shows a sudden boom. These phenomena can undoubtedly be assessed as the results of direct and systematic contacts with the Mesopotamian high culture, which existed in this region already during the early Sumerian dynasties — states, for example, Amiet (cf. Amiet 1977, p. 137).

However, the intensity of these relations became even more intimate in the second half of the 3rd millennium, and eventually led to the appearance of the most characteristic manifestations of Sumerian culture and religious cults at Turang Tepe. A huge Sumerian-type stepped tower temple was excavated from the highest hill of the large settlement consisting of several mounds of rubble in the mid-1970s. Its detailed description has not yet been published, so all we know from the preliminary notice is that it is much larger than the previously discovered ziggurat of Altin Tepe in southern Turkmenistan (cf. Amiet 1977, p. 548), which was also of considerable size (its façade was 55 m long and at least 12 m high; - more details in 1. below).

This monumental stepped tower temple is a testament to the fact that during the Akkadian and Ur III periods, Turang Tepe was no longer just a place of regular Mesopotamian trade or other relations, but also of a large number of newly arrived Mesopotamian-Sumerian settlers, among whom - as evidenced by the design of the enormous ziggurat, the organization of its construction and its successful execution - obviously included a significant number of former leading elements (priests, scribes, master builders, high-ranking state officials) and skilled artisan middle classes (masons, metalworkers, etc.). In other words, we can rightly conclude from 6 phenomena that

794
===== PAGE 784 =====

| that during the Akkadian period, large communities - often entire cities - migrated en masse from Mesopotamia,

Undoubtedly, the emergence of equally large, organized groups
| we must also reckon with in southern Turkmenistan in the second half of the 3rd millennium, | during the Namazga IV period there, which Soviet archaeologists | generally date to between about 2500-2000, but many signs indicate that it belongs to a | century or two earlier, about 2600-2200, or perhaps 2500-2100 (cf. e.g. Propyl. Kunstgesch. vol. XIII. p. 171; Amiet 1977, p. 548).

p.). From the beginning of this period, the pace of cultural development in the settlements of southern Turkmenistan also noticeably accelerates. The phenomena indicating the beginnings of urbanization, already noticeable towards the end of the preceding III. period, led to the formation of real cities in the second half of the 3rd millennium, e.g. Namazga Tepe and Altin Tepe. At Altin Tepe, during the Namazga IV period, we can also observe the rapid growth of the settlement, the built-up area expanding to at least 80 hectares. According to the excavating Soviet archaeologists, Altin Tepe was a respectable city surrounded by walls during this period. The most significant building is the already mentioned stepped tower temple, the first version of which was built in the second half of the Namazga IV period, around 2300. In its original form, its floor area reached 27 x 10.5 m, with four — gradually decreasing — floors, the total height of which was at least 8-10 m. Its walls were divided by vertical pillars projecting forward at an angle and recessed niches, typical of representative Mesopotamian buildings (cf. e.g. Propyl. Kunstgesch. Vol. XIII. p. 175; Amiet 1977, p. 548).

This ziggurat is therefore identical in every detail to the Sumerian tower temples of the 3rd millennium BC, and its origin — taking into account the rapid growth of the settlement and its fortification with a wall that can be observed at the same time — can very likely be directly linked to the appearance of Mesopotamian-Sumerian groups between 2400 and 2300 BC. This is also supported by the fact that tin bronze, which was already produced locally, appeared at Altin Tepe — but also elsewhere in southern Turkmenistan — almost exactly at the same time (cf. e.g. Propyl. Kunstgesch. Vol. XIII. p. 171). In the next period — Namazga V — which Soviet researchers date to around 2000-1700, but (similarly to period IV) this stage can probably be dated earlier, more likely to be around 2200/2100 and 1900/1800 (cf. what was said above about the dating of the Namazga IV period; also see the literature), the Altin Tepe ziggurat was rebuilt several times and greatly expanded. In its final form, its facade was extended to 55 m, and the second step was raised by more than 3 m. The dimensions of the upper floors have not been precisely determined, but the building reached a height of at least 12 m: the ruins of the first and second steps still stand up to a height of 6.5 m (see Amiet 1977, p. 548. Propyl. Kunstgesch. Vol. XIII.

795
===== PAGE 785 =====

175. p.). A very important additional circumstance — and also direct evidence of Mesopotamian-Sumerian traditions — is that a large underground temple was also excavated at the base of the tower temple, just as underground temples are always found next to Sumerian ziggurats (1. Brentjes 1977, 17, p.). The repeated reconstruction and expansion of the Altin Tepe ziggurat during the Namazga V period attests to the presence of a large number of Mesopotamian-Sumerian colonists in southern Turkmenistan during this period, who continued to cultivate the indigenous cults. The substantial expansion of the tower temple at the very end of the 3rd millennium, however, suggests that new Sumerian groups appeared in the southern Turkmenistan region after the fall of the IIIrd Uri dynasty. However, the cultures of southern Turkmenistan — and with them the sites of northern Iran discussed above — show a picture of rapid decline starting from about 2000, which occurred almost as quickly as we could observe their sudden cultural boom starting from the middle of the 3rd millennium. Most settlements are depopulated, and the already relatively developed canal systems soon become unusable due to lack of maintenance. Parallel to this process, however, agricultural cultures develop in the agriculturally viable areas of the Asian grassy steppes: starting from about 2200, the Afanasyevo culture in the Altai-Yenisei region, and around 1700, the Andronovo culture from the Altai to the Urals and from southern Kazakhstan to the southern border of the forest belt. We have already mentioned that, according to the results of recent research, both cultural circles were the creation of agricultural colonists who migrated from Southern Turkmenistan — and partly from the Caucasus region — around the turn of the 3rd/2nd millennium BC, and were ultimately mostly of Near Eastern and Mesopotamian origin. This is also proven by anthropological studies, as well as the many Turkmen and, through them, Mesopotamian parallels found in the finds of the Afanasyevo and Andronovo cultures (I. pl. 29-30. and 525-526. pp.). Therefore, we must definitely take into account the presence of smaller or larger Sumerian groups in the areas of these cultures.

In Transcaucasia, Northwestern Iran (around Lake Urmia and in Azerbaijan), and Eastern Anatolia, the archaeologically clearly distinguishable III. period of the early Transcaucasian civilization, which has already been described in part, began around 2400-2300. (cf. Burney-Lang 1975, p. 127). The most striking and important phenomenon of this period is the rapid development of metal technology, which the vast majority of experts attribute to direct and strong Mesopotamian influences. It has long been well known in research that there are very significant correspondences between Sumer and Transcaucasia in the forms of weapon and tool types, as well as in the technical skills of their production, such that

196
===== PAGE 786 =====

to such an extent that many researchers previously believed that the beginnings of Sumerian civilization also originated in Transcaucasia. Although this does not seem to be provable in the light of recent excavations, there are still many ways to rule out all doubt. It can be shown that in the last centuries of the 3rd millennium, trade and other relations and contacts between Transcaucasia and Mesopotamia assumed very large dimensions. The trade routes probably led through Western Iran to the area of Lake Urmia and from there further to the Araxes valley. This is evidenced by the fact that in the more western areas of the early Transcaucasian civilization — in Eastern Anatolia — these Mesopotamian influences are hardly noticeable (cf. Burney-Lang 1975, pp. 127-128), in the east, e.g. In Georgia, however, a very advanced metallurgical technique suddenly appears around 2300 (I. Burney-Lang 1975, p. 130). Similarly, in Armenia, the settlement of MetSamor (near Yerevan) can be called a metallurgical center as early as the second half of the 3rd millennium, and by around 2000 at the latest, genuine tin bronze was being cast there in large quantities (1. Burney-Lang 1975, p. 137-138).

It is surprising, however, that although we know of well over 400 copper deposits in the Caucasus, in-depth series analyses of early — 3rd millennium — Azerbaijani metal finds preserved in the Baku Museum have clearly shown that these copper objects do not contain the usual impurities of Caucasian copper ores (arsenic, antimony, lead, bismuth, cobalt), and that each piece contains silver, which is very rare in Caucasian copper ores (1. Burney-Lang 1975, pp. 139-140).

In other words, these copper tools were not made locally, but were brought to Azerbaijan from elsewhere — that is, undoubtedly from Mesopotamia. Thus, the work of Burney-Lang, cited above, also states that the Sumerians exported metal objects to Transcaucasia very early on (cf. Burney-Lang 1975, p. 139), and that Transcaucasian metalwork was undoubtedly influenced by pre-Asian, ultimately Sumerian influences, and only at the turn of the 3rd/2nd millennium BC did it reach the stage where it was already external. could continue to develop independently, regardless of external influences (cf. Burney-Lang 1975, p. 147). The aforementioned Transcaucasian find connections, without exception, all testify that in these northern regions in the Akkadian period, between about 2400-2300, in several places, settlers of Sumerian origin appeared in smaller or larger groups, who brought with them the knowledge of advanced Sumerian metalwork and bronze casting. These groups were undoubtedly welcomed by the local Subir-Hurrian chieftains - if they did not come directly at their invitation. We recall in this connection that at the time of Sargon's death the Hurrians had already expanded as far as Kazallu in northern Babylonia and fought together with the Sumerians against Akkadian rule (1. above, p. 783). They also strongly support the idea of writing in Transcaucasia.

797
===== PAGE 787 =====

The probability of the Sumerian migrations of the Akkadian period is also supported by the striking similarities that appear between the Sumerian art, symbolism, legendary and mythological images of the 3rd millennium and the identical phenomena of the Hurrian mythology of the 2nd millennium (more on this later, 1). The emigrants appear to have arrived mainly through Western Iran and settled mostly in the eastern half of Transcaucasia: the Sumerian influences from Mesopotamia are, as we have seen, barely noticeable in the more western areas. However, the direct emanations of Sumerian culture - and, as follows, also some colonial groups - reached much further north. In the richly magnificent of the Maykop princely tomb belonging to the Kuban culture, which modern research generally dates to around 2400-2300, the Sumerian parallels suddenly and without precedent appear extremely strongly (cf. e.g. Burney-Lang 1975, p. 539, note 89). At the same time, it is also very closely connected with the princely tombs of Alaca Hüyük, which are partly earlier and partly contemporary; these, in turn, also show numerous correspondences with the ca. with Uri royal tombs and other Mesopotamian burials between 2800-2600 BC (cf. e.g. Müller-Karpe 1974, p. 355; but also I. pp. 695-696).

The striking Sumerian correspondences of Maykop have already been discussed earlier (our book 1, p. 39), but it is absolutely necessary to deal with them again and with the entire Kuban culture in general, since, according to all the archaeological data currently available to us, this cultural circle played an outstandingly primary role in the cultural development of the more northern Eastern European areas, in the formation and spread of the agricultural and metalworking cultures there - especially the so-called late pit and catacomb cultures (cf. e.g. Müller-Karpe 1974, p. 355).

In this case too, G. Childe recognized decades ago that the rich gold, silver, copper, bronze and other finds of the Kuban civilization bear direct Mesopotamian roots. The burials with carts and yoked animals, for example, reflect the burial order of the royal tombs of Ur, he states. The gold, silver, electron, copper and bronze objects prove excellent quality metalwork, which, apart from Mesopotamia and Syria, was known at that time only from Anatolian finds (Alaca Hüyük, Troy, etc.), but in the latter places it also had organic Mesopotamian connections. Soon, their own mining and local metal industry developed, the technique, motifs and tool forms of which were clearly of Mesopotamian origin. It is also striking that, simultaneously with the Kuban culture, in the second half of the 3rd millennium BC, a short-headed population from the south appeared in the Caucasus foreland, and somewhat later in South-Eastern Russia, and displaced the earlier long-headed Proto-European type, just as this process can also be observed in the eastern Mediterranean region in the middle of the 3rd millennium BC, at the beginning of the Bronze Age there (cf. Childe 1949).

798
===== PAGE 788 =====

We can add that somewhat earlier, in the first half of the 3rd millennium, the same anthropological change took place in Anatolia: the previous, predominantly long-headed Euro-African and proto-Mediterranean population was displaced by short-headed people arriving from the east (1. pp. 770-771).

M. Rostovtsev and F. Hanéar also recognized very early on, in the 1930s, that the role of Sumerian culture in the formation of Kuban culture was indisputable (cf. also Müller-Karpe 1974, p. 355). Müller-Karpe lists the exact Sumerian and Anatolian correspondences of the Maykop finds over half a page, the latter of which, however, are ultimately largely of Mesopotamian origin (1. Müller-Karpe 1974, p. 355).

At Maykop, for example, two types of sheathed axes are identical in detail to slightly earlier and contemporary Sumerian forms, but there are also numerous other Mesopotamian parallels. Turquoise and lapis lazuli beads are clear signs of Maykop's connections with Iran and Afghanistan (cf. e.g. Burney-Lang 1975, p. 168). The pole-mounted metal figures and openwork metal plates found at Alaca Hüyük, generally interpreted as military insignia, are also found at Maykop, but here they clearly crowned the bars of the portable decorative ceiling above the death bed.

In other words, these finds prove that the culturally active population of the Kuban civilization in the second half of the 3rd millennium belonged to the same Sumerian long-distance trade network, of which the settlements known so far (Alaca Hüyük, Troy, Horoztepe, Tepe Hissar, Turang Tepe, Altin Tepe, Mundigak, etc.) were also important intermediate stations.

The close Mesopotamian parallels, and even often detailed coincidences, that appear in large numbers in the Kuban princely tombs of the second half of the 3rd millennium BC, however, also make it highly probable that Mesopotamian-Sumerian colonial groups appeared along the Kuban during the Akkadian period. This is also explicitly confirmed by the map-like engraving on one of the Maykop silver vessels, already described in Volume I, which depicts the region from the Caucasus to the Persian Gulf, with mountains at the top, the Tigris and Euphrates rivers below, and the flora and fauna characteristic of the northern and southern regions (Book I, p. 39). In order to better illuminate the real Transcaucasian connections of the so-called "Kurgan culture" (1. pp. 431-439) already mentioned, it is necessary to mention that the "kurgan" or mound burial form appears in the area of early Transcaucasian civilization already in its second period, in the first half of the 3rd millennium. The age of the huge kurgan of Uctepe, located in the Mil-steppe of Azerbaijan, is 2539±120 and 2867±230 BC according to two "C" measurements (cf. Burney-Lang 1975, p. 191, and p. 544, note 21).

This means that the tomb must definitely be dated to the first half of the 3rd millennium, if not earlier: Narr referred to 799
===== PAGE 789 =====

according to his handbook, it belongs to the end of the 4th millennium (1. Narr 1975, p. 470). The important thing about this mound is that it is much older than any kurgan in southern Russia (more on this later), so it cannot serve as a basis for interpreting it as a transcaucasian expansion of some northern -- Indo-Germanic - "kurgan culture", as M. Gimbutas does (cf. e.g. Narr 1974, p. 470). On the contrary, all signs point to the fact that the gradual expansion and effects of early Transcaucasian civilization reached the Pontic region and the northwestern shores of Lake Caspian as early as the first half of the 3rd millennium.

The northeastern expansion of this culture in the III. period is indicated by the finds of Lugovoe, Velikent, Kayakent and Mamaikutan, which settlements, however, existed already in the II. period (cf. Burney-Lang 1975, p. 131). In other words, the early Transcaucasian civilization in the northeast had already reached the Terek River line in the first half of the 3rd millennium: Kayakent lies about 400 km north of Baku. Apparently, the southern settlers also arrived in the Kuban region via this route.

In the III period of the early Transcaucasian culture, mound-grave structures appeared in several places. The best known are the rich kurgans near Trialeti in eastern Georgia, the earliest group of which is now dated to between 2300 and 2000 (cf. Burney-Lang 1975, p. 192), thus roughly contemporaneous with the kurgans of the Kuban culture.

All this indicates that the custom of kurgan burial spread from the eastern regions of the early Transcaucasian civilization to the north of the Caucasus.

We are particularly interested in the fact that burials with oxen and carts appear in the earliest group of kurgans near Trialeti (1. Burney-Lang 1975, p. 191), which can be first recorded in Sumerian tombs between 2800-2600 (Kis, Ur royal tombs), and then around 2500 at Alaca Hüyük, but there with partial animal burials (1. above, pp. 774-775). In Trialeti tomb no. 5, dated ca. 2200-2100, for example, a four-wheeled cart, complete cattle skeletons and a dagger made of tin bronze were found, which is completely identical to the one found in Mesopotamia in the IIIrd millennium BC. with daggers used during the Ur dynasty (1. e.g. Burney-Lang 1975, p. 192; Narr 1975, p. 519). Another early grave yielded skulls and shin bones of cattle 616. The joint burial of the skin — just as at Alaca Hüyük — is also likely here (cf. Narr 1975, pp. 518-519).

We must say a few more words about the cultural development of southern Russia. First, because the names of the cultures of this area have been subject to considerable uncertainty in the archaeological literature for decades; second, because recently M. Gimbutas and some of his followers have thoroughly overturned the traditional chronological classification of the Neolithic and Early Metal Age cultures of Eastern Europe (cf. also pp. 431-439).
80
===== PAGE 790 =====

We must begin our review with the Cucuten Tripolje culture of Moldavia-Western Ukraine, which, according to the consistent findings of international archaeological research, arose in the Romanian part around 3500 and in the Ukraine somewhat later, around 3300 (cf. e.g. Müller-Karpe 1968, Tafelband, table no. 4). Its earliest Ukrainian stage (Tripolje A) can only be detected in the westernmost part of Ukraine, while the much later Tripolje B and C stages are also found further east (cf. e.g. Müller-Karpe, 1968, p. 167).

To the east of the areas of the Tripoli culture, between the Dnieper and the Sea of Azov, primitive pottery appeared in several places (Sulayev, Igren, etc.) in the first half of the 4th millennium, with pointed-bottomed vessels decorated with comb-pits, but still without agriculture, only in some places (e.g. Igren) with traces of incipient animal husbandry, i.e. at the sub-Neolithic stage (cf. Müller-Karpe 1968, p. 165). We already know that this vessel decoration can be detected at the earliest at /Iani Tepe, a distant Azerbaijani colonial settlement of the 5th millennium Chalcolithic cultures around Lake Urmia (Dalma Tepe, etc.) at the very beginning of the 4th millennium (cf. Burney-Lang 1975, p. 81). It must have reached the northern Pontic regions from there. This cultural connection is also confirmed by the fact that polished stone vessels made with remarkable technical knowledge were found at one of these sub-Neolithic Dnieper settlements (Sulayev), which also testifies to the preservation of ancient Transcaucasian traditions, where the technique of stone polishing was already mastered in the 5th millennium (cf. e.g. Burney-Lang 1975, pp. 82-83).

From these beginnings, the Dnieper-Donetsk culture developed, perhaps as early as the end of the 4th millennium, but more likely only around 3000 (1. below), with which the aforementioned terminological confusion also began. Its synonymous names are the following: 1.) North Pontic culture; 2.) Mariupol culture; 3.) Early Ochre Grave culture; 4.) Early Pit Grave culture; 5.) Kurgan I culture (Gimbutas). This culture represents the beginnings of the Neolithic in the region east of the Dnieper, although the population still lived largely from fishing and hunting. Traces of agriculture and animal husbandry can only be detected in the western parts, closer to the Dnieper - i.e. the Tripoli culture. Likewise, pottery is still very rare, what is there is a later development of the earlier comb-pitted sub-Neolithic type (1. e.g. Müller-Karpe 1968, p. 166 and 1974, p. 358).

The burials are characteristic: dozens of skeletons are found placed in long, trench-like pits on a thick layer of ochre, closely next to each other, sometimes even on top of each other, and generously sprinkled with ochre from above (cf. e.g. Müller-Karpe 1974, p. 358). This burial custom served as the basis for the name ochre grave or pit grave culture.

We have concrete finds to determine the dating of the Dnieper-Donetsk culture. It can be proven that the Tripoli culture
801
===== PAGE 791 =====

It appeared during the B/I stage: typical Tripolje B/I ceramics were found at several sites of the Dnieper-Donetsk culture, and conversely, Dnieper-Donetsk ware with comb decoration first appears in the layers corresponding to the B/I stage in the settlements of the Tripolje culture (cf. Müller-Karpe 1968, p. 169). Since Tripolje 2/1 stage belongs to ca. 3000-2800 (1. e.g. Müller-Karpe 1968, Tafelband, table no. 4), the development of the Dnieper-Donetsk culture can be dated to ca. 3000 at the earliest. It is also worth mentioning that in this culture and in some late Trypillian settlements, the custom of thinning ceramics with shell powder appears at the earliest, in the first half of the 3rd millennium (cf. e.g. Müller-Karpe 1974, pp. 349-350), which then spread to more northern - "Finno-Ugric" and other - areas in the second half of the same millennium.

After the last phase of the Tripolje culture (Tripolje C, ca. 2600-2400), starting around 2400, in Western Ukraine we find the Gorodok group in the north and the Usatovo group in the south, which are also collectively called the Tripolje C/2 stage. It is in these cultures that kurgans first appear in these areas (cf. Müller-Karpe 1974, p. 350), after they appeared somewhat earlier further east and in the Caucasus, at the beginning of the culture often called the Kurgan culture (1. | Müller-Karpe 1974, p. 351).

This cultural horizon began around 2500 (1. Müller-Karpe 1974, p. 777), and covers roughly the second half of the 3rd millennium. In fact, it is nothing more than the successor (but not the origin!) of the Dnieper-Donets culture in the eastern Ukrainian areas, but extending as far as the Volga. In this period, we find only individual burials, in sharp contrast to the collective graves of the Dnieper-Donets culture. In other words, we must sharply distinguish this culture from the ochre-strewn mass graves of the Dnieper-Donets culture, which began around 3000, and which testify to a completely different burial rite and fundamentally different afterlife concepts. The only common feature is that the custom of sprinkling with ochre persisted in the second half of the 3rd millennium. That is why the name "ochre-grave" or "pit-grave" culture applies to both the Dnieper-Donetsk culture and the Kurgan culture, these two completely different cultural circles being distinguished only by the fact that the former is called the Early Ochre-grave or Early Pit-grave culture, and the latter is called the Late Ochre-grave, Late Pit-grave I culture. However, this terminology is extremely misleading, because it involuntarily creates the impression that the two cultural horizons in question are organically connected, that is, we are dealing with the early and late stages of the same cultural circle, although - as we will see below - the Dnieper-Donetsk culture cannot be ethnically linked to the Kurgan culture. As for the custom of sprinkling with ochre, it is so widespread that

802
===== PAGE 792 =====

x1661662681 rite (already occurring in the Neanderthal Paleolithic) that, viewed by itself, cannot prove any cultural connection. In order to avoid such a confusion of concepts, several researchers have recently restricted the term "bucket grave" or "pit grave" to the second half of the 3rd millennium, i.e. to the Kurgan culture, and in the first half of the millennium they consistently speak only of the Dnieper-Donetsk culture (cf. e.g. Müller-Karpe 1974, pp. 358 and 777). Since this terminology is much more precise than the one previously described, we will stick to it in the following.

On the other hand, the practice employed by M. Gimbutas and some of his followers, extending the concept of Kurgan culture to the Dnieper-Donetsk culture under the name "Kurgan I culture", is completely unacceptable and unmethodical; — for the simple reason that in the first half of the 3rd millennium we do not yet find Kurgans in Southern Russia (1. above). It goes without saying that Kurgan culture can only be called Kurgan culture if Kurgans are present.

In this phase, in the middle of the 3rd millennium, the short-headed element also began to spread from the south in the population, in contrast to the long-headed element of the Dnieper-Donets culture, with the Paleo-Europeans. In some settlements it can also be observed that the two populations with different anthropological constitutions lived side by side for a certain period of time: the forms and types of the Dnieper-Donets culture maintained themselves in several places until around 2400, while the pit-grave colonists of southern origin appeared around 2500 (L. e.g. Burney-Lang 1975, p. 163). Therefore, from an ethnic point of view, it only causes confusion to call the Dnieper-Donets culture an "early pit-grave" culture.

Apart from the Tripoli areas, this pit-grave, ochre-grave or kurgan culture in the narrower sense is the first mature Neolithic culture in Eastern Europe (cf. e.g. Müller-Karpe 1974, p. 358). In accordance with these new observations and the precise chronological division, modern research sees the focus of the kurgan culture in the Kuban culture.

We can take it as proven — states Müller-Karpe, for example — that the Kuban culture played a decisive role in the development and spread of the agricultural, metal-using Copper Age cultural complex that emerged in the second half of the 3rd millennium in the region from the Káspi Mountains and the Volga to Ukraine, i.e. the ochre-grave or kurgan culture.

The metal objects, weapons, decorative motifs, jewelry and vessels found in the area of the culture all reached Southern Russia via Kuban-Maykop. The same applies to the polished stone axes (l. Müller-Karpe 1974, pp. 355—356).

Müller-Karpe's last comment is quite obviously a side-splitting remark, and since then it has been towards the supporters of the "battle axe" theory. In European prehistory research, since the end of the last century brought up again and again.

803

===== PAGE 793 =====

traditional assumption that the Central and Eastern European polished cubes - the "battle axes" — would be the legacies of the ancient, still unified Indo-Germanic core population. They call these "battle axes" (Streitaxtleute), but often also "cord-decorated pottery battle axes" (schnurkeramische Streitaxtleute), since these stone axes often occur together with cord-decorated pottery (1. also map no. 8). However, in recent decades, only the so-called renowned researcher has simply qualified this hypothesis as absurd, as Müller-Karpe later clearly states: "... whether the cord-decorated nurtured by the "ahistorical" trend, most pottery makers were Indo-Germanic belongs to those invented interpretations that extend beyond the limits of scientific knowledge..." (l. Müller-Karpe 1974, p. 485, note no. 4).

We ourselves only mention it because it will help our readers better understand the intellectual historical roots of M. Gimbutas' Kurgan Indo-Germans (l. Gimbutas 1963 and our book III, pp. 431-439).

Let us also take a look at the more northern regions. In the former northeastern fringes of the Tripolje culture, roughly in the Kiev area, the Middle Dnieper culture developed around 2200-2100, with two regional variants: the Kiev group and the Dnieper-Desna group. Both show unmistakable connections with the last stage of the late Tripolje culture, the already mentioned Gorodsk-Usatovo circle, as well as with the pit grave and Fatyanovo cultures, but at the same time there are strong parallels with the more western Corded Pottery culture circle (l. e.g. Müller-Karpe 1974, pp. 359-361).

The origin of the not-so-soon-to-be-later Fatyanovo culture (along the upper Volga, between ca. 2000-1900) is completely uncertain. Almost all researchers attribute it to something else: the late Tripoli or Middle Dnieper culture, the ochre-grave or cord-decorated pottery culture have been considered as well as autochthonous origin, immigration from the Baltic or the idea of certain "fuzzy, southwestern animal breeders" (cf. Müller-Karpe 1974, pp. 361-362).

We would just like to remind you that in light of the above, we must consider the views of our prehistory research regarding the bearers of the Fatyanovo culture to be rather unmethodical, according to which they believe that "Indo-Germans" are discovered with practically 100% certainty in this archaeological culture, and that the ancient Indo-Germanic word matches found in Finno-Ugric languages are explained by their linguistic effects. Of course, it is not easy to get rid of the reflections of the "schnurkeramias battleaxes" ingrained in us for half a century (at that time, the bearers of the Fatyanovo culture were also classified as these "salabardos"), but perhaps it would be useful to take a look at the more recent foreign literature, which builds far-reaching prehistory and language development conclusions on the "Indo-Germans" of Fatyanovo.

804

===== PAGE 794 =====

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To the north and northeast of the archaeological cultures listed so far, roughly in the area of the Eastern European forest belt, from Scandinavia to Northwestern Siberia, in the second half of the 3rd millennium and the first half of the 2nd millennium, we find various cultural groups of the F-Esäs-Pitted Pottery. The most important are the Scandinavian, Baltic, Volga-Oka and Kama-Ural main groups, which are divided into numerous subgroups. The relatively uniform pottery in this vast area is related to the general character of this cultural circle: its bearers were Mesolithic fishing-hunting tribes of roughly the same level of culture everywhere, who took over the pottery from the neighboring agricultural cultures to the south. Similarly, the copper and sometimes even bronze objects that appear here and there are of southern, Caucasian or Ukrainian origin (cf. Müller-Karpe 1974, p. 363). The direct source of northern comb-pitted pottery in Europe is primarily the Dnieper-Donets culture, but this decorative method can also be found in other places in the steppe zone, all the way to the Aral Sea region (cf. László 1961, pp. 118-120). Final

its origin, however, can be traced back to Ilani Tepe in Azerbaijan, and through it to Dalma Tepe around Lake Urmia (1. above, pp. 720-723).

Let us now return to Transcaucasia, where, about two centuries later than the period discussed above, we also find tangible archaeological traces of the Sumerian migrations after the fall of the Ilird Uri dynasty (which we already know can be proven by written records from nearly the same period; — cf. p. 790).

Around the turn of the 3rd/2nd millennium BC, the earlier continuity of early Transcaucasian civilization, which had lasted for more than a millennium, was interrupted. Many ancient settlements were abandoned, but in many places new immigrants appeared, whose archaeological legacy is strikingly different from that of individual sites (cf. Burney-Lang 1975, p. 174). Tin bronze, which had previously been found only sporadically, became widespread at this time with remarkable rapidity, e.g. around Lake Urmia or in MetSamor, where — as mentioned above — we find industrial bronze work around 2000 BC. Almost simultaneously, at the very beginning of the 2nd millennium, the technique and tool forms of metalworking in Syria, northern Mesopotamia and the Zagros region became surprisingly uniform with suddenness (cf. Burney-Lang 1975, p. 194). These phenomena, viewed globally, are undoubtedly mainly related to the historically verifiable fact that the Hurrians extended their power to the aforementioned areas around this time.

The uniformity of metalwork in such a vast area, however, cannot be explained solely by the Hurrian expansion, but can apparently be traced back to the arrival of a large number of metalsmiths and blacksmiths from some highly developed metalworking center to the mountainous regions (l. e.g. Burney-Lang 1975, pp. 194-195).

805

===== PAGE 795 =====

Well, this highly developed metalworking center in the period under examination, around the turn of the 3rd/2nd millennium, could undoubtedly only have been Mesopotamia, where the general anarchy and complete uncertainty of the Isin-Larsa period reached its peak at this time (l. above, p. 789). It is therefore very likely that these metalworking masters migrated from Mesopotamia to the Hurrian territories, where they could have had much better opportunities as respected specialists. This is also supported by the striking fact that also in the 3rd/2nd millennium At the turn of the 2nd millennium, we can observe very strong cultural influences arriving in Armenia and Georgia from the south, from the direction of the Lake Urmia basin — that is, along the ancient main route leading from Mesopotamia to Transcaucasia (l. Burney-Lang 1975, p. 195). Burney does not understand why these southern influences appear with such great intensity precisely at this period (l. ibid.), although he himself later emphasizes that at the beginning of the 2nd millennium, "... the Lake Urmia basin was practically saturated with Mesopotamian cultural elements" (l. Burney-Lang 1975, p. 202).

It seems certain that all these otherwise inexplicable phenomena can only be explained by the immigration of Sumerian groups. At the same time, however, the presence of Sumerian groups in the Hurrian territories — to a lesser extent in the Akkadian period and to a greater extent in the period after the fall of the ȳȳ. Ur dynasty — also clearly explains the numerous surprising 3rd millennium BC parallels with the Sumerians, which appear in the society, customs, religious ideas, mythology, symbolism and art of the Hurrian peoples in the middle of the 2nd millennium BC, and which have puzzled research.

For example, the Hurrian seal cylinders of the 2nd millennium revive pure Old Sumerian motifs that had disappeared from Mesopotamia since the Akkadian period (cf. e.g. Moortgat 1950). Moreover, this style was later adopted by the Assyrians, who had been under Hurrian and Mitanni rule for several hundred years, and who, during the period of Hurrian rule (between ca. 1840-1400 according to the long chronology), were ethnically very mixed with the Hurrians. This also explains why the ancient Sumerian gods surprisingly continued to live in Assyria until the beginning of the 1st millennium: a pronounced cult of Enlil can be attested, and the Assyrian kings often adopted Sumerian god names as their throne names (cf. e.g. Schmökel 1957). The magnificent seal cylinders of the Middle Assyrian period revive the naturalism of the 3rd millennium, and even of the Jemdet Nasr period, a style that reached the Assyrians via the Hurrians (cf. Schmökel 1957 and 1962).

Research has long established that Assyrian and Babylonian civilizations were essentially the adoption and continuation of Sumerian culture. However, while Babylonia did not add anything new to the forms it had adopted, but merely repeated them over and over again in a stereotyped manner, Assyria remained much closer to the vibrant Sumerian culture.

806

===== PAGE 796 =====

to civilization; 86+ developed it further in a creative way; This can only be explained by the fact that some people = who were very close to the Sumerians | had to be ȳ *a 2.

gave new powerful impulses to Assyrian culture in the 2nd millennium. However, historical sources clearly prove that in the first half of the 2nd millennium the whole of Northern Syria and significant parts of Palestine were occupied by the was the Hurrian empire of Mitanni, whose art can be Hurrian people, and in the middle of the millennium, between about 1600 and 1400, Mesopotamia in Northern Mesopotamia, including Assyria, and in Syria the most powerful state traced back in almost every detail to the Sumerian culture of the 3rd millennium.

back. Since Assyria itself was under Hurrian rule for centuries in this period, and Hurrian art is nothing more than the renewal of Old Sumerian traditions, it is undoubtedly the Hurrians who gave the aforementioned impulse to Middle Assyrian art (all this l. Goetze 1936). Similarly, MvOppenheim (Der Tell Halaf) and A.Ungnad (Subartu) also emphatically point out that the Hurrian culture of the 2nd millennium is a direct continuation of the Subir culture of 500-1000 years earlier, which in turn can practically hardly be separated from the Sumerian culture of the 3rd millennium. The motifs of Hurrian art, the social structure, the epics, and myths can all be traced back to the Sumerian forms of the 3rd millennium.

But even in the first half of the 1st millennium BC, in the Hurrian successor state of Urartu, which flourished in the territory of present-day Armenia, the characteristic features of the ancient Sumerian civilization can be clearly recognized. After 2000 years, the idea of the ancient Sumerian theocratic state was once again realized in the social organization. Likewise, the high development of Urartian agriculture and gardening, the huge scale of canalization and agricultural terrace construction works, the perfection of architecture that was uniquely outstanding in the entire Middle East at that time, and the high-quality products of metalwork that reached far and wide, all testify that the Urartian civilization was nourished by very old and deep roots, that undoubtedly reached back to the Old Sumerian times through the Hurrian culture of the 2nd millennium (all this l. Schmökel et alii 1961). Some researchers have already

These multifaceted connections of Urartian culture became apparent at the beginning of the 6521161st century. CFLehmann-Haupt, for example, who carried out the first systematic archaeological research in the area of former Urartu, emphatically pointed out that the Urartians remarkably renewed many ancient Sumerian features, and after a millennial hiatus, a whole series of Mesopotamian motifs of the 3rd millennium reappeared among them (cf. Lehmann-Haupt: Armenien Einst und Jetzt). In short, all these otherwise inexplicable phenomena suddenly become understandable if we consider the settlement of Sumerian groups who migrated from Mesopotamia in the second half of the 3rd millennium BC in the Hurrian territories.

807
===== PAGE 797 =====

However, we can only form a realistic picture of the enormous scale of the Near Eastern movements that accompanied the formation of the Akkadian Empire and the fall of the III. Uri Dynasty if we also include the more distant areas surrounding Eio-zsia in our studies.

We have seen earlier that the development and consolidation of the pit-grave or Kurgan culture in the Eastern European region, extending from the Dnieper to the Volga, must be viewed largely as a consequence of the pre-Asian migrations triggered by the pressure of the Akkadian empire, and that the Mesopotamian-Sumerian origin of the culturally active population of the Kuban civilization is - at least in part - highly probable (cf. also pp. 39-40 of our book, 15).

We have also shown that the southern settlers who appeared en masse in the middle Volga and Southern Ural regions between about 2000 and 1500 can only be directly linked to the large-scale Hurrian migrations that began in the last centuries of the 3rd millennium, as well as to the simultaneous northern expansion of the Kuban civilization, and thus we must in all probability also count among them groups of Mesopotamian-Sumerian origin (see pp. 46-51 of our book).

We have also repeatedly dealt with the huge migrations from the area of the Mesopotamian-originated southern Turkmen civilizations to the West Asian steppe in the centuries before and after the turn of the 3rd/2nd millennium, which led first to the formation of the Afanasyevo and then the Andronovo civilizations (I. pp. 29-31, 320-322, 432-433, 526 and 602-603). These migrations, in turn, are also organically connected with the pre-Asian historical processes just discussed. We saw above that the population of the southeastern coast of the Káspi-tö (Tepe Hissar, etc.) and southern Turkmenistan (Altin Tepe, etc.) first increased in the Akkadian period, then in the 3rd millennium. In the period after the fall of the Ur dynasty, it increased again by leaps and bounds -- undoubtedly as a result of new immigration from Mesopotamia -- and the limited arable land of the oases could not support the increased population. Moreover, in the same period, the catastrophic drying of southern Turkmenistan began, and thus the masses of people of pre-Asian origin of the oases-cultivating cultures were forced to emigrate (cf. e.g. Propyl. Kunstgesch. Vol. XIII. pp. 170-171. and 175.; Staviski 1979, p. 19.), apparently in two major waves, the temporal rhythm in which corresponds exactly to the 16 Akkadian period, or, in the III. repeated mass migrations from Mesopotamia after the fall of the Ur dynasty: the Afanasyev culture appears around 2200, and the Andronovo culture around 1800-1700.

All these massive popular movements, which -- as we've seen, In almost all cases, we must also count with a very high degree of probability the presence of Mesopotamian Sumerian ethnic groups, specifically archaeo-BOS

===== PAGE 798 =====
provide a clear and unambiguous explanation, supported by Seti data, for the massive Sumerian parallels found in today's Eastern European and Western Jalal (Slavic, Finno-Ugric, Altaic, Iranian) languages,

We have also briefly mentioned that the final waves of the large-scale Transcaucasian-&Sak-Mesopotamian population movements that occurred at the beginning of the Akkadian period can still be observed in Central Europe, and moreover, in the times after the fall of the III. Ur dynasty, new colonists of pre-Asian origin appeared in the Carpathian Basin and the territory of present-day Romania, who brought with them the knowledge of bronze casting (1. 23-25., pp. 43 and 45). Let us now take a closer look at these distant European connections. According to the almost general conclusion of the research, the Pécel or Baden-Pécel culture was the northernmost peripheral culture of the Pre-Asian -- South-East European cultural complex, and in its development agricultural settlers who arrived from the direction of Anatolia played a leading role (cf. e.g. Propyl. Kunstgesch. Vol. XIII. p. 99). In the case of this culture too, G. Childe was the first to point out -- already in the 1930s -- the diverse Paleo-Asian-Egyptian connections. He was soon followed by V. Milošic, one of the most renowned experts on South-East European primitive cultures (1. Milošic 1949). Further research then increasingly expanded the evidence for the Anatolian origin of the Pécel culture. Nowadays, there can be little doubt that this culture was formed by a mass colonial wave arriving from the direction of Anatolia, which undoubtedly took place during the Trojan War II. It was associated with large-scale population movements observed at the end of the period, around 2400, during which the city of Troy's 11/8th layer and several other Anatolian-Aegean settlements were destroyed.

The numerous Anatolian parallels of the Pécel culture were compiled by Nándor Kalicz in his excellent monograph "Die Pécelér (Badener) Kultur und Anatolien", to which we refer for details. Kalicz also emphasizes that, according to the evidence of the excavated settlements and burial grounds, the population of the Carpathian Basin increased significantly in the Pécel-Badenian period, i.e. large masses must have arrived from the south (1. Kalicz 1963, p. 82). In addition to the close Anatolian-Proto-Asiatic correspondences of the vessels, metal tools, jewelry, etc., the Budakalasz wagon model and the burials with cattle and carts (e.g. Budakalász, Alsónémedi) in particular reveal the Near Eastern origin of the Pécel culture. The closest correspondences to these are known only from Central Anatolia (Alaca Hüyük), Transcaucasia (Trialeti, etc.) and -- at the earliest -- from Mesopotamia (Ur, Kiš, etc.) (cf. Childe 1949). Moreover, the Budakalasz wagon model completely matches the construction of the chariots found in the royal tombs of Ur (cf. e.g. Hanáér 1956).

Based on all these close Middle Eastern parallels, international archaeological research -- in contrast to Hungarian paleoarchaeology -- has focused on the 60011 (Baden) cult 809
===== PAGE 799 =====

dates the tour to the 24th-22nd century BC (1. e.g. Müller-Karpe 1974, pp. 209 and 265, but also cf. pp. 781-782). Starting with the end of the Pécel culture -- according to Kalicz around 1900, but in the chronology of foreign archaeology about two centuries earlier, around 2100 (cf. the above) -- again large-scale migrations can be detected in the archaeological finds, which again led from the direction of Anatolia towards the Carpathian Basin. These colonists introduced bronze art in the Jaszak-Balkan-Kárpát Basin region (cf. e.g. Kalicz 1963, pp. 87-89).

Most researchers attribute all these migrations to the Hurrian expansion, since most tool forms of the Late Copper Age and Early Bronze Age in the Carpathian Basin show very close Anatolian and Transcaucasian parallels (cf. e.g. Handár 1937 and 1956; Childe 1949). We must therefore assume with a very high degree of probability that among the colonists of Anatolian-Transcaucasian origin who appeared en masse in the Kárpát Basin between about 2400 and 2000, in at least two major waves, there were also representatives of smaller or larger Mesopotamian-Sumerian groups. This idea is significantly supported by the archaeologically verifiable data described above, which testify that a significant number of Sumerian groups were present in the Transcaucasian Hurrian territories already during the Akkadian period (1. pp. 797-798), the ýý. In the period following the fall of the Ur dynasty, Sumerian bronze art spread uniformly throughout the region in question, and the culture of Transcaucasia was "truly imbued with Mesopotamian cultural elements" (1 above, p. 806, or Burney-Lang 1975, p. 202). However, we also have further concrete archaeological data on the appearance of ethnic groups of Proto-Asiatic, including undoubtedly partly Mesopotamian-Sumerian origin, in distant areas in the second half of the 3rd millennium and around the turn of the 3rd/2nd millennium, and indeed from almost the whole of Europe. The earlier waves, which began as early as the beginning of the 3rd millennium and led to the formation of the so-called megalithic civilizations in the coastal areas of Southern and Western Europe, have already been discussed above, together with the Mesopotamian-Proto-Asiatic emanations and migrations that occurred during the early Sumerian dynasties. We have already pointed out there, however, that the later waves of these colonizations left Proto-Asia in the Akkadian period (1, 780, p.).

The ethnic groups of the Bell-shaped Ware culture, however, are connected with the popular movements following the fall of the Urian dynasty, which appeared en masse in Western and Central Europe, from Portugal to England and Hungary at the end of the 3rd millennium and the very beginning of the 2nd millennium (1, map no. 8). Today it is most likely that the cultural sphere was formed by pre-Asian colonists, first in Portugal, and then spread eastward to Central Europe, forming numerous separate regions. \$10
===== PAGE 800 =====

formed. However, it is conceivable that Asia Minor people immigrated simultaneously to Southeast and Central Europe,8 and then these two waves met somewhere in the region of Germany. Immigration from the Middle East can also be strongly supported anthropologically: together with the bell-shaped vessels, a new population appears everywhere, the Asia Minor type of which can hardly be doubted. It is also undoubtedly that the vast majority of this ethnic group immigrated to Europe recently: the ancestors of this type are nowhere to be found in the earlier European population. Signs of advanced metalworking are clearly detectable in each group, and the anthropological data testify to a closed ethnicity, which was probably held together by the knowledge of metalworking (all this 1. Narr 1975, pp. 503-504.).

In other words, the population of the bell-shaped vessel culture was, by all indications, composed of ore-finding and metal-processing groups of pre-Asian origin belonging to the same ethnicity, who preserved their specific culture, burial customs, religious cults, anthropological character and -- necessarily following from this -- their original pre-Asian language for centuries in a foreign environment. -- Here, it is once again shown -- in an archaeologically and anthropologically demonstrable manner -- that the massive Sumerian correspondences found in European languages have specific anthropological reasons.

Additions to the issue of partial animal burials
We must definitely say a few words about the origin and spread of the tradition of burials with carts and draft animals, as outlined in the light of our current archaeological knowledge, because the custom of partial horse burials, which was also characteristic of the burial order of the conquering Hungarians, continued to develop unbrokenly from this tradition. We have already mentioned that the earliest examples of burials with carts and draft animals are known from Sumerian Mesopotamia. Thus, in Urban, Kis (but also elsewhere) several rich graves (so-called "royal graves") dating from ca. 2500-2600 BC were found with carts and occasionally the skeletal remains of cattle or "equidae" -- "onagers" (wild donkeys), possibly horses (cf. p. 430-431) (1. e.g. Müller-Karpe 1974, p. 697). In these Mesopotamian graves, it seems that the whole animal(s) was usually buried, although due to the extremely decayed state of the bones, it is not always possible to determine whether they are complete skeletons. The closest to the Mesopotamian finds are the chariot-and-horse burials of the princely tombs of Alaca Hüyük, dated ca. 2600-2300 BC. As already mentioned, at Alaca Hüyük the remains of sacrificed horse-drawn animals (cattle) were placed on the roofs of the tombs, on the beams covering the burial chamber; here, however, it can be clearly shown that only the oxen were sacrificed.

===== PAGE 801 =====

The bones of the animals, the bones of the legs and probably the skin were buried, i.e. we are dealing with regular partial burials (cf. also p. 774). It should also be noted that in one of the chariot burials excavated in Kis, the bones of the animals were found on the top of the grave, even though the chariot itself was removed from the tomb, away from the deceased (cf. e.g. Müller-Karpe 1974, p. 698), i.e. the burial rite at Alaca Hüyük corresponds in detail to the arrangement of the somewhat earlier Sumerian graves. Shortly afterwards, around 2300, burials with carts and draft animals also appear in Transcaucasia. The remains of carts, complete cattle skeletons and skull and hoof bones of cattle were found in some of the earliest groups of kurgans excavated around Trialeti, dating back to around 2300-2000. A joint burial of the skin is also probable here (cf. e.g. Burney-Lang 1975, 1. Narr 1975, pp. 518-519).

However, we find this burial custom relatively early in distant areas, such as the Carpathian Basin. In the two rich graves of the Budakalász and Alsónémed cemeteries belonging to the Pécel culture, complete cattle skeletons buried with the deceased also appear in the second half of the 3rd millennium, and the graves undoubtedly originally contained carts similar to the Budakalász model (cf. e.g. Narr 1975, p. 213). These close parallels in the burial order, in addition to the numerous other close Anatolian-Proto-Asiatic correspondences, also support the East Anatolian-Transcaucasian origin of the culturally active population of the Pécel culture. Also in the second half of the 3rd millennium, burials with carts and draft animals appear in Southern Russia, in some graves of the pit grave culture, e.g. in the Storozevaya Mogila kurgan along the lower Dnieper River, or in the Kuban region, at the princely grave in Maykop. Six large cart wheels were found in one of the so-called pits about 2-3 centuries later, and a cart model made of clay in the "three brothers" kurgan, where the and cattle bones were found in the soil layer covering the grave (cf. e.g. Müller-Karpe 1974, p. 1034). Here too, the tradition of placing animals above the grave can be observed.

After the antecedents outlined above, in the 2nd millennium, partial animal burials -> now mostly with horse skulls, femurs and probably skin ~ became widespread in Eastern Europe in the area of the so-called catacomb grave culture, the successor of the pit-grave culture: e.g. Gerasimovka I (South Urals), Krasny Perekop IV (Crimea), Staroje Gorozino (near Nikolaev), NemSaj (along the Dniester), a catacomb grave near Asatra-yyy, etc. (v6. Narr 1975, p. 694). But in addition, in some places the chariot sky The tradition of nested cattle burials also survived, both in the catacomb culture (e.g. the yyy near Kutor Kamenka 812

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in Takomabaf, and in Transcaucasia - e.g. Trialeti XVII, 874- year kurgan, which belongs to the very beginning of the 2nd millennium (v6. Narr 1973, p. 695, or p. 519). Apart from the 17th century grave, however, in the middle group of Trialeti kurgans, dated between ca. 2000-1600, burials with oxen and carts are also common (1. Burney-Lang 1975, p. 191), and this tradition continued in Transcaucasia in the second half of the 2nd millennium: from the Lšaen graves located near the shores of Lake Sevan, which date from ca. 1300-1150 BC, numerous remains of chariots were found, as well as skulls, hooves and hides of cattle and horses (1. Burney-Lang 1975, pp. 211-215). 568, and in some Urartian tombs 16- tool and chariot accessories from the first half of the 1st millennium have been found, e.g. at Altintepen east of Erzincan (cf. Burney-Lang 1975, p. 325). In the 1st millennium BC, during the Scythian-Sarmatian period, cattle completely disappeared from the graves (due to the general spread of horse riding, the horse was obviously considered a more valuable and worthy sacrificial animal than cattle), and from these times we know only equestrian burials, mostly without carts. However, in some places - as in the kurgans of the Altai region - oxen-drawn carts and horse-drawn carts were often placed in the grave (cf. Grijasnow 1970, pp. 155 and 195-196). There is no doubt, then, that the custom of burial with carts and draft animals first spread from the Sumerians to Anatolia and Transcaucasia, and then from there to the remote areas of the Eurasian steppe, from the Kárpát Basin to the Altai. On the other hand, it can be clearly stated from the above cursory overview of the finds that we can hardly see any ethnospecific characteristic in the custom of partial animal burials — since even in the graves of the same grave group that are roughly contemporaneous, partial and complete animal burials occur mixed together (e.g. in the earliest group of Trialeti kurgans; I. above). We should therefore think much more that complete and partial animal burials only reflect differences in rank or financial status — perhaps only the generosity or thrift of the heirs. The Sumerian kings were obviously rich enough - or their dignity required it - to bury their chariots with draft animals; the princely families of Alaca Hüyük, on the other hand, were content with sacrificing the skull, shin bones and skin (or perhaps

not "did they get the whole animal?"), while the meat was spent on the funeral pyre. This interpretation is strongly supported by the phenomena observed in the equestrian burials of the Scythian period: the larger a Scythian kurgan, the richer the precious metal and other grave goods, but the greater the number of buried horses, from one to several dozen. The same is true of the kurgans of the Altai region (Paziriks, etc.), and in these it is even possible to determine with precision whether the horse was a tribal leader or the chief of a subtribe. 813

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or was only the head of the clan buried in the grave: the leader of the tribe was given 10, 14 or 16 horses (depending on whether the tribe consisted of 10, 14 or 16 clans), the head of the sub-tribe was buried with 5, 7 or 8 horses (the tribe consisted of two sub-tribes — "wings" or "phratryes"), and the head of the clan received only one horse (cf. Grijasnow 1970, p. 214). That is, the more distinguished or richer the deceased was, the more horses he was given as a means of travel to the afterlife, or the more horses the family could sacrifice. On the other hand, in poorer Scythian graves (again, ethnically the same people, just like in the Trialeti kurgans), partial horse burials were found in more than one case: In simple kurgans, which reach a height of barely one meter, the dead also lie in underground, but smaller burial chambers. Their weapons consist only of ordinary iron swords, spears and arrows. The only symbols of horses are bits and parts of the bridle." (1. Grakow 1978, p. 30) Unfortunately, we cannot deal with this very important issue in more detail within the scope of this volume. We hope that this is enough to arouse interest, and perhaps someone will undertake a monograph-like treatment of complete and partial animal burials. 814

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He Chapter 3
y REASONS FOR MIGRATION FROM THE AREA OF PROTO-ASIAN CIVILIZATIONS
I. Tülnepeseds

As we have seen above, the primary food-producing cultural centers that emerged in the Middle East around the 9th millennium BC already formed a connected chain in relatively large areas by the beginning of the 7th millennium BC, practically extending to the entire areas of East Asia that could be cultivated without irrigation. Then, at the turn of the 7th/6th millennium BC, those huge migrations began, as a result of which, barely a millennium later, around 5000 BC, the food-producing economic form became established throughout the Middle East, the Balkans, the southern areas of the Carpathian Basin, and the western half of Iran.

The question inevitably arises: how can these mass migrations from East Asia be explained, which — as we have shown — were repeated in waves at relatively short intervals starting from the very beginning of the 6th millennium, and brought new and new masses of agricultural settlers of East Asian origin to increasingly distant regions of Western Eurasia?

The primary and most important reason is undoubtedly that by the beginning of the 6th millennium, the populations of the pre-Asian societies, which had been engaged in food production for at least two millennia, had multiplied explosively due to the incomparably higher productivity level and surplus product of the agricultural-animal husbandry economy compared to the fishing-hunting stage.

It is well known that in the hunter-gatherer economic stage, a family of 4-5 members requires about 1 geographical square mile (7420 x 7420 m?), i.e. a little more than 55 km?, which corresponds to a population density of about 0.07-0.09/km?. Ethnographic studies conducted recently among backward and isolated ethnic groups still living a hunter-gatherer lifestyle in all parts of the world have repeatedly proven that this extremely low population density exists everywhere with an inexorable law, simply because the game and fish stocks, or edible berries, of a given area are not sufficient to support a large number of people.

In Hungarian, this means that the population size remains roughly constant at the fishing-hunting stage, since the increase above 0.07 —0.09/km² is forced to migrate and take over new hunting areas (cf. e.g. László 1981, pp. 38-39). As an illustrative example, we mention that with such a population density, the population of today's Hungary would only be around 6,500-8,400 people, 815

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which also makes it indisputably clear why you cannot speak of the ethnic and linguistic identity of the populations of larger areas as fishermen and hunters. about the equalization of ethnically more or less uniform sub-populations, about the development of 6 common "language family basic languages". Anyone can estimate how likely it is that such an event would occur under conditions when barely 3-400 people roam the territory of a modern county, grouped into 8-10 hordes, each numbering 30-40 people, which constantly wage bitter battles among themselves for the possession of hunting grounds. However, the thesis of both the Indo-Germanic and the Uralic or Finno-Ugric "Ösnepe" and "language family basic language" is based on the a priori assumption that such uniform ethno-linguistic formations would have existed already in the 4th millennium BC, i.e. at the fishing-hunting stage. However, taking into account the basic requirements of logical thinking, it is evident that such ideas must be referred to the world of fairy tales.

Let us take up the thread of our previous thought again and outline what happens next with the multiplying fishing-hunting hordes. The migration of the increase continues unhindered until the area of a connected continent (or group of continents) is completely populated. Since further expansion is no longer possible after this, bloody clashes for possession of the hunting grounds naturally begin, which necessarily lead to the periodic extermination or decimation of the smaller, weaker hordes, and thus the demographic stagnation mentioned above sets in.

Later, as evidenced by the clashes that were full of losses and had very uncertain outcomes, other methods of controlling population growth also emerged. One such method was the exposure or killing of weak, sickly, and seemingly non-viable infants, as was the custom, for example, in ancient Sparta as early as the middle of the first millennium BC. Similarly, the marriage taboos found in almost all primitive groups, elaborated in intricate detail and embedded in a religious framework, serve to curb the unlimited growth of the population, the strict observance of which is ritually obligatory for all members of the community. The separation of spouses — also ritually justified — is also common: men gather in male alliances, and only those who are determined by the community's magician in the context of religious ceremonies can approach their wives. We also find a further developed form of this custom in Sparta, which jealously preserved archaic traditions, only the justification was no longer . They live in "men's houses" and only on certain "auspicious" days of the year, religious, but bureaucratic-political; the separation of men was intended to provide a more rigorous training for warriors.

- The exact analogies of these conditions have been unanimously found in recent years by ethnographers researching various backward ethnic groups in the most diverse regions of our Earth.

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sa, Let us now look at what happens when the peoples of a certain geographical area switch to a food-producing economic mode in rapid succession, within a relatively short period of time, as happened in the Middle East about 10,000 years ago.
In the first stage, the population can increase without limit. It is a well-known fact that with agriculture and animal husbandry, the same area is capable of supporting at least a hundred times as many people as on the fishing and hunting economic stage — of course, taking into account the relatively low productivity level of primitive Neolithic agriculture. This means that after the establishment of food production, the population density of a given area can increase from 0.07 — 0.09/km2 without hindrance to about 7-9/km2 without the population having to migrate.

To stick with the previous example, the area of Hungary could support approximately 650-840,000 people in the Neolithic period. With such a population density, however, we can rightly expect constant contacts, lasting relationships and a high degree of ethnic mixing, as well as the development of a more or less understandable, universally used lingua franca.

According to human calculations, we can therefore only speak of "primitive peoples" or "basic languages of a language family" in Neolithic conditions, as we have repeatedly and emphatically pointed out in our volume III (cf. pp. 408-410, 421-423, 442-444, 471-472, and 504-505).
Going further: as soon as the population density reached the aforementioned critical limit of 7-9/km², the agricultural production of that time could no longer provide for the livelihood of the further increase, and this sooner or later led to famines. The only way out could obviously be the migration of the surplus population and the opening up of new farmland, and at first — as long as irrigation and canalization were still unknown — settlers could only go to areas where natural precipitation ensured crops. We will see below that the very first migrations were indeed directed exclusively towards regions with more rainfall.

After these considerations, let us try to determine approximately with the help of a model calculation how much time is needed for the population of a given area to convert to an agricultural-animal husbandry lifestyle to increase a hundredfold. In the calculation, we must start from the empirical fact that in Europe the population doubled approximately every 200 years under medieval conditions. However, we can obviously only apply this formula to the core-neolithic societies that were still practicing primitive agriculture with considerable corrections, and — especially at the beginning — we must count on a much slower pace. On the other hand, on the other hand — with the opposite sign — we must also take into account that the medieval Eu-817

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The average rate of reproduction of the fly was greatly restrained by the fact that the population density was relatively high from the beginning of the era, and the population could only grow to the extent that the development of productivity allowed. In the coreneolithic East Asia, however, at the starting point, i.e. at the time of the transition to agriculture, only the already known population density of 0.07 - 0.09/km² could have existed among the earlier fishing-hunting hordes, i.e. the population could have increased by a hundredfold without any significant increase in crop yields. Thus, with careful consideration, we can calculate that the population doubled approximately every 250-300 years in coreneolithic times, which means that the population of a given area increased by a hundredfold approximately 1700-2000 years after the beginning of agriculture.
If we project the results obtained with this model calculation onto the known archaeological data of the development of culture in Pre-Asia, we find surprisingly accurate matches. We know that food production in Pre-Asia began around the turn of the 10th/9th millennium BC — for now only in smaller foci — and by the beginning of the 8th millennium BC it had become consolidated and spread over larger, contiguous areas, which extended in a more or less wide band from Western Anatolia
They stretched through southern Syria, eastern Anatolia and northern Mesopotamia to the Zagros mountain range, with a considerable southern projection towards Palestine (see map no. 1).
Now, if — in accordance with the archaeological findings — we place the final development of the aforementioned primary Neolithic zone and the beginnings of large-scale population growth around 7900-7800 BC, then the population of this extensive region would have reached that critical limit after 1700-2000 years, i.e. between 6200-5800 -- the 7-9/ km² population density -- after which the further population growth was forced to emigrate.

Let us now see what the archaeological finds reveal. It is nothing more than that almost exactly in the period obtained by the above calculation, the expansion of the primary pre-Asian agricultural cultures into the neighboring areas begins and takes on ever greater dimensions: starting from the turn of the 7th/6th millennium, Neolithic settlements founded by pre-Asian agricultural colonists appear one after another in Greece (Nea Nikomedia, Sesklo), Crete, the island of Cyprus, Transcaucasia up to the Kura and Araxes regions, and even on the coasts of Dalmatia, Sicily, Italy, and southern France (the so-called Cardium pottery cultural circle). In other words, -- as we have already mentioned -- these are the first
Colonizations were, without exception, directed towards areas that could be cultivated with the help of natural rainfall,
Shortly afterwards, around 5400, the migrating Near Eastern settlers reached Central Iran (Tepe Sialk) and colonized the previously unexploited, rain-poor southern Mesopotamian and Susiana (Elamite) plains, after having been cultivated by the Samarraites somewhat earlier, around 5600-5500.

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The technique of irrigation-canalization agriculture was developed in the area. However, the migrations continued in the last centuries of the 6th millennium, and by the 5000s, the Near Eastern agricultural settlements had already colonized the Balkans (Starvevo, Karanovo culture) and the southern regions of the Carpathian Basin (Körös culture), and in the east they reached southern Turkmenistan (DZeltun culture).

The results of archaeological research therefore truly indicate that population growth reached the critical limit of 7-9/km2 in the primary Neolithic zone of the Near East by 6000 BC, and they clearly prove that during the 6th millennium BC a huge demographic explosion occurred in the areas of the agricultural and animal husbandry cultures of East Asia, during which the agricultural settlers of these cultures literally flooded the surrounding regions from the Carpathian Basin to Central Iran and from the Caucasus and Turkmenistan to the Persian Gulf. This is the process that is often called the Neolithic Revolution after G. Childe.

According to the evidence of the finds, we have distinguished three clearly demarcated peaks in the population movements that occurred in the 6th millennium. The first falls on the turn of the 7th/6th millennium, roughly between 6000-5800, i.e. the time when the population density reached the limit of 7-9/km². This wave led to the Neolithic colonization of Transcaucasia, the Greek mainland, the Mediterranean islands and coastal regions. The peak of the second migration occurred around 5400, and resulted in the inclusion of Southern Mesopotamia, Susiana, and Central Iran in the Neolithic cultural sphere. Therefore, the demographic pressure reached such a level again about 400-500 years later that significant masses were forced to migrate again.
The third major wave took place at the end of the 6th millennium, i.e. about another 400 years ago, and with it the agricultural-animal husbandry economic form reached the Balkans, the Carpathian Basin and Turkmenistan.
These larger migration cycles of 4-500 years can then be detected with considerable regularity in later times, which undoubtedly indicates that at that time, within about half a millennium, population growth again greatly exceeded the limit set by the extremely slow increase in productivity.
However, the demographic situation changed fundamentally with the spread of flood-fed agriculture and the large-scale construction of canal systems on the Mesopotamian and Elamite plains, which occurred around the first half of the 5th millennium and resulted in a dramatic increase in crop yields. Thus, the population on the southern plains could grow much faster than in the north, and the demographic impact of the Eridu-Ubaid and the parallel Susiana civilizations on the northern Mesopotamian, Syrian, eastern Anatolian and Iranian regions is clearly evident: the Endu-Ubaid culture 3, before the Pendusa

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Within a century, it uniformly superimposed itself on the former Samarra and Halaf areas, forcing a large proportion of the population of these civilizations to migrate, and even penetrating as far as Eastern Iran (Tell-i Iblis) (see, pp. 689-691 and 724-725).
Our current archaeological knowledge indicates that the earliest westward migrations started from the area of the Western and Southern Anatolian Primary Neolithic zone (Hacilar, Catal Hüyük, Suberde, Mersin, etc.) at the beginning of the 6th millennium. These migrations created the very first European Neolithic cultures in the Mediterranean coastal regions and on the islands. However, in later times -- notably with the development of the most ancient Balkan and Carpathian Basin Neolithic civilizations (Starévo, Körös, etc.) -- in addition to groups of Western Anatolian origin, large-scale migrations starting from the area of Eastern Anatolian, Syrian, and Northern Mesopotamian cultures (Hassuna-Samarra-Halaf circle) can also be detected. Thus, very close parallels to Halaf pottery can be found from the Aegean to the Al-Duná and Transylvania (cf. e.g. Oppenheim 1931; Heine-Geldern 1950; Moortgat 1945; Childe 1949).
In the Hassuna-Samarra-Halaf cultural area, i.e., apparently, overpopulation occurred only in the middle of the 6th millennium, about half a millennium later than in the Hacilar — Catal Hüyük region in western Anatolia, but the demographic pressure was much greater and lasted longer than in Anatolia at the beginning of the 6th millennium. The Samarra-Halaf population spread over much larger areas; first, they settled the immediately adjacent southern Mesopotamian and Elamite plains, and then soon penetrated in successive waves eastward to Central Iran and Turkmenistan, and westward to the Balkans and the Carpathian Basin (cf. e.g. Falkenstein: Fischer Weltgesch. Vol. II.).

All these connections have been well-known in international archaeological research for decades. R. Heine-Geldern, for example, wrote about these issues in 1955: "Proof that the Neolithic the final civilizations of Greece, the Balkans and the Danube region originate from the Asia Minor-Syria region was provided two years ago ... by Fritz Schachermeyr, and in a very excellent and convincing manner" (L. Heine-Geldern 1955, p. 3). He then continues: "The Neolithic cultural movements can only be compared in terms of their size with the present-day expansion of Western European civilization. Their influence was enormous;" (ibid., p. 4).
Heine-Geldern pointed to the essence of the question with this analogy. Indeed, the migrations and settlements originating from the pre-Asian cultural circle of the Neolithic and Early Metal Age can only be compared with the European colonizations of recent centuries, 8 with their effects and consequences, in all aspects; both cultural and socio-political, as well as ethnic and linguistic. However, with the two very important differences that on the one hand, these pre-Asian colonizations were most
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In some cases, they took the form that we see today only in a few former European colonies, e.g. in North America or Australia, i.e. the population of the colonized areas was overwhelmingly from the mother countries, i.e. it was pre-Asian. On the other hand, even in those areas where the pre-Asian settlements mixed to a greater extent with the indigenous population (cf. e.g. today's Central and South America), the difference in the level of education and social development between the latter and the colonizers was much greater than that between the Europeans and most of the colonial native peoples: apart from one or two backward areas, the Europeans everywhere conquered peoples who were already practicing advanced agriculture and were at a more or less advanced stage socially - not to mention ancient cultural regions such as e.g. India, Southeast Asia, the Middle East or Mexico and Peru -- while the pre-Asian settlers, without exception, found only Paleolithic-Mesolithic fishing-hunting hordes in the colonized areas.

Under such circumstances, there can be no doubt for a moment that in all areas where the Proto-Asian colonizers were in the majority -- and this was the case everywhere, due to the already well-known extremely low population density of the indigenous population living on the fishing-hunting continent -- they preserved their original languages or dialects; and in all regions where they lived together with a significant mass of indigenous population (which, however, could practically only happen if they lured a larger number of indigenous people from the surrounding areas as labor or simply united them; -- we find countless examples of this in the history of European colonization!), pidgin-like mixed languages based on the Proto-Asian cultural language or its dialects had to develop. Just as this linguistic process can be observed in most former European colonies after only 2-300 years, even in places where the colonists were only a tiny numerical minority, the indigenous population was at a low cultural level.

The fact that the former East Asian colonists could spread almost unhindered over vast areas is also explained by international science by what we ourselves have repeatedly pointed out - most recently on the previous page - with emphasis: namely, that the indigenous population of the areas in question, who were still fishermen and hunters at that time, could only represent a negligible number. It will be instructive to familiarize ourselves with F. Schachermeyr's summary lines on these migrations, s It seems that the Aegean region, as well as the Balkans and the Danube region up to the border of Austria, became offshoots of the pre-Asian cultural development in the 5th and 4th millennia. This process can only be imagined as a cultural flow lasting for many centuries, which was partly

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was triggered by the migrations of explorers and traders, but above all farmers in search of fertile land. These - probably initially from the area of Hacilar and Catal Hüyük - first reached Western Minor Asia and then the Aegean. They discovered fertile Thessaly and finally the loess areas of the Danube and Tisza region. Italy was also apparently colonized by these farmers ... With agriculture, the religion of the great fertility goddess also reached Europe ... Together with this pre-Asiatic cultural flow, the indigenous ethnic group and its language from Eastern Asia Minor and Cilicia spread to the Aegean region, the Danube region and Italy. Since the Aegean became the center of this vast area -- stretching from Armenia to Carnuntum (a former Celtic and later Roman city near the Austro-Hungarian border; -- our note) and from the Euphrates to Sicily -- this language family can be called Aegean. These "Aegean" agricultural settlements had "Aegean" place names everywhere in the mentioned region.

In addition to the pre-Asian one, there was a second cultural stream that originated in North Africa, primarily Egypt...

... In both cases we are faced with movements that always took place in one direction, that is, they were not disturbed by any movement in the opposite direction. That is why we speak of a "current." This one-sidedness -- the one-wayness -- of the migrations is explained by the enormous superiority of the cultural potential of the Middle East, and by the fact that the settlers found only wandering hordes of fishermen and hunters in the virgin lands. In more recent times, this process can be roughly compared to the eastern cultural current that started in Russia and covered the whole of Siberia to the Pacific Ocean, or the other that spread in America from the New England coastal states to California." (1. Schachermeyr 1979, pp. 16-17.)

However, we cannot ignore the fact that some researchers mainly view the European Neolithic development -- and even the development of the Neolithic in southern Turkmenistan and eastern Iran -- as a more or less autochthonous process. The argument against a direct pre-Asian origin is usually that the similarities in cultural phenomena are often limited to a few groups of finds.

This objection seems obvious at first glance, 5 when considered together with the already mentioned chronological hypothesis according to which the earliest Neolithic culture to appear in the northern areas of Central Europe, the banded pottery culture, would be older than the Starcevo-Körös culture, or at least contemporaneous with it, it could undoubtedly serve as strong support for the autochthonous origin of the European Neolithic 162186062. As we have already shown, the real situation is that the beginnings of the banded pottery culture are about 1000 years later than the appearance of the Starcevo-Körös culture in the Balkans and the Carpathian Basin, and even in the banded areas, the new ones came in from the south.

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"Alongside and among the migratory agricultural settlers, groups of the Paleolithic-Mesolithic indigenous population living at the fishing-hunting stage can still be detected for a long time, in all regions that were unsuitable for agriculture (1. p. 723). The objection that the archaeological finds of European and other more distant Neolithic civilizations only partially coincide with those of the pre-Asians is generally correct, but methodologically unfounded. It can be clearly stated that it is precisely in the field of the most essential cultural phenomena that we find the closest parallels, almost to the point of identity. G. Childe and several other renowned researchers demonstrated convincingly decades ago, through extensive comparisons of finds, that in the European Neolithic -- but also in Turkmenistan, Eastern Iran and elsewhere -- the pre-Asian fertility cult, the great mother goddess

The worship of the bull, the cult of the bull, and the characteristic cult clay figures of Pre-Asian culture are closely paralleled, but other symbols of religious beliefs, as well as numerous motifs of ceramics, vessel shapes, and architectural and burial traditions, also show extensive similarities with Pre-Asian culture (cf. e.g. Childe 1949; Schachermeyr 1953). We have seen in the above that recent excavations have since largely confirmed the findings of these researchers in almost every respect. However, a reasonable archaeologist cannot expect every cultural phenomenon or each set of finds in two cultures to be identical in every detail, even within the same cultural circle, since experience teaches us that each initially unified cultural circle sooner or later breaks down into regional variants, even in relatively small areas. We cannot expect anything else: the multi-coloured flow of human life cannot be forced into a few templates. On the other hand, approaching individual cultures from the perspective of the aforementioned vibrant life, we must not forget that the inherently limited possibilities of archaeological research preclude, on principle alone, the possibility of getting to know former cultural phenomena in their full splendour. In Hungary, for example, our most renowned and experienced archaeologist, László

Gyula seizes every opportunity to repeatedly and emphatically point out this very important circumstance from a methodological point of view in his writings. It is also sufficient R. Heine-Geldern also sheds light on the issue in his aforementioned dissertation, "Where there are no favorable soil and climate conditions like in Egypt, all decaying materials - wood, cloth, leather, etc. - have long since decayed. Where there are no written records, we learn nothing about the social and state organization, religion and myths, or at most we can only draw vague, completely general conclusions from the finds. If, therefore, - as is indeed the case

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"That's right - in Mesopotamia we find fragments of pottery whose original homeland is actually Syria, or conversely: Mesopotamian-type pottery is found in Syria; then it would be absurd to assume that the cultural exchange between the two areas was limited to the trade in pottery or to the imitation of the forms and decoration of pottery. We must rather assume that they also adopted many other things, of which no traces have remained." (1. Heine-Geldern 1955, 6th edition.) He then explains that the Neolithic-Palestinian cultural development in East Asia eventually gradually, without a break, led to the emergence of the urban Mesopotamian Sumerian high culture in the 4th millennium, while in the more distant regions of the Eurasian continent the waves of the Neolithic cultural current that once originated in East Asia continued to circulate. Due to the direct and indirect influences of Mesopotamia, urban high culture soon spread throughout the entire Middle East, first in Egypt around the turn of the 4th/3rd millennium, and then in the middle of the 3rd millennium in such distant areas as Troy, Crete or the Indus region (cf. Heine-Geldern 1955, pp. 7-8). Moreover, Heine-Geldern continues, even the American Mayan and Peruvian high cultures are not -- independent of primary Asian civilizations. Although in our present study we are only concerned with the West Eurasian connections of the pre-Asian cultures (since, in our opinion, at the present stage of research, we should first examine these closer regions in thorough detail before looking further afield), it is nevertheless worth briefly familiarizing ourselves with Heine-Geldern's lines in this regard, because they very clearly point to the stubborn continuation of a traditional approach that has become almost ineradicable in European prehistory research -- based on mostly unprovable a priori ideas -- once, mostly in the 19th century, in the possession of rather incomplete subject knowledge, the so-called "scholastic opinion" (Lehrmeinung) has developed on certain topics.

"Although Alexander Humboldt had already expressed his conviction a century and a half ago that the high cultures of America originated in Asia, later, under the influence of a prevailing scientific fashion (sic!) all such ideas were rejected. There have always been some researchers who did not bow to the prevailing school of thought and tried again and again to demonstrate the ancient connections between Asia and America, but their work was simply ignored. The independent origin of all Indian cultures became a dogma, and it was considered scientific heresy to even speak of the possibility of Old World cultural influences in America. This situation has only changed in the last few decades." (1. Heine-Geldern 1955, 11. p.)

Heine-Geldern goes on to outline the most important results of recent research in this area, according to which the Peruvian and Central American
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The development of high cultures was in all probability the result of direct cultural influences and trade expeditions from China via the Chen Dynasty (cf. *ibid.*, pp. 12-15).

The actualization of this topic is largely due to Thor Heyerdahl, who in 1947 proved beyond any doubt that sea voyages across the Pacific Ocean were possible in prehistoric times (cf. Heyerdahl 1949). - Let this be enough for now about this extremely exciting research topic, which - as we mentioned - cannot be dealt with in more depth within the framework of this work.

However, it is essential to mention Heyerdahl's latest venture in the winter of 1977/78 and spring of 1978, when he sailed from the confluence of the Tigris and Euphrates, via the Shatt-el Arab and the Persian Gulf, to Oman, from there to Pakistan, to the mouth of the Indus, and then back to the Red Sea, with his sailing ship called "Tigris", built from bundles of reeds based on the model of Sumerian ship depictions from the late Uruk and Jemdet Nasr periods (approx. 3400-3000). The reed ship was still seaworthy in every respect after 5 months, which clearly proved that the Sumerians already had ships capable of deep-sea voyages of several thousand km in the second half of the 4th millennium, and, as can be found in contemporary records and also with archaeological finds, It can be verified that they really reached India and Africa (cf. Heyerdahl 1979).

II. Ecological changes

In the migrations from the area of ancient pre-Asian civilizations, in addition to overpopulation, changes in the biological balance of the natural environment also played a very significant role. The research team of the aforementioned Braidwood's "Iraq-Jarmo" enterprise, which included geologists, climatologists, zoologists, botanists, agriculturalists and soil scientists in addition to archaeologists, also carried out extensive ecological studies in the Middle East in the 1950s. The most important result of this research was undoubtedly the fact that human activity, starting from the Neolithic Age, had caused increasingly large-scale, almost irreparable destruction in nature. For example, It turned out that in East Asia during the Early Neolithic period there were still vast subtropical forests extending from Palestine to the Zagros Mountains, interspersed in many places with distinctly park-like groves. (Evidently, memories of these idyllic prehistoric conditions have survived in the

(in the traditions of the "Golden Age" and the "Garden of Paradise"). These primeval forests provided a sufficient amount of rainfall, and at the same time They also prevented soil erosion. Population growth and the rapid depletion of fertile soil (at first, agriculture was practiced for a long time on all kinds of soils) 825
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vftas) necessitated massive and continuous deforestation -- the Coreneolithic farmers usually burned a section of forest and sowed seeds in the ashes until the soil's fertility reached its equilibrium. Parallel to the rapid decline of forested areas, the moisture content of the 16/66 also decreased significantly, regular rainfall --, and grazing of domestic animals, however, completely destroyed the ecological balance of the landscape became increasingly irregular and absent, and the soil became increasingly dry. This process led, after a relatively short time, to the first Neolithic

In the place of the vast forests that had been cleared over thousands of years, trees remained only in the immediate vicinity of springs and streams, but not even undergrowth grew because the grass, which had been grazed from the roots, no longer bound the soil, and the first heavy rain washed away the arable land. More on all this in I. Lloyd 1981, pp. 39-42. These research results are of paramount importance for our current research question, as they indisputably prove that the gradual erosion of farmland and pastures, the decrease in rainfall, and the increasing ecological destruction and desiccation of the relatively densely populated Neolithic areas of the Middle East were one of the most important causes and triggers of the wave-like mass migrations originating from East Asia that archaeology has documented in the southern part of Western Eurasia. demonstrated in its areas.

In our own time, we can see that the unbridled deforestation of recent decades -- for example in Africa -- has very quickly led to permanent, catastrophic drought and famine in vast areas.

In Book I, we have already indicated that the Subir-Hurrian migrations, which began in the second half of the 3rd millennium BC and lasted for several centuries, and which affected Eastern Europe as far as the Pontus-Volga-South Ural region, and through Anatolia as far as the Balkans and the Carpathian Basin, and at the same time radically upended the previous ethnic relations of Northern Mesopotamia, Syria and Palestine, were demonstrably triggered by such ecological devastation (I. pp. 42-43). Soviet researchers O.M. Dzhaparidze, TNTsubinishvili and B. Piotrovsky, with the help of extensive animal bone studies, clearly demonstrated that during the 3rd millennium in the area of the early Transcaucasian civilization (Kura-Araxes culture) cattle breeding steadily decreased, and more and more sheep and goats were bred (cf. Burney-Lang 1975, pp. 151-152). This striking shift undoubtedly testifies to the fact that the rapid growth of agricultural communities necessitated continuous deforestation, which gradually extended to the higher regions of the mountains, where agriculture could no longer be practiced, but only animal husbandry. However, on the mountain pastures established in the place of former high-altitude forests, only cattle were kept.

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sheep and goats lived more unpretentiously. However, deforestation increased in parallel with the growing needs, and eventually led to microclimatic changes, especially to a drastic decrease in the amount of precipitation. Thus, the yield of agriculture also noticeably decreased, the flourishing farming communities of the valleys and flat areas became increasingly impoverished, and eventually most of the settlements were abandoned: the population moved to the mountainous and hilly regions, where there was slightly more precipitation. This process can be traced very well archaeologically in many places - especially in Georgia (all of these are described in I. Burney-Lang 1975, p. 152).

Likewise, the gradual depopulation of the area of ancient agricultural cultures in northern Iran and especially southern Turkmenistan, the collapse of the civilizations there, which can already be called explicitly urban, around the turn of the 3rd/2nd millennium BC and after, was also largely a consequence of the ecological destruction of the landscape and the drying up of the oases (cf. e.g. Propyl. Kunstgesch. Vol. XIII. pp. 170-171.; Staviski 1979, pp. 19-20. -- but also 1. pp. 29 and 318-319.), although in this case the rapid increase in the population of the region played an important role also its increase in the second half of the 3rd millennium, which -- as we have seen -- was triggered by new waves of migration from the south, from the direction of Mesopotamia (1. pp. 791-796). The limited arable land of the oases could only temporarily support this population, and emigration became inevitable. However, ecological destruction can be detected in some areas much earlier. For example, in the Hacilar-Catal Hüyük cultural circle region of western and central Anatolia, which is one of the oldest Neolithic foci, and where agriculture and animal husbandry were fully developed as early as the 8th millennium. In this region, after the turn of the 6th/5th millennium, the interruption of the continuity of cultural development can be clearly established archaeologically: most domestic settlements are depopulated in rapid succession at the beginning of the 5th millennium, and this settlement hiatus lasts until the last centuries of the 4th millennium, when -- apparently as a result of newer and better agricultural methods and the increasing need for metal by the Mesopotamian high culture -- a population arrived in the central areas of Anatolia from the east, anthropologically completely different from the indigenous population. colonists repopulate it (1. 672, 117-718 and 770-771).

III. The salty

Added to all this -- as a third reason for the emigration -- was the "dread" of riverine farming cultures based on canal systems, which was salinization. Constant irrigation with relatively abundant river water compared to natural precipitation -- especially in the hot climates of the Near East -- 827

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led to the fact that on the one hand the total salt content of the soil slowly but surely increased to a certain level; on the other hand the river water led to the fields largely evaporated in the hot sun, its salt content precipitated, and thus the uppermost layer of the arable soil was particularly damaged. The danger was increased by the fact that as a result of continuous irrigation the groundwater level gradually rose, and thus a large part of the salt content of the deeper layers of the soil was washed to the surface,

From the investigations of Th. Jacobsen we know that, for example, in the area of the southern Mesopotamian city-state of Laga\$ salinization began around 2400 (middle chronology), and slowly spread westward, towards the Euphrates. A thousand years later it had already reached the height of Babylon. Jacobsen showed that in Laga\$ around 2400 wheat still accounted for 16% of all cultivated plant species, three centuries later its proportion had fallen to 2%, and between 2000 and 1700 the contemporary records no longer mention wheat at all. At the same time, parallel to the decline of wheat, the cultivation of barley, which is less sensitive to saline soil, gradually increased, but by the beginning of the 2nd millennium its yield had also declined sharply. According to Jacobsen, not least of all the consequence of the salinization of southern Mesopotamia was that the economic, and thus naturally the political, centre of gravity15 shifted further and further north: first at the beginning of the 2nd millennium from the Sumerian tribal areas, which had by then largely become salinized, to the region of Babylon, and then at the end of the millennium to Assyria, after around 1400 the salinization had also reached the area around Babylon (for more on all this, see 1. Lloyd 1981, pp. 18-19). In this regard, we recall that in the last decades of the Third Ur Dynasty, Southern Mesopotamia was completely dependent on grain shipments from the northern regions, and the lack of these -- due to the invasions of the Amorites -- led to a catastrophic famine within the shortest possible time, which ultimately led to the disintegration of the Third Ur Dynasty empire (1. above, p. 789).

The large-scale reduction in crop yields due to salinization therefore also contributed significantly to the migration of the Sumerian population of southern Mesopotamia at the end of the 3rd millennium and the beginning of the 2nd. Thus, the Sumerian migrations triggered by the Akkadian and Amorite influx, which were already significant in themselves, were undoubtedly intensified by the almost simultaneous salinization into a real migration of people. All this provides a very obvious reason for the fact, which has been recognized by several researchers before, but has not been satisfactorily explained so far, that starting from the end of the 3rd millennium BC, the Mesopotamian-Sumerian culture almost flooded the more northern areas of Eastern Europe and Western Asia (cf. e.g. Childe 1952; Randa 1954, de 1, III, our book 321, p. 18.)

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Chapter 4
EMPROPRIATE CONNECTIONS OF MESOPOTAMIA-SUMFRIENDLY ORIGIN IN WESTERN EURASIA

However, the migrations and colonizations originating from Pre-Asia, described in Chapter 2, which are clearly archaeologically proven, have, in addition to archaeological finds, also recently demonstrated in the field of ethnography massive Mesopotamian-Sumerian parallels and similarities in almost all ethnic groups of Western Eurasia. Within the framework of this work, we cannot deal exhaustively with this extremely diverse, in fact huge topic requiring volumes. Thus, we can only present excerpts, primarily of course, the more distant Mesopotamian connections of the Finno-Ugric-Hungarian folkloristic similarities or similarities that interest us more closely. 1 y

It is well known that in the theory of the Finno-Ugric origin of the Hungarian ethnic group, an important role was played by the Hungarian-Finno-Ugric ethnographic parallels, which in many cases are artificially constructed (cf. e.g. Kandra Kabos's Hungarian analogy of "Vogul, heaven"; 1. II. book, p. 330, or Kandra 1897, p. 434), but which are often truly found. For this reason, it is especially important that our readers at least in broad outline understand

to become familiar with the current results of international research in this area, which are mostly ignored by domestic scholarship — with the exception of commendable exceptions that have emerged in recent years, mainly in the field of folklore studies.

To our knowledge, F. Lenormant was the first to point out the great similarities between the mythology, religious ideas, superstitions, and magical ceremonies of the Finno-Ugric and Altaic peoples, on the one hand, and the close pre-Asian parallels and Mesopotamian roots of all these ideas - and of all North Asian shamanism in general - on the other hand, in his book "Die Magie und Wahrsagekunst der Chaldäer" (1878). The German edition of the original French edition published in 1874

a completely new version of the work, substantially improved and greatly expanded ("... durch eingehende Revision und erhebliche Erweiterungen zu einer völlig neuen Ausgabe umgestaltet" — writes Lenormant in the preface). At present, it is not our task to describe all the massive coincidences that Lenormant exhaustively discusses and systematically proves over 571 pages, so we can only limit ourselves to mentioning a few characteristic parallels, referring for details to the original work of this pioneer of Sumerology — one of the most famous —

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- 1.) The Kalevala's triad of gods (Ukkó, Vejnmöjnen, Ilmarinen) corresponds exactly to the Sumerian triad of An, Enlil, Enki. In particular, the figure of Vejnmöjnen completely covers the properties and functions of the Sumerian Enki (L. Lenormant 1978, pp. 235-237).
- 2.) The land of the happy, the abode of heroes, the Finnish Kaleva is obviously the same word as the Sumerian kalama ("land of the strong", "princely land", "the dear land"), which was also the name of the Sumerian country, in the sense of "homeland" (L. Lenormant 1878, p. 243).
- 3.) The Sumerians placed the homeland of evil spirits and harmful demons in the surrounding wastelands, in the sandy deserts of Arabia, while the Finns - in a completely analogous way, in accordance with the geographical features of their northern homeland - in the region of the icy deserts of the Arctic Ocean. This parallelism may seem forced at first glance, but a closer examination clearly shows that despite the completely different geographical environment, the original Sumerian idea is still being washed out: one of the songs of the Kalevala describes in detail the dwellings of evil spirits, from where they attack people, and there is also a mention of the "land of sand", although there are no sandy deserts anywhere in Finland and its surroundings (see Lenormant 1878, pp. 245-246).
- 4.) There is also a striking parallel between the Finnish Kipumäki or Kipuvuori — the "mountain of pains", where the demons of diseases reside — and the Sumerian "harsag gig-ga" (the mountain of the west, but also "mountain of disease"; cf. gig = "night, dark", i.e. "west", but also "sick"), from where, according to the magic texts, the most powerful harmful demons originate. Moreover, the Finns imagined the "mountain of pains" in the same west-northwest direction as the Sumerians: on an island in the northwestern part of the Tuonela River. According to Finnish myth, there are nine holes on the top of this mountain, into which diseases fall back under the influence of magic spells. This image corresponds exactly to a Sumerian magical text, which speaks of the disease being absorbed by the earth (L. Lenormant 1878, p. 249).
- 5.) However, even when viewed in general terms, Finnish exorcism formulas are structured in the same spirit and in a completely similar manner as the corresponding Sumerian exorcism texts: often even the mode of expression is identical. This is a particularly striking and characteristic feature, because at the same time, the spirit, content and mode of expression of the ancient Egyptian exorcism formulas, which were much closer in space and time to Sumerian Mesopotamia, are completely different from those of the Sumerians (L. Lenormant 1878, p. 250). Lenormant cites as evidence on pages 250-251 the 45th song of the Kalevala, which contains truly surprising, detailed serial correspondences with Sumerian rereadings of the same theme.

In the light of our current archaeological knowledge, all these similarities must undoubtedly be viewed in the light of the West Eurasian emanations of ancient Pre-Asian - more specifically Mesopotamian - civilizations, in this case the Volga.
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[Early Bronze Age colonists appearing in the Ural region after 2000 BC
As a result, archaeological data suggests that these settlers have been around for half a millennium.
I arrived en masse in several waves, namely from the south, from the Caucasus region, from the Kuban civilization and the early Transcaucasian culture, in which region, however, in several places, we must also reckon with Sumerian groups who migrated from Mesopotamia with a very high degree of probability (L. pp. 796-799 and 805-807, but also cf. pp. 46-52). The surprising Sumerian-Finnish ethnographic parallels, which have been pointed out by the Lenormant al for more than a century, therefore, according to the evidence of recent excavations, are a concrete ancestral
I are based on real facts, direct relationships that once existed, and in no way can they be considered "coincidental coincidences". This means that the prehistory of the Hungarians — even if we were to remain within the traditional Finno-Ugric framework! — cannot be examined today without taking into account the former Mesopotamian-Sumerian connections, the pre-Asian cultural flow and migrations that extended over half of Eurasia. In other words, any narrowed, self-contemplated Finno-Ugric or Uralic theory of folk and language development is irrefutably outdated and does not meet the current basic requirements of systematic scientific research.

After Lenormant, many others pointed out the surprising ethnographic and mythological parallels that connect the peoples of Western Eurasia. For example, Fvder Velden, in his work already mentioned in our book I (Über Ursprung und Herkunft der indogermanischen Sprachen und anarische Sprachreste in Europa, 1912), emphasizes that the ethnic groups that speak Indo-Germanic languages today originate from the same human race from which the Semites, the Uralts, and the Altaians also originate. This is proven not only by the numerous mutual linguistic borrowings and parallels, but also by the fact that myths, tales, religious images, and superstitious customs appear almost everywhere in the same form (cf. pp. 73-74).

In our country, Gyula László has recently conducted pioneering research in this direction. He has shown the Mesopotamian origin of several fairy tale, mythological and cosmological motifs that were widespread among the Hungarians and the Uralic and Altaic peoples (Art during the Migration Period in Hungary). These include: the twin hunters and the antlered hind, the tree of life and the world tree (világhegy), the cosmic duel, the Ancient Great Mother Goddess (potnia theron), the son of the sun.
In outlining the migration of these Mesopotamian steppe motifs, Gyula László - in agreement with previous literature - generally assumes a Greek or Iranian transmission to the Scythians, or their Asian brothers, the Sakas.
However, in the light of recent research, we can no longer speak unequivocally about such things. We have repeatedly shown so far that Scythian art cannot be considered as a single

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of the adoption of Old Persian or Greek motifs, i.e. Indo-Germanic cultural!), but its formal "hostel" (not least because it was built long before the establishment of the ancient Persian empire) treasure reached the Scythians directly via Urartu and Transcaucasia (L. pl. Zivije treasure find), or from there the Scythians took it with them to Southern Russia, after their stay in Proto-Asia in the 8th-7th centuries BC (cf. pp. 86-89, and 231-233). On the other hand, the Greek art that was in the process of formation at that time also largely drew its tricks of metalworking and the variations in the decoration of metal objects from the same Proto-Asiatic-Transcaucasian metal technology and treasure of motifs (cf. the so-called "orientalizing" period in early Greek art!); This is where the very close parallels between Scythian and Greek metalwork come from. A very good summary of the questions of the real roots of Greek art can be found, for example, in W. Helck's book "Die Beziehungen Ägyptens und Vorderasiens zur Ägäis bis ins 7. Jahrhundert v. Chr." (1979), in which he lists the Urartian, Luristanian, North Syrian (Hurrian), and even partly Cimmerian and early Scythian antecedents of the Greek motif treasure over 34 pages (cf. id.m. pp. 188-222); but also in many other recent relevant works (see, for example, Pjotrowski 1969, Metzger 1969).

Let us now look at the five mentioned motifs of Mesopotamian origin one by one, starting from the derivations of Gyula László, but expanding them where possible with data that prove that the symbolism in question was already known far beyond the borders of Mesopotamia in the times before the Greeks and ancient Persians.
I.) The two hunting brothers and the antlered hind
Gyula László places the origin of the motif of the antlered hind in Lurisztan, during the time of the so-called "Lurisztan bronzes" (ca. 1100-600 BC), and he also points out that the style of these bronzes greatly influenced later Scythian art. At the same time, he considers the Lurisztan bronzes — evidently following R. Ghirshman, whom he also refers to in his bibliography (cf. Ghirshman 1964) — to be Iranian works.

As a general addition to this question, we must mention again the circumstance already described (L. pp. 228-229), that according to the conviction of recent research, which can be said to be unequivocal, the Luristan bronzes represent the art of the 1st century before the appearance of the Indo-Germans in Persia, and are more than likely the remains of the Kassites who were forced out of Mesopotamia around 1160 BC (L. e.g. Porada 1962, pp. 69-82, Strommenger 1962; Altheim 1947-1948, vol. I). As we have seen above, recent excavations have also revealed that the organic antecedents of these bronzes, together with several characteristic motifs, can already be found in the metal art of Luristan from the 3rd millennium BC (cf. this vol., pp. 790-791).

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More specifically, in the case of the Luristani muzzle decorations depicting deer, it is also necessary to point out that the Mesopotamian fallow deer was a sacred animal among the Sumerians as early as the 3rd millennium BC: the fallow deer were bred in the cult groves of the temples, and their milk was offered to the god Ningirsu, for example, in Lagaš (L. Schmöckl 1963, 134-135, p.). This also includes the lion-headed eagle relief (Imdugud or Anzu) found in al-Ubaid near Ur, dating from around 2600 BC, with two deer standing with their backs to each other, as well as the numerous 16 cylinder seals depicting deer, also from the first half of the 3rd millennium.
That is, the cultic role of the deer and the use of the deer motif in art, which was exclusively religious at that time, can be traced back to Sumerian Mesopotamia to times much older than the Luristan Bronze Age. In the middle and second half of the 3rd millennium, the deer motif appeared in several places in Anatolia (Alaca Hüyük, Horoztepe, etc.), accompanied by many other very close Sumerian parallels (similar shapes of metal tools, weapons, vessels, identical techniques of metalwork, close correspondences in burial customs, the arrangement of graves, and grave goods; cf. above, pp. 773-775). From here, not long later, the

It also reached the Caucasus and the Kuban region (Maykop), and then, around the turn of the 2nd/1st millennium, it entered Scythian art, acquiring its closed, unified stylistic features (the so-called AmlaS culture) on the southwestern shores of Lake Caspian (cf. our book I, p. 88).
The straightforward Mesopotamian origin of the deer motif of Scythian and later Hun-Avar-Hungarian art is also proven by the extraordinary example of the artist's respect for tradition, which is found in the Nagyszentmiklós treasure no. 2. can be seen on the jug: the falling deer, according to the definition of the zoologist (Zoltán Kádár), clearly depicts a Mesopotamian fallow deer, "... a specimen of *Dama mesopotamica* presented with great realism" (1. László 1977, p. 181). But we must also include the crowned, bearded human-headed griffin of jug 7. 57, a surprising parallel to which is found on a gold pectorale from the Scythian-Urartian treasure find in Zivlje (cf. e.g. Ghirshman 1964, p. 105-106, plate no. 137). On the tail of the winged lion of the same pectorale, a cosmic zodiac sign can be seen, which in turn is closely paralleled in the Proto-Asian style lion frescoes of Esztergom.

These similarities clearly indicate that among some steppe peoples — in this case the Hungarians and the late Avars — even in the second half of the 1st millennium AD, very vivid and clear mythological traditions of Mesopotamian origin existed, which were also expressed with great precision in the symbolism of art.
In connection with similar Greek representations, the antlerless red deer of the goddess Artemis is definitely worth mentioning. It is worth noting that
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that Apollo and his brother Artemis were not original Greek gods, but rather originated in Greek mythology itself from a foreign land, namely from the north. Apollo traveled to his northern homeland, the so-called Hyperboreans, every autumn and returned to Greece in the spring; Artemis also had her most ancient sanctuary in the north, in the Crimean peninsula, where she abducted Iphigenia, for example.

In this context, Herodotus' statement that the Hyperboreans even in his time sent annual sacrificial gifts to Apollo's sacred island, Delos, is very important; namely, through the mediation of the Scythians, Thracians and the ancient, pre-Greek (!) Dodona is very important. This route undoubtedly indicates - as J. Wiesner states, for example - that this is a very old tradition (see Wiesner 1968, p. 7). But the same is also proven by the fact that the tomb of the so-called "Hyperborean virgins" on Delos belongs to the Cretan domed tomb type from before the Mycenaean period, which is a clear indication of a tradition that can be traced back to at least the beginning of the 2nd millennium BC. The "Hyperboreans" are therefore from the 3rd/2nd millennium BC. Around the turn of the millennium, they still lived in the northern Balkan-Trypolian region, and later moved east and northeast, states Wiesner (1. above).

It seems, therefore, that these Hyperboreans cannot be separated either from the settlers who appeared in the Carpathian Basin in the second half of the 3rd millennium BC, coming from Troy, the Aegean or Transcaucasia, or from the simultaneous north-eastward expansion of the Tripoli culture. Moreover, the Volga-Ural colonists of Transcaucasian-Cuban origin from the first half of the 2nd millennium BC also belong to this group in some way. For a closer look at this, we recall the deer symbolism of the princely tombs of Alaca Hüyük, Horoztepe etc. and Maykop around 2500-2300, which clearly show the path of this Anatolian cultural flow to the Eastern European steppe region. Well, the same deer motif appears not long after, in the first half of the 2nd millennium BC, in Greece: a depiction of a suckling red deer with antlers was found in a 4th-century Mycenaean shaft tomb dated to the 16th century BC (1. Wiesner 1968, p. 8, fig. 5), and from another tomb of a similar age an image of a deer engraved on a seal stamp of a war chariot was found (1. Matz 1958). This, in turn, leads to the conclusion that the bearers of the Mycenaean culture that emerged around the 19th century BC also preserved Anatolian-Transcaucasian traditions, and their appearance in Greece is also connected with the huge Subir-Hurrian migrations that took place around the turn of the 3rd/2nd millennium BC.

All these data clearly prove that the deer symbolism of Mesopotamian-Sumerian origin reached the steppe areas of southern Russia as early as the second half of the 3rd millennium BC, well over a thousand years before the appearance of the Greeks and Iranians, and by the middle of the 2nd millennium BC it had reached Mycenae. This, ultimately, is of Proto-Asian origin.
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Soviet archaeology has found an almost unbroken survival of Kullik traditions in the recently excavated Scythian settlements of southern Russia: numerous analogues of the ancient Trypillian traditions have been identified (cf. e.g. Talbot-Rice 1957; Wiesner 1968; Charitire 1974; Grakow 1978; Rolle 1980).
2.) The Tree of Life and the World Mountain
With detailed comparative ethnographic studies, Gyula László traces the occurrences of the tree of life motif back to Han-era China and the North Germanic Yggdrasil world tree, i.e. to roughly the centuries around AD. However, if we extend the research into the history of the motif to earlier times, it soon becomes clear that the tree of life is also of Mesopotamian origin: both depicted on its own and with animals facing left and right. Tree of life depictions begin as early as the second half of the 4th millennium (late Uruk period and Jemdet Nasr period), and they continue to constitute one of the leading motifs in the glyptic, relief and painting of the Near East.

However, this question also includes perhaps the most characteristic manifestations of Sumerian civilization, the stepped tower temples (ziggurats), which, according to most experts, are cultic copies of the mythical "world mountain" (in Sumerian: "barsag kalama"), and with their seven floors symbolize the seven spheres of heaven. Similarly, the tree of life or world tree spread throughout Eurasia also has seven (sometimes nine) branches or levels, and the world mountain of the Altai peoples also has seven (sometimes nine) floors. The Mesopotamian influence is therefore evident in this case as well. As e.g. G.Rünk also notes that the idea of a "world tree" or "world pillar" of the steppe and Arctic peoples "... undoubtedly can be traced back to ancient pre-Asian ideas" (1. Rank 1968, p. 43). The monumental Sumerian-type ziggurats from the end of the 3rd millennium BC (Altin Tepe, Turang Tepe) discovered during recent excavations in southern Turkmenistan and northern Iran show exactly the path by which these cosmological ideas reached the Eurasian steppe and even further north.

3.) The Cosmic Duel
Gyula László also showed that the frescoes of St. Ladislaus in our medieval churches actually bring to life an ancient, oriental mythological story: the struggle between light and darkness, the hero of which, the champion of light, was identified with St. Ladislaus after the adoption of Christianity. This motif goes back to an ancient common mythological basis, which we can first grasp in the cosmic battle between Ahura Mazda and Ahriman.

These data can also be supplemented by the fact that both the cosmic duel and its symbolic representation, the animal fight, can be traced back to the Sumerian seal cylinders of the 4th-3rd millennium BC, and from that time onwards they continued to play a leading role in the pre-Asian
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in glyptic art, especially in Hurrian-Urartian art, which is known to have formed the basis of Scythian art.
In literary tradition, Gilgamesh's unarmed duel with Enkidu is the first ritual combat we know of. Then Gilgamesh and Enkidu's fight with the demonic Humbaba and the celestial bull are the first cosmic duels, the latter of which has an astonishingly accurate late Hungarian parallel in the fight of the Talots with the black bull flying in the clouds, when they themselves often take the form of a bull. Compare this mythological element with the animal fights seen on numerous early Sumerian seal cylinders, in which bulls with partly animal and partly human heads fight each other.

In connection with the aforementioned cosmic battle between Ahura Mazda and Ahriman, we are reminded again of the new realization that the dualistic religion of Zarathustra developed in Eastern Iran, in the region of Sogdia and Bactria, in an environment where, in the period in question — the middle of the first millennium BC — the majority of the population was not Iranian (l. pp. 211-212 and 236).

Regarding the invulnerability of the heroes of the cosmic duel, Gyula László mentions Jason, Achilles, Siegfried, Wieland, and the Persian Isfandar. As an ancestor and prototype of all of these, we can also include Gilgamesh, who, however, differs from the others in one essential respect: in his fight against Humbaba (or Huvava), he was not made invulnerable by some kind of occult magic power or magic ointment — like the sacrifices and prayers of the aforementioned Indo-Germanic heroes (1. Rákos-Komoróczy 1974, p. 111).

4.) The Great Mother Goddess
Gyula László sees the prototype of Tabiti, the great mother goddess of the Scythians — who, according to the Scythian tradition preserved by Herodotus and Diodorus Siculus, was the chief goddess of the Scythians and at the same time the progenitor of the royal tribe — in the figure of the Greek "Potnia theron" (Magna Mater) and in the anthropo-zoomorphic renderings of certain Luristani bronzes, the latter of which he interprets — again evidently under the influence of Ghirshman — as representations of the Indo-Iranian deities Sraosha and Ashi.
We have already mentioned that modern research does not consider the Luristan bronzes to be Persian or Indo-Iranian in general (1. 832, p., but also cf. pp. 86-88). In the place cited in our Book I, we listed the relevant literature.
26 So now we only need to supplement what was said there by saying that the Luristan mountain tribes lived in complete independence throughout the existence of the ancient Persian empire. For example, when the Persian king traveled from Babylon to Persepolis, he and his entourage had to pay a regular tax to the Luristans, who controlled the passes of the Zagros. Alexander the Great had to force them to pay homage in a separate campaign.
- Thus, even at the end of the 4th century BC, we cannot count Iranian-836

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with them, let alone at the end of the 2nd millennium or the beginning of the 1st, when the Luristan bronzes originate.
On the other hand, we also recall the recent research finding that the Scythian "Magna Mater" appeared in southern Russia as early as the Bronze Age, and its depictions are strikingly similar to the somewhat earlier Mesopotamian and Elamite goddess statues (cf. Talbot-Ryce 1957, ally our book I, p. 82, but also 1. Charrière 1974, p. 83).
Also according to G. Charrière, the prototype of the great goddess of the Scythians was probably the Sumerian Imdugud (Anzu), the lion-headed eagle with deer (1. Charnière. 1974, p. 85).
Considering that the Imdugud motif appears in the Banat region as early as the 68-107608 V1y culture, around the turn of the 4/3rd millennium (find in Timisoara; 1. 743. p.),
Charrière's above derivation of the motif seems very convincing indeed.
5.) Son of the Sun
The title of the ancient rulers, "son of the sun", is closely connected with the myth of the cosmic duel, the battle between light and darkness. The "son of the sun", sent from heaven, confronts the harmful powers of darkness to protect his subjects.

The roots of this belief can also be found in the ancient Near East. "Kingdom descended from heaven," write the Sumerian king lists. But Gilgamesh, the first two-thirds of the cosmic duels that can be dated, was of divine origin and under the special protection of the sun god. The Egyptian pharaoh was a deity descended directly to earth, indeed the son of the sun hero (the god Ra).

Let us now return to the Hungarian-Finno-Ugric ethnographic parallels in the narrower sense. The summary opinion of international scholarship on these ethnographic and mythological questions can be found in a well-organized form in the relevant volume of the "Handbuch der Kulturgeschichte" (Wiesner et alii; Die Kulturen der eurasischen Völker, 1968.).

Let's first look at what Gy. Farkas and T. Bogvay write in the chapter on the Hungarians (Die Kultur der Ungarn). Although the authors do not question the opinion of Finno-Ugric linguistics about the Uralic-Finno-Ugric origin of our language, they emphasize that the Hungarians have absorbed so much Turkish blood over time that, according to modern human research, the strongest component of the Magyar language at the time of the conquest was of Turkish origin (L. Farkas-Bogvay 1968, p. 8). Therefore, even if we look in vain for parallels in the Finno-Ugric religion, ideas, lifestyle or traditions among the conquering Hungarians, the sources of consciousness or the recent ethnographic research have revealed that it is the same as the religious ideas of the peoples of North Eurasia and Central Asia (L. iott, p. 9). —, because everything that is said about them

From the two quotes we can see that the authors, despite accepting the thesis of our Finno-Ugric origin, are extremely cautious in their wording and do not want to 837

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It is an effort to find Finno-Ugric parallels everywhere, an effort that is evident in our relevant domestic literature. In the chapter on the Finns (R. Dencker: Die Kultur Finnlands), we can also read several very instructive things that are rarely mentioned in Hungarian linguistic and pre-historical works. For example, the Finns only appeared in the southern regions of present-day Finland around the 1st century AD, 5 mixing with the indigenous population of combed pottery who had been living there for thousands of years. However, this indigenous population using combed pottery cannot be identified with the Lapps, because the latter only appeared in the central and northern regions of Finland at the end of the Bronze Age, around 500 BC, just a few centuries before the Finns, arriving from the east, and nomadic in Northern Finland for centuries, until they finally adopted their present-day Finno-Ugric language. On the other hand, it cannot be proven that the comb-ware indigenous population were Finno-Ugric (L. Dencker 1968, p. 6).

For us, the late appearance of the Finns in their present-day territory is a particularly important fact. This circumstance strongly supports our conviction that the so-called Finno-Ugric language unit cannot be traced back to the 3rd millennium BC (cf. e.g. our book IH, pp. 510-513), because in this case the Finns would have needed more than 2000 years to reach Finland from the Upper Volga region. The same is proven by the results of anthropological studies: the anthropological consolidation of the Finno-Ugric peoples can be dated well after the turn of the 3rd/2nd millennium, and even probably only to the 1st millennium BC, during the time of the Ananjin civilization (cf. e.g. Jetmar et alii 1966; Lipták 1977).

We also learn from Dencker that the Finnish language is very conservative and has hardly changed throughout its history (ibid., p. 5). This indicates — and Hungarian linguistic monuments also testify — that sound laws and regular sound changes did not play such a large role in the development of Finno-Ugric languages as, for example, in Germanic. It follows from this, however, that the exclusive application of sound laws in Finno-Ugric language comparison cannot lead to reliable results; -- not even if we assume their validity in the development of languages in general.

The Finns came under very strong North Germanic influences during the first millennium AD — continues Dencker, They adopted, for example, the custom of ship burials from the Vikings (L. id. m. p. 7). -- In this connection, we recall the boat-shaped headstones of the Upper Tisza region, which, according to the Finno-Ugric perspective, are the oldest known (see our book II, . They preserve the tradition of an "ancient Ugric" burial custom (cf. pp. 369-370). However, it is indisputable from this data that this custom reached the Finno-Ugric peoples from the northern Germans, and not even in "ancient" times, but only in the middle of the 1st millennium AD.

838
===== PAGE 828 =====

[The most instructive chapter of the volume for us is the study by G. Rank (Völker und Kulturen Nordeurasiens). Immediately after the introductory climatic and geographical overview Rank begins the substantive part of his treatise with the following lines. "The ancestral relationship of both the Lapps and the Ugrians with the other Finno-Ugric peoples living in Eastern Europe and Hungary is uncertain, above all in their racial characteristics they are strikingly different from all the others. The only sign of kinship is language, but this is not sufficient to give a sufficient understanding of the ethnic history of the peoples in question." to illuminate; the linguistic affinity in this case is not necessarily a sign of common origin, but can also be deduced from the long coexistence of these peoples. In the languages of both the Lapps and the Ugric peoples, many foreign, linguistically undefined basic stratum elements are found; thus, it can be assumed that these peoples came into contact with each other in the high north, and in the course of time they mixed with ethnic elements about which we still know nothing today." (L. Ránk 1968, p. 5).

He then continues: "In particular, with regard to the Ugrians, some Russian researchers and W. Steinitz have pointed out the possibility that these peoples originated from two ethnic components: a Paleo-Siberian core population and later nomadic elements invading from the Eastern European steppe." (L. id. m. p. 6) — We can read very little about all this in the Hungarian scientific literature.

No less important are the data — also carefully suppressed by the Finno-Ugric school — that Rank lists about the Samoyeds and the various ethnic groups living in the Altai-Sayan-Yenisei region, collectively called "Paleosibirids." Regarding the Samoyeds, he states that although the current school of Finno-Ugric linguistics (sic!) considers it probable that they once lived in northeastern Europe, in the immediate vicinity of the Finno-Ugric peoples, this hypothesis is contradicted by the fact that the Samoyed-populated areas of Western Siberia were much more extensive in the past than today, and extended eastward to the Sayan Mountains. In the middle of the 19th century, Castrén found a number of ethnic groups in the Sayan region who also understood Samoyed, but their common language was Mar Turkic (Kamass, Koybals, Motors, etc.). Furthermore, it is known that the Samoyedic linguistic base is also found in the language of the Aaragass (Tofars) and Soyots of the Sayan region. It seems, therefore, that here we are faced with the last remnants of a once larger Samoyedic ethnic group that was at home in the Sayan region (L. Rank 1968, p. 6).

This former Samoyed group is now separated from the northern Samoyeds by the Yenisei (Kerck) people, Ránk continues. They were formerly classified as Palca-Asians, GJ However, Finnish researchers Ramstedt and K.Donner have shown that the Ket language belongs to the Indochinese languages. In the last century, in their vicinity along the Yenisei River, 10150

839
===== PAGE 829 =====

There were even more ethnic groups speaking languages similar to these two — i.e. of Indochinese origin — (e.g. the Aryans, Assans, Kotts), but today they have completely lost their languages. The population of this region is made up of extremely heterogeneous ethnic elements: Samoyed, Indochinese, Paleosiberian-Europid and Turko-Mongol groups (L. Ránk 1968, pp. 6-7). The presence of Indochinese-origin Chitic basal elements, which at first glance seems mysterious, becomes understandable if we consider that Southwestern Siberia had been the scene of repeated invasions from East and Southeast Asia since 1200 BC - starting with the appearance of the narrow-faced Mongoloid population of the Karasuk culture. The Europid human type (the so-called Andronovo type; - our note) living in Southwestern Siberia in the Neolithic and Early Bronze Age was eventually displaced from Asia by these successive eastern influxes (L. Ránk 1968, p. 7). Ránk later emphatically points out that these "Europids" should not be considered Europeans in the modern sense, because at that time this type of people also dominated south and southwest of Siberia (L. id. m. p. 9).

In this regard, we recall what was said in Chapter I of our Volume I (Book I, pp. 30-31 and 49-50) about the ethnic character of the population of the Afanasyevo, Pit Grave, and then Andronovo cultures, and that, according to the results of recent archaeological excavations, all these cultures were formed by agricultural colonists of ultimately pre-Asian origin who migrated from the area of the Early Bronze Age cultures of southern Turkmenistan and Transcaucasia (cf. Book II, p. 317 and also p. 801 of this book). In the following, Ránk deals with the question of the ancient cultural center in Southeast Asia, which has been emerging more and more clearly in recent times. The signs indicate — he writes — that in addition to the pre-Asian cultural center, we must in all probability also count on a second early cultural center in Southeast Asia, the creators and bearers of which were the Mongoloids. This type of man already very early spread northward, into the Arctic region of Eastern Siberia, and from there spread in a wide arc along the Arctic Ocean to the Northern European region (Lapland) on the one hand, and to America on the other. This is evidenced by the predominantly Mongoloid population of the aforementioned areas, but also by the largely uniform appearance of the cultural forms of the Arctic region, which in no way can be explained solely by similar ecological conditions or neighboring contacts. These facts led to the discovery of the independent, distinctive Arctic cultural sphere, which encompasses the entire Arctic region, the Arctic coasts of Europe, Asia and America (L. Ránk 1968, pp. 10-12).

Rank then discusses the characteristic features of this Arctic cultural circle. He emphasizes, for example, that the bear cult was widespread throughout the entire Arctic region; in addition to the Ob-Ugric peoples, it is also found among the Ainu, the Giljaks, and even among the American Arctic peoples. Therefore, totemism cannot be equated with modern-day 840

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in to be connected to a certain ethnic group (1st edition, p. 38-39). He also emphasizes that only a few of the Arctic peoples can be found with real concepts of celestial gods - e.g. the Ob-Ugric and the Jurach-Samoyeds -, and even then only incidentally. This suggests that these concepts came to them from their southern steppe neighbors. The concept of celestial deity, the distinction between good and evil, first came to the steppe from ancient Pre-Asian cultures, and then from there - but only in fragments to the few Arctic groups mentioned (see Ránk 1968, p. 42-43).

The listed linguistic, ethnographic and anthropological data all again testify that the Samoyeds, the Ob-Ugric and the Lapps do not belong to the Finnish peoples ethnically, but rather changed languages at some point, and thus became part of the so-called Uralic or Finno-Ugric language family (cf. Chapter 3 of our Volume III, especially what is said on pages 510-513). However, we cannot forget that in recent times complex research methods have been gaining ground in Hungarian ethnographic research. In contrast to the exclusively Finno-Ugric perspective of linguistics and prehistory and its research horizon limited to the northern Finno-Ugric areas, ethnographic scholars no longer consider every faint possibility or feigned, forced parallel as proof of our "Finno-Ugric origin" - although this was a general phenomenon in ethnography until recently. This new, critical and at the same time, complex ethnographic approach, working with broad Eurasian parallels in mind, is very well demonstrated by Vilmos Voigt method" in the last 100

In the introduction, Voigt mentions some typical examples of the hasty, one-sided conclusions (of course, in all cases, in the direction of "Finno-Ugric") that were common in ethnographic research. Then, regarding the results of the research on Hungarian folklore before the conquest, he writes as follows: Having reached the end of our methodological review, it hardly seems hasty to state that the questions are very complex, and that the discipline of ethnography is not uniform in its assessment. It is therefore no coincidence that so many of the relevant statements in historical, literary, art and music history handbooks are wrong, false and one-sided. We must only warn everyone to be extremely cautious regarding the introductory ('historical', 'poetic', 'pre-conquest art') chapters of summary works written outside the discipline of ethnography. At best, here, "ethnographic" results that have been outdated for decades and were only valid once are readable. It is even worse if we find uncritical adoption of untested, debatable theories here. We should avoid these." (1. Magy. Östör. talumm. p. 310-311)

Ethnography also includes the question of the parallels and connections between our musical traditions. This is a topic of primary importance, since music, ritual, and

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mus, singing is like a second mother tongue. A few years ago, László Vikár briefly summarized the results of comparative Hungarian folk music research to date in his essay "Volga Turkish and Finno-Ugric Relations of Hungarian Folk Music" (Hungarian Ancient History Studies). Vikár's field research clearly shows that the parallels of our folk music are not found among the Finno-Ugric peoples, but among the Turks: "What lessons can we extract from the material we have learned so far? First of all, that what is related to Hungarian folk music in the contemporary oral tradition of the Volga Turkic and Finno-Ugric peoples is mostly characteristic of the musical culture of the Turkic peoples" — states Vikár (1. Magy. ystört. tanulm. p. 249). Then he writes below: he also found a convincing connection with Hungarian lament melodies in published Ob-Ugric material. György Bence Szabolcsi and Lajos Vargyas are the few Szomjas-Schiffert collected Hungarian parallels to the melodies of the Baltic Finns. Walking among the Chermis, Mordvins and Votyaks, we also attempted to uncover the Finno-Ugric layer, and succeeded in preserving extremely archaic types, moving in two-three-four tones. However, we do not know the Hungarian equivalents of these. The question arises: can we even talk about Finno-Ugric music? Was there once a common musical language, which then dispersed, and the present-day Finno-Ugric peoples preserved something of it. This is difficult to imagine. We should rather put it this way: just as there is no Finno-Ugric basic language today, it would be an equally futile attempt to force the present-day music of the Finno-Ugric peoples into some kind of reconstructed unity. There is no Finno-Ugric language, there are only different Finno-Ugric languages, and they are more or less related. There is no specifically Finno-Ugric music, but every Finno-Ugric-speaking people has its own musical tradition, and these musics are just as diverse, on the one hand similar and on the other different, as are the languages." (1. Magy. yy- tört. talumm. pp. 296-297.).

There is really not much to add to these lines. They show unequivocally that the thesis of a common ancient Finno-Ugric basic language is clearly refuted by the great differences in the musical "second mother tongue". And according to the data at our disposal, not only in the Hungarian-Finno-Ugric, but also in the relationship between the individual Finno-Ugric peoples. This becomes especially clear if we examine Vikár's further statements, in which he - albeit unspokenly, but all the more convincingly - dissects the real evidentiary value of the Hungarian-Finno-Ugric Zenci parallels found by Szabolcsi, Vargyas and Szomjas-Schiffert, mentioned above; When examining the Turkish and Finno-Ugric connections of Hungarian folk music, we must not lose sight of the fact that the pentatonic scale, the descending melodic structure, and within this the change of fifths are all much more characteristic of the Hungarians than the few northern Finno-Ugric peoples.

842
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music consisting of three, four or five notes, endlessly repeating a motif, it is much easier to define and find Turkish musical elements than to say what is truly typical in the Finno-Ugric musical tradition, We find a copy of something that is composed of few data sooner than of something that is characteristic and cannot be confused with anything else, Melodic motifs moving on three, four or five notes next to each other are also known from many other parts of the world, they could have arisen independently of each other, because they are simple and natural manifestations of the human soul. They cannot be appropriated for any culture or language family." (1. Magy. Östör. talumm. p. 298) In other words, the Obi-Ugric and Baltic Finnish parallels found so far prove nothing in the world, and further research does not promise results, because the original ancient music of the northern Finno-Ugric peoples is so primitive that parallels can be found among primitive natural peoples almost all over the world. Here we must also mention the characteristic fate of Béla Bartók's 1936 collection of southeastern Anatolian folk songs, which, while not fitting into Finno-Ugric theory, is strikingly reminiscent of the attitude of Finno-Ugric "scholarship" experienced around the Tarih-i Ungürlüz (cf. pp. 628-629). At that time, Bartók collected more than 90 ancient Turkish melodies accompanied by the Turkish A. Saygun. A preliminary review of it was published in the 2nd, 4th and 5th issues of "Nyugat" in 1937 under the title "Collection of Folk Songs in Turkey". In this work, he talks about the Hungarian equivalents of twenty melodies (cf. e.g. Ferenc Kunszabó, Reflections on a Prolonged Misstep; Forrás, November 18, 1981, p. 18). — After that, Bartók's insights were shrouded in profound silence for almost 40 years, until in 1976, Bartók's former accompanist, Saygun, published Bartók's definitive evaluation of his collecting journey, supplemented by Saygun's own analyses and his correspondence with Bartók. The history of the publication: Saygun examined the papers left behind by Bartók in America, and among them he found notes on the results of the collecting trip to Turkey. Thus, the publication can obviously be traced back primarily to Saygun's initiative and encouragement. This is also confirmed by the fact that - characteristically - the book was published only in English, and copies of it were not even sold in Hungary. - All of this information comes from Dr. Miklós Erdy's American radio lecture series "My Inner Asian Journey to the Homeland of the New Gurs, the Sinkiang Province of China", the text of which is available to us from the friendly kindness of the Author,

Dr. Érdy then quotes Bartók's statements about the parallels between Turkish and Hungarian melodies verbatim (translated into Hungarian from the English text of the aforementioned academic publication). Bartók divided the collected melodies into several groups. Class I included the four-line melodies with 8 syllables per line.

843
===== PAGE 833 =====

songs containing 4-line melodies, and in class II. melodies also containing 11 syllables. Of the 87 Turkish folk songs suitable for analysis, 20% belong to class I and 23% to class II, a total of 43 and 37 songs respectively (1. Saygun 1976, pp. V and IX.). In his summary (on page XXXIV.) Bartók writes the following about these two groups: "The oldest, most characteristic and most homogeneous part of the material, which is 43% of it, consists of isometric 4-line melodies, with lines of 8 or 11 syllables... The part of this material that has 8 syllables is identical to the old Hungarian 8-syllable material. The 11-syllable part is closely related to the old Hungarian material." In light of this, what should we say about the procedure by which Gábor Bereczki, head of the Finno-Ugric Department in Budapest, tries to refute the correctness of Ferenc Kunszabó's aforementioned reference that Bartók found twenty Hungarian-Turkish melodic correspondences at the time: "He is talking about melody." (I. Kortárs, June 1982, p. 993) — Dr. Érdy quite rightly According to my friend László Vikár... Bartók only had three remarks in his radio presentation: who is responsible for this, Vikár or Bereczki? — Remembering the lines of László Vikár quoted above, it seems Is this a deception or a lie? And if it is, who that we can hardly hold him responsible for this mystification. To summarize what has been said so far, we cannot find any concrete data in ethnographic terms that would point — even indirectly — towards the supposed Uralic or Finno-Ugric ancestral people and ancestral language. On the contrary, the complex ethnographic research carried out so far all, without exception, testify that the "Uralic", "Finno-Ugric" and "Ugric" ancestral peoples and basic languages of Finno-Ugric never existed in reality. Let us think of the completely different anthropological character of the Samoyeds and Ob-Ugrics, and the demonstrably non-different nature of their languages, which all connect them with the peoples of the Arctic Circle, from Norway through the Behring Strait to the Canadian their basic strata of "Uralic" or "Finno-Ugric" origin, and their cultural and cultic traditions are sharply different from those of other Finnish peoples. Arctic ethnicities. So the Baltic, Volga and Permian groups of Finno-Ugric in the narrower sense remain; for these - within certain limits - we can still speak of some kind of ancient language community, but not for the others.

Finally, we emphasize that the higher-order mythological-religious concepts (heaven) that are found in a rudimentary form among the Ob-Ugric peoples - in comparison with the relatively closer Hungarian linguistic affinity of this ethnic group - again only testify to what we have already outlined in more detail in our book YL: namely, that the Proto-Hungarian linguistic and cultural influences were much stronger among them than among the other northern peoples, 8 and that contacts between them and the Proto-Hungarians occurred repeatedly and repeatedly,

844
===== PAGE 834 =====

Chapter 5
THE LINGUISTIC IMPACTS OF CENTRAL ASIAN COLONIALISM IN WESTERN EURASIA
Having become acquainted with the vast migrations that once started from the area of Proto-Asian civilizations, their causes, and the main lines of their cultural, anthropological, and ethnographic consequences, let us briefly and selectively review the paleolinguistic consequences of these colonizations, which can be concretely grasped in source data and linguistic monuments, in the older and newer languages of Europe and Western Asia. Naturally, within the narrow framework at our disposal, we cannot provide a detailed description of these complex issues, the closer examination of which has not even begun: the research on some of the detailed topics that have been touched upon here and there by international linguistics has not even progressed beyond its infancy. .

Until now, European scholarship has mostly dealt with the basic linguistic influences of the so-called Anatolian-Aegean-Tyrrhenian ancient language community, and this is only because, in the course of examining the linguistic material of Greek and Latin that cannot be explained from Indo-Germanic, which amounts to a very considerable amount in the case of both classical languages (for example, almost half of the vocabulary of Ancient Greek cannot be interpreted from Indo-Germanic languages; cf. the statement of A. Meillet, described in our book I, p. 74), it has inevitably or unwillingly begun to consider these pre-Indo-Germanic languages: it has become clear that parallels to these languages can still be found in large numbers in the languages of the Mediterranean areas that were gradually Indo-Germanized much later.

In accordance with this situation, it seems appropriate that we ourselves begin our review in the more or less satisfactorily researched area of the Anatolian-Aegean-Tyrrhenian proto-language group. In this way, we can start from the already fairly well-known ancient, pre-Indo-Germanic linguistic connections, and on these data, which are also accepted by international linguistics, we can build our further, broader paleolinguistic investigations. On the other hand, from the lessons gained through observing the nature of the diverse connections that appear between the basic linguistic material of this language community and the later developed South European Indo-Germanic languages, we can draw well-founded conclusions regarding the nature of the processes of stemming, language leveling and superstratification that took place between other ancient language groups. F. Schacher provides a brief, easily understandable summary of the results of research on the Anatolian-Aegean-Tyrrhenian language community since 1961.

845
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meyr, "Die Minoische Kultur des alten Kreta" (1979). Below, we will describe the current state of the question mainly based on this work.

Researchers of the Greek language and prehistory noticed as early as the 19th century that in the vast area stretching from Armenia and Eastern Anatolia through the Aegean islands, Greece and the Balkans to the middle Danube region and southern Italy, there are a large number of place and other geographical names whose roots and endings are not Indo-Germanic. The most frequently occurring such pre-Indo-Germanic endings or stem extensions are the following: -nth-, ant-, -nd-, —ss-, —s-, —mn-, —r-, -M-, —n-, -ý-, and -ý- (cf. e.g. Korinthus, Lakraunda, Halikarnassos, Kephisos, Methymna, Atabyron, Pergamos, Athenai, Kydrellos, Miletos).

The excellent P. Kretschmer recognized as early as 1896 (Introduction to the History of the Greek Language) that we are dealing here with the remains of an ancient, pre-Greek Asia Minor-Aegean-Mediterranean language community (1. Schachermeyr 1979, p. 231). Several people have attempted to distribute these endings among some of the ancient languages they assumed, but these efforts have not led to any results, because it can be clearly demonstrated that, on the one hand, the geographical distribution of the individual suffixes and formations essentially overlaps, and on the other hand, these roots and endings are organically linked linguistically by the fact that we occasionally find a root word with a different suffix or formation in names. For example, the derivatives of the root lar- are Laranda, Larynthios, La-rissa, Larasa, Larymna, Larymon, Larine; or those of the root sam- are Samikon, Saminthos, Samassis, Samylia. The stems and endings in question are thus so intertwined that their separation is impossible (1. Schachermeyr 1979, pp. 231—233).

In the first half of our century, the pre-Indo-Germanic linguistic strata of the western Mediterranean were also researched in more detail (e.g. A. Trombetti, F. Ribezzo, P. Kretschmer and others), and it turned out that the ancient languages of Italy and the Iberian Peninsula also had very close connections to the Aegean-Eastern Mediterranean language community. For example, Trombetti recognized the Caucasian connections of the Basque language, and Ribezzo revealed the Aegean-Asia Minor proto-linguistic connections of the Italian stratum, which he called the "Tyrrhenian language community". These studies therefore revealed that the ancient languages of the western Mediterranean also belonged to the pre-Indo-Germanic Asia Minor-Mediterranean language community (1. Pallottino 1980, 192-193, pp.).

Schachermeyr linked the widespread distribution of this nomenclature to the East Asian cultural flow in the 1950s, during which the 1011076168 and the settled lifestyle from the Middle East reached the Aegean region, the Balkans, the Danube region and southern Italy. However, the fact that the name did not yet fit into this picture at the time did not fit in.

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% geographical names formed from roots and endings, although less frequently,
* but further west, as far as SE France and Spain, they can be found. At that time, we did not yet know about the small islands of the Central Asian cultural heritage. In recent years, however, it has been shown that, for example, the so-called "nail-marked pottery" from Syria and Cilicia (a variant of the already known impression pottery; 1. pp. 668, 669, and 716) was widespread in Thessaly, the Balkans, and Sicily, as well as in almost all of Italy and southern France. Furthermore, it has been established that the Iberian Peninsula was much more heavily influenced by Proto-Asian and Aegean influences during the late Neolithic and early Bronze Age than previously thought (1. Schachermeyr 1979, pp. 233-234). - This latter remark of Schachermeyr applies to the numerous settlements of the megalithic and bell-shaped ware cultures in Spain and Portugal, which we already know were the creations of Proto-Asian ore-finding and metalworking colonists (1. pp. 780-781 and 809-810).

Since the aforementioned characteristic geographical names were first noticed by linguistics in the Aegean region during the research of the Greek language, international research generally calls this proto-linguistic community "Aegean". Schachermeyr, although he sticks to this traditional terminology, clearly recognizes the relative legitimacy of the term "Aegean" and tries to justify the geographical precision of the of this language or language group can primarily be distribution zone of these names by saying that the Aegean region constitutes the center of it. However, he immediately emphasizes that the origin attributed to the central and eastern regions of Anatolia and Cilicia, i.e. we should actually speak of an Anatolian or East Anatolian language community (1. Schachermeyr 1979, p. 235). Very important in this respect are also the maps of Schacher-meyr, on which he shows the occurrences of some of these "Aegean" geographical names from Eastern Anatolia to Austria and Italy. The comparison only includes names ending in —nt(h)- and —s(s)-, but their number still amounts to several hundred. At the same time, however, the maps also clearly show that the origin of these geographical names should actually be sought in Anatolia: well over 50% of them are concentrated in Asia Minor (1. Schachermeyr 1979, pp. 230-234, fig. 122-126).

Schachermeyr further explains that this "Aegean" — but actually Anatolian — language group was more closely related to the Caucasian languages in the east, and in ancient times these linguistic relationships apparently extended as far as Media and Elam, and in the north there are connections and similarities with the Indo-Germanic languages, especially in the field of word roots (1. Schachermeyr 1979, p. 235). Until very recently, many people flatly denied that this ancient language community originated in Anatolia, since they were not developed in the Anatolian Neolithic.

847
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and our data testifying to the age of the beggar. However, the excavations carried out in the meantime in Hacilar, Catal Hüyük, Can Hasan have proven that there were highly developed civilizations in Anatolia already in the 5th millennium, the expansion of which can clearly explain the appearance of the Aegean nomenclature in large areas (1).
Schachermeyr 1979, p. 235). - We must note that Schachermeyr is mistaken here by millennia, because at Catal Hüyük we find a very advanced Neolithic as early as the 7th millennium, and at Hacilar and Can Hasan we find a Chalcolithic culture as early as the 6th millennium (1. of this book, p. 669).
In Anatolia itself, the first tangible member of the Anatolian-Aegean language group appears in the Central Anatolian Hattic language, which can be dated from the 3rd millennium BC, and which then formed one of the linguistic bases of the Hittite (Nesi), Palaian and Luwian languages, which already contained significant Indo-Germanic elements, in the 2nd millennium BC (1. Schachermeyr 1979, p. 236).
At the same time, however, these three, partly Indo-Germanized Anatolian languages also show very strong Hurrian and Sumerian influences, in the form of massive loanwords and numerous grammatical features. In Western Anatolia, the Lycian, Karic and Lydian languages represent the more developed descendants of this ancient language community in the 1st millennium BC.
In the Aegean region, the Cretan language of the Minoan culture was the most important member of this language group (1. Schachermeyr 1979, p. 236), but the language of the Pelasgians before the Greeks of Herodotus also belonged to it. The lingua franca of the Mycenaean culture in the second half of the 2nd millennium BC was largely based on these two linguistic bases, and then, starting from the end of the 2nd millennium BC, the gradually Indo-Germanized Ancient Greek language. — We will say a few words about the alleged Ancient Greek language of the Mycenaean civilization (Ventris's decipherment of the so-called Linear B tablets) later.
Further west, the Etruscan language of Central Italy was the longest-surviving and best-known representative of the Anatolian-Aegean-Tyrrhenian language community, to which Ligurian and Raetian were also related (see Schachermeyr 1979, p. 236). Even further away, the Iberian language was also closely related to this language group, and here, in the far west, we find the clearest evidence of the high degree of linguistic equalization that once existed between the individual languages of the ancient Anatolian-Mediterranean language community: one of the descendants of the Iberian language that is still alive today, Basque, even after thousands of years, shows close parallels of several orders of magnitude with the Caucasian languages and even with Sumerian (1. Bouda 1938), i.e. with languages that did not belong directly to the actual Anatolian-Aegean language family, but were only related to it in the east,
Paleolinguistic research on the Anatolian-Aegean-Tyrrhenian language group has therefore led to the conclusion that a Central and Eastern dialect was widespread in the Mediterranean in the 5th-3rd millennium BC.

848
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An Areal language alliance-like lingua franca originating from Anatólidbdl, which was understood more or less by the Neolithic, Chalcolithic and Bronze Age population of this vast region, who were predominantly of Pre-Asian origin, and who named their settlements and other features of the geographical environment (mountains, rivers, lakes, etc.) in the newly colonized areas with the words of this language. At the same time, we have also seen that this ancient language community was in close linguistic relations with the Pre-Asian areas that connected further east, especially with the settlements of the Subarctic and Hurrians in northern Mesopotamia and Transcaucasia, as well as with the regions of western Iran, where at that time we find the Gutian, Lullubian, Kassian and Elamite languages.

On this question, we must definitely remember the German Semitologist F. Hommel, who had an excellent synthetic sense, who in his work "Verwandschaftsverhältnisse" in 1884, through a systematic comparison of linguistic relics that were still rather incompletely known at that time, clearly recognized these ancient linguistic connections. In fact, much more. At the end of his aforementioned book, he emphatically emphasizes that the Caucasian languages are related to the Proto-Elamite, Hurrian and Urartian languages. Kassu also seems to be a language of the Caucasian-Elamite type. All signs indicate, he continues, that to the north and east of Mesopotamia, from Cilicia to Elam, we find different but related languages of the same ancient people group. This so-called "Alarodic" (Caucasian) ýýý- or ýýý- or ýýý-language group was, however, at the same time related to Sumerian, also, and even Basque, Etruscan and Ligurian are in all probability of Caucasian origin. Based on the above - concludes Hommel - a more or less uniform paleolinguistic picture unfolds before our eyes in the 5th-4th-3rd-2nd millennium BC. It seems that ethnic groups speaking various dialects of this proto-Asian language community populated the great Eurasian steppe, Mesopotamia, Iran, Southern and Central Europe up to the Pyrenees.

We have seen earlier that the studies of the former distribution and eastern connections of the Anatolian-Aegean language group led to results identical to Hommel's former insights in very important points in the linguistic field. In archaeological terms, however, recent excavations have confirmed Hommel's insights in every respect (cf. Chapter 2 of this book).

The later language mixing, superstratifications, and the process of the development of new types of languages can be observed relatively well through the example of language 620108, since its vocabulary is sufficiently known from historical times. Nowadays, it is a settled fact that Greek was massively influenced by words from the ancient Anatolian-Aegean language community; in the short linguistic summary of Schachermeyr's cited work, which is barely seven pages long, he lists - as an example only - more than 40 Greek words that are proven to be of Cretan-Aegean origin.

849
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(1. Schachermeyr 1979, pp. 236-238). If we take into account that almost 50% of the many thousands of known words of the ancient Greek language cannot be interpreted from the Indo-Germanic languages (1. above, p. 845), then it becomes clear that we should count the Greek borrowings from the Anatolian-Aegean proto-language not in the hundreds, but in the thousands. Not to mention that in addition to this nearly 50%, there are still many other ancient Greek words that only became Indo-Germanic roots as a result of the chronically Indo-Germanocentric view of European linguistics, although their parallels appear in Sumerian linguistic monuments from the 4th-3rd millennium BC. In this regard, we remind you of A. Nehring's fundamental methodological insight that even in the case of impeccable Indo-Germanic etymologies, it can be proven repeatedly — and specifically the Greek and the — that the words in question are in fact ancient loanwords (1. pp. 414-415). But we can also refer to Chapter 5 of our Book I, where — just as an example in relation to Latin! — we ourselves 164-165). presented 40 Sumerian-Greek word correspondences, not one of which extends to the names of the simplest everyday concepts and actions, i.e. clearly belongs to the core vocabulary (1. pp.

Let us now give the floor to Schachermeyr again, because from his lines below anyone can easily recognize the real reason why international linguistics has been unable to do anything with the ever-increasing archaeological results, and why it has not been possible to conduct truly objective, methodical investigations into the linguistic connections of the times before the emergence of the Indo-Germanic languages — despite the tens of thousands of ancient linguistic monuments now available for research. If we knew Illyrian, Thracian, Lydian, Carian, or Lycian as well as Greek, Schachermeyr writes, we would find numerous Aegean loanwords in these languages as well. In Hittite and Luwian, words of Anatolian-Ange origin could be distinguished even with our current knowledge, but the relevant research has so far ceased to be useful; experts studying the Hittite language are usually Indo-Germanists, who are of course primarily interested only in Hittite-Indo-Germanic connections (1. Schachermeyr 1979, p. 237 and note 8, p. 341) This is precisely the approach whose one-sidedness and lack of method have been repeatedly pointed out: the newly discovered ancient languages are examined only from an Indo-Germanic (and Semitic) perspective, and other more general, and consequently more significant, linguistic connections are not even taken into account. [This naturally leads to distorted results, and thus, for example, the ancient Proto-Asiatic languages = 6 kon with Sumerian - become, with phenomenal speed and superficiality, "unrelated island languages", and in all cases, if not successfully

850
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... it is possible to show an Indo-Germanic (or Semitic) relationship in them (cf. 11. -6y nyvünk 384-385, p. also). Obviously, the search for Indo-Cyrillic elements in non-Indo-Germanic languages already carries the risk of reckless conclusions and going in circles, "idem per idem," Schachertneyr further emphasizes. If researchers with this approach, e.g. "they find the ending 'ia', this can only be an Indo-European feminine ending for them, and likewise no ending -ti can mean anything other than the ending of the 3rd person singular of the Indo-European verb, etc. etc. He emphatically warns that these aspirations have recently been, for example, in the case of the Minoan Cretan language, stealthily, although not openly stated, but in practice they have become all the more powerful, and therefore it must be pointed out forcefully and clearly that they do not correspond in any way to the historical circumstances and the historical situation resulting from them (1. Schachermeyr 1979, p. 264).

Later he emphasizes again that Indo-Germanists everywhere only look for Indo-European words - and find them. He characterizes this "method" excellently, and we quote his lines in this regard verbatim: "They usually end with a positive (or at least Such attempts, given the vast mass of Indo-European roots and the resources available for such derivations, apparently positive) result for a wide range of methodological aids. To my knowledge, there has never been a single non-Indo-European word for which a language comparator has not found a suitable derivation, if he really wanted to and had the patience and skill necessary for such a thing. Indo-Germanism has almost become a victim of its own over-cultivated routine. With its methods, almost everything can be proven." (1. Schachermeyr 1979, pp. 265-266.)

This is almost the same word-for-word assessment of the phonetic language comparison methods of Indo-Germanic studies, with which even proven non-Indo-Germanic languages — e.g. Japanese Ainu — can be transformed into "Indo-Germanic", as we have already seen in his formulation in connection with the characterization of the so-called "ahistorical" school (1. I. of our book, pp. 77 and "Reallexikon der Assyriologie" 81). At the same time, it also becomes clear from Schachermeyr's previously presented statements why the RLA calls this school "ahistorical": its aspirations do not correspond in any way to the historical circumstances and the historical situation resulting from them (1. above).

Closely related to these questions is the decipherment of the linguistic relics of the late Mycenaean culture by M. Ventris and «I. Chadwick in the early 1950s. These relics of the Mycenaean language are preserved on tablets written in Linear B characters, which developed from the so-called Linear A writing system of Minoan Crete-851

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preserved from about the 14th-13th centuries BC, at Knossos and other sites in Crete, and at Pylos and Mycenae in the Peloponnese. The Ventris concluded that the Mycenaean language was "... a distorted and mutilated Greek" (1. Chadwick 1980, p. 97). The overwhelming majority of Indo-Germanist linguists and ancient historians — even many of the more sensible ones — naturally embraced this interpretation with both hands, because with its help, the Mycenaean "ancient Greek" language of the Linear B tablets of the 14th-13th centuries could be excellently linked to the Anatolian (Hittite, Luwian, Palaian) linguistic monuments of the second half of the 2nd millennium, which showed Indo-Germanic features, in Greece as well. Furthermore, as a result — referring to the language — the entire Mycenaean civilization could be evaluated as an Indo-Germanic cultural achievement from its beginnings in the 19th century. Indeed: in recent summary historical works, without exception, we can read, as if it were a proven fact, that the Mycenaeans were ancient Greeks.

However, there is a lot of doubt about this. Let's start with the fact that many researchers with a truly objective approach have not acknowledged that the Ventris interpretation would concretely prove that the language of the Linear B tablets was ancient Greek. For example, The internationally renowned archaeologist and paleontologist G. Childe, already in 1949, at the initial stage of Ventris's decipherment studies, based on similar experiments published up to that time, emphatically emphasized that he himself and many other scholars, knowing the archaeological facts and the cultural-historical picture of the centuries after the fall of Mycenaean culture, which all testify to the almost complete break-up of Mycenaean traditions and the great decline and primitiveness of culture, could not accept the a priori starting point represented by others that the language of the Linear B tablets was archaic Greek (cf. Childe 1949). Well, despite these fundamental methodological considerations, Ventris's investigations started from the same presupposition. Similarly, N. Plato, the renowned Greek archaeologist, was also very critical of Ventris's decipherment. The sum of his judgment is: "not proven". Chadwick himself admits this, adding that Plato has changed his opinion in recent years (see Chadwick 1980, p. 121). The interpretation was rejected, among others, by the American researcher J. Henle and J. Beattie, the Greek professor at the University of Edinburgh, who wrote: "to prove the sound value of one or another sign" (1. Chadwick 1980, p. 129). Berlin professor E. Grumbach, with a theoretical and methodological edge, asked the question whether the spelling rules established by Ventris were not merely convenient tools with the help of which we can identify foreign words with Greek ones (I. Chadwick 1980, p. of course we do not know whether Ventris did not primarily (emphasis added by Beattie) use these words to 133).

852
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Pe as counter-camp cow + also evidence of Chadwick's referenced work According to > very large, Author from page 121 to 142 almost exclusively deals with criticisms, moving rather in generalities, giving mostly evasive answers instead of concrete refutations, and even arguing outright falsely more than once. Moreover, it is clear from Chadwick's book itself that the reserved blessing of sober professionals is very well-founded. The correctness of the interpretation is strongly questioned, first of all, by the highly suspicious fact that at least 65, but perhaps more so 75% of the interpretable sign groups are proper names (see Chadwick 1980, p. 139). In this regard, we recall that since the beginning of these language comparisons, this has been one of the classic evasions: "method" — i.e. the knowledge- Arguing with proper names — already in Indo-Germanic if there are no common Indo-Germanic equivalents in the memories of a language that they want to Indo-Germanize based on certain cultural priorities, then they come up with Avestan, Vedic or other names (cf. e.g. the Indo-Germanization of the Scythians; our book 1. I, pp. 57-59) — in this case, the countless surviving personal names of ancient Greek mythology and literature; all of these are of course postulated as autochthonous Greek names, although among them, as evidenced by the fact that almost 50% of the common words of the Greek language cannot be interpreted from Indo-Germanic(!) we undoubtedly have to reckon with at least the same number — if not much more — of proper names of foreign origin.

Chadwick's explanations of the "orthography" rules of the Linear B tablets also raise strong doubts in the objective observer. The Ventris did not include a large part of the vowels of the syllabic signs they themselves deciphered in their translations, but on the other hand, they successively supplemented the individual groups of signs with consonants that do not appear in the original texts. According to them, for example, the tablets do not mark the sounds l, m, n, r, s at the end of words, and not even when they are before another consonant inside the word, etc. (I. Chadwick 1980, pp. 108-109). There can be no doubt that the Ventris succeeded primarily in translating the vowel-rich and consonant-poor language of the Linear B tablets into the We saw above that Grumbach also highlighted these arbitrary textual changes in his criticism.

The peculiar procedure is also very instructive, that when the use of the singular or plural in the texts is not consistent - in the ancient Greek interpretation of the Ventris - in its nakedness: the
..... the guard lied" (1. Chadwick 1980, p. 119). Well, here another classic trick of Indo-Germanic studies appears in its full

853

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the unsystematic practice of source criticism, according to which in cases where a contemporary source states things that contradict certain 19th-20th century ideas, then that source is "obviously wrong".

Now less typical is the interpretation of the famous so-called “tripod” tablet found in Pylos, on which – among other things – the words “ti-ri-po-de” and “qe-to-ro-we” can be read repeatedly before drawings of three-legged and four-handled vessels (in Ventris’s *atrasia*). This tablet was at the time the pivotal proof of the correctness of the Ventris-Chadwick interpretation. However, if we look more closely at the words of this tablet, it soon becomes apparent that while the name of the three-legged vessel “tripode” is indeed identical to the Greek “three-legged” and “getowere”, the prefix *qe-* can be matched with the Latin “quattuor” (four), and not with the Greek “tetra” (four). In addition, the first word of the second line of the tablet inscription in question *is not the word “tetra” but the word “tetra”* (four), and not the word “tetra” (four). The interpreters, of course, do not understand this. Now we do not want to point out the striking similarity between the name of the two-handled vessel, “geto” and the Hungarian numeral “ket, kőy”, although this is not a negligible parallel either. For the purpose of our current question, the fact that, according to the evidence of this tablet, in the “Ancient Greek” language of the Mycenaeans, the numerals of the later Greek language three, “It has no Greek equivalent,” Chadwick notes.

The Latin four and the “third, unknown language” two, two were freely mixed (whether this relation is related to the ancient Hungarian language requires further in-depth investigations, but is not authoritative in this context is far more important). This undoubtedly indicates that the language of the Mycenaean civilization was a pidgin-like mixed language, in which the words of the ancient Anatolian-Aegean linguistic base layer were mixed with various other linguistic elements, and from this conglomerate the Ancient Greek language crystallized over the following centuries.

“unknown language” was related-

We are not alone in this opinion. In 1964, S. Levin examined *Levi's* interpretation with a thorough methodological analysis, and although he broadly accepted it, he emphasized that barely half of the sound values of the signs can be considered to have been definitely deciphered. He also pointed out emphatically that in all those numerous cases where there is no Ancient Greek parallel to a word, we must reckon with the presence of a foreign language (1. Chadwick 1980, pp. 193-194). In other words, the methodical critical examination actually came to the same result as we mentioned earlier: in the case of the language of the Linear B tablets, we are dealing with language mixing, pidginization, the beginnings of the development of a language that later became Indo-Germanized, similar to the Hittite, Palaean, and Luwian languages. In this respect, in addition to the numerous Mycenaean words of the A tablets that do not have Greek equivalents,

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We must also count all those common Mycenaean-Greek words that do not have an Indo-Germanic etymology, i.e. are undoubtedly adverbs in Greek.

We remind you that such words account for almost half of the known Greek vocabulary.

In the last pages of his book, Chadwick deals with the current state of decipherment of Linear A (the Cretan predecessor of Linear B). He says that so far only one word is certain: "ku-ro", which means roughly "sum" or "as much as", and then writes about the word B: "If we could find such a word in a known language, we could solve the problem of Linear A, but unfortunately no decipherment has so far led to a convincing result on this point." (1. Chadwick 1980, p. 210) According to him, regarding the character of the language, one is the vast and extensive Indo-European language family, of which several branches lived in Asia Minor in 2000 BC, the best known of which are only two language families; among them Hittite, but — Minoan — with Hittite and Luwian, but without any there is also a lesser-known language, Luwian, spoken in the southwest." (1. Chadwick 1980, pp. 210-211.) Thus, several people have attempted to compare the language of the Linear A tablets with results — Chadwick continues. The semiologist Professor Cyrus Gordon, on the other hand, interpreted the language of the Linear A script as Semitic, but his method is not at all convincing. From the negative results of these comparative attempts, he then draws the conclusion that the Minoan language has died out without a trace, and has no known relatives (see the above in 1. Chadwick 1980, pp. 211-212).

We have thus arrived once again at the boringly familiar kintona, which is already penetratingly evident in Chadwick's characteristically "narrowed" questioning; according to him, with regard to the possible connections of the Minoan language, only two language families, Indo-Germanic and Semitic, can be considered, and since Minoan shows no parallels with these, it can only be an unrelated, "extinct" "island language". He does not even consider other possibilities of connection, let alone examine them. Moreover, he does not even mention that the Minoan language may have belonged to the Anatolian-Aegean language group, although the vast majority of researchers consider the Minoan language to be the most important representative of this language community in the Aegean region (I. above, p. 848).

But we can go even further. The Minoan word mentioned by Chadwick earlier, which has so far been the only one to be read from the Linear A tablets, "ku-ro" meaning "amount", has an obvious parallel in Sumerian; "gur, guri, guru" = "voll sein, voll werden" (to be full, to become full), in compound form "ul-gur,-ra" = "Zahl" (number); and also "kur, kuru," = "gross, Grösse" (large, greatness). Let us relate with what great regret Chadwick stated above that if we were to find in a known language a word with a similar meaning and phonetic form to the word "ku-ro", we would have to

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find, then we could solve the Minoan language problem, but unfortunately.^x Well, the apparently so feverishly sought parallel is already in the linear The tablets

It appears in the only known Minoan word, and moreover, it undoubtedly indicates that the Anatolian-Aegean language community, to which Minoan also belonged, was obviously in close linguistic relations with Sumerian, or in a broader sense with the ancient Proto-Asiatic language koine. Why should we not therefore continue research in this very promising direction, in order to

despite the fact that the findings uncovered by archaeology in recent decades almost imperatively point towards Near East in every respect? Only because researchers with Chadwick's approach

~ who unfortunately still constitute the vast majority today ~ are inherently rigid
they are closed off from all kinds of Indo-Germanic and Semitic languages and dialects
from comparative linguistic studies outside or before them. Instead, they tend to mystify, as Chadwick
did when he spoke of the fact that the "vast and extensive" Indo-Germanic language family "had numerous branches in Asia Minor in 2000 BC", even though it is well known that on the one hand the 3

Luwian and Palaian languages can only be dated to the middle of the 2nd millennium, and no Indo-Germanic language remains are known from before that, on the other hand these three languages were still very distant even in the second half of the 2nd millennium from being able to call them truly developed Indo-Germanic languages (1. III. our book 410-411. and 557. pp.). Thus in 2000 BC there was no mention can be "vast and extensive" Indo-European language family.

This question also includes the characteristic way in which Chirikov tries to make the Cretan language appear almost completely unknown, suggesting to the reader that we know only a single Minoan word. 15 On the other hand, he does not mention the fact that, on the one hand, numerous Minoan words can be read from the Linear B tablets found in Crete, and, on the other hand, Hesychios has handed down to us more than 120 Greek loanwords of Cretan origin (see e.g. Glotz 1923), about a third of which are also found in Semitic languages, and some of them can even be detected in Sumerian.

Let us look at some of these common Greek-Aegean-Sumerian-Semitic words; kanna — cf. Sumerian “kunnin and gannum” = ‘kanna, jug’ (its semantics, as well as its Semitic, Indo-Germanic and Hungarian parallels in our book 1. III. pp. 578-582):
 kybernao = ‘to govern, to lead’ – cf. Sumerian “kub” = ‘, Krug’ – cf. Sumerian “kud” = ‘a container made of wood or leather’; Akkadian “kuddu” = ‘... lenés’ és „gubur” sz. ‘assistant, deputy’; héber “giver” =
 u.az.; Old Hebrew “Statthalter”; Arabic “kebir” = wotatthalter’ (book 1. 1. p. 118, also); kados = ‘y’ = faveder (I. pp. 595-597, also);

01016 = *yū* (oil), = This common Minoan-Greek-Semitic word for "oil" is particularly interesting for us, because in its case we are obviously faced with a typical pidginization process. In Sumerian, namely, Haronian

856

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there were different phonetic words for "oil": "i," #11," and aa"; in the word "elasa" three synonyms of the Sumerian name for oil were apparently merged into one word, as a typical pidgin pleonasm: i, + li, + ja, "e-la-ia",

Similarly, the following common Greek-Aegean-Semitic words have 15 Sumerian parallels lying on their hands: perphyros = "purple, crimson red" -- cf. Sumerian, "bar" = "rotglänzend, glühend rot" (shining red, burning red), doubled in the form , barbar" to denote a particularly bright red light, e.g. "Sonnenaufgang, Morgenröte" (dawn red); Hebrew Separera' (Morgenröte); kamara = "Gewölbe" (arched arch) -- cf. Sumerian, "gan" = "sich biegen, biegen, Neigung" (bend, bend, incline), Akkadian kamatu' (u.az); pyrgos = "Burg" (vár) -- cf. Sumerian, "bar" = "umschliessen, Ringmauer" (round wall, circular wall) and "barag" = "Wohnung, Wohnraum" (dwelling, dwelling place); Hebrew "bira" (Burg); Arabic "burgh" (Burg); kantharos = "handed jug" -- cf. Sumerian "gan, kan, kunin, gunum" (1. above for the word "kanna"); skaphos = "Wanne" (tub), Minoan "su-pu" = "kind of vessel" -- cf. Sumerian "sab"

= 'Becken, Topf' (basin, bowl, pot), as well as Assyrian 'saph' and Hebrew 'saph', both 'Becken, Schale' (basin, bowl); koros = 'Scheffel' (vessel) – cf. Sumerian, *gur* = 'Getreidemass' (grain measure); Akkadian, *tag* = 'schlagen' (hit, beat); Hebrew, *gurrū* (i.e.); tykos = To illustrate the number of lexical items between Sumerian and Akkadian (Barnard 1990: 40). Sumerian-Greek word correspondences presented in Book I (Book I, pp. 1-100) follow. In the left-hand column, the Sumerian words and their German meanings are listed, and in the right-hand column, the parallel Ancient Greek words, also with their German meanings.

41. SUR, TUR (bull, to stab) BULL-bone (Taurus)
 42. GAD, GADA (flax, linen garment) CHIT-on (linen garment)
 43. HAB, (to strike down) KOP-to (their)
 44. BARUN, FIVE (Ameise) BYRM-ex, MYRM-ex (ant)

45. DIR, DIRIG (go)

46. KUR (Mountain)

47. KUR, (crooked, bend)

48. KUR, (strong, powerful) (Furst)

49. GIR, (blitzen, Bhitz)

50. BAL (beat, split), 857

TREK-o (go)

KAR-enon (tip)

KAR-non (Horn)

KRA-teros (stark), KAR-anos

KER aunos (Lightning bolt)

PLEX o (to strike, to split), BULUG, (to strike, to split)

PLEG e (blow)

===== PAGE 847 =====
858

. BAL (turn, spindle) . GUL (cut) . GUL (smash)

GUB (bind, loop)

. IGI (eye, see)

BIL (to light, to burn, fire) BAR (to sprout)

BARAG (enclose, ring wall),

BARAG (apartment) BARBAR (intense red, dawn)

. GIL, (running)

. GIL, (light up)

. GAL (a large jug)

. GUR, (cut)

. HAR "(to curl up)

. HARUB (an insect)

. GAN, KAN (tube frame)

KUNIN, GANNUM (Kanne, Krug) . 1, LL és IA, (OI)

KUD (a container made of

(Wood or leather)

. GAM (to bend, to flex, inclination) . DURAH (Capricorn)

. GUR (a liquid measure for grain)

SAB (cut off)

SAB (basin, pot)

. SAG (covering, net) . TAR (point, spike) . TAR, DAR (split, separate,

(a piece)

. KUD, (to sit, to live) . AGAR (field, meadow) . TAG (to strike)

PLE-ko (braiding) -

GLY-pho (chisel, cut) KOL-apto (beat) HYPH—aino (weave) |

EYE (light, brilliance, eye, gaze) PHL-ox (fire)

BR-Y-o (judge)

PYRG-os (Burg)

PORPHYR-os (purple) KEL-os (runner)

KEL-eos (lodern)

KYL-ix (Kelch)

KRI-no (cut) HER-po (crawl, wriggle) HERP-es (worm, snake) CHAN-na (reed mesh) KAN-tharos (handled jug) E-LA-IA (oil)

KAD-axis (Circle)

KAM-ara (vault) TRAG-os (buck) KOR-os (bushel) XYPH-os (sword) SKAPH-os (tub, trough) SAKK-os (sack) TYR-sis (tower) TRY-phos (piece)

KEUTH-os (stable, dwelling) AGR-os (field) TYK-os (hammer)

Reviewing what has been said so far, it seems beyond doubt that the ancient Anatolian-Egyptian language community originally consisted of the languages and linguistic descendants of the settlers who migrated from East Asia — mainly from the area of the Hacilar — Catal Hüyük culture circle in Central Anatolia, and to a lesser extent from the area of the Syrian-Kclet-Anatolian-Cilician Halaf culture — in the first and second waves, who established the earliest agricultural cultures in Greece (proto-Sesklo, Magulica), the Balkans (StarCvvo, Karanovo), the Carpathian Basin (Körös) and the Mediterranean coastal areas (the Cardium or Impressé pottery culture circle) (cf. above, pp. 716-719). On top of this primary ancient Anatolian and Syriac linguistic base layer, there were then layered

===== PAGE 848 =====

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those Eleusinian, and later specifically Sumerian linguistic influences, which were brought by further waves of people migrating from Mesopotamia and its surroundings in the late Ubaid, Uruk and Jemdet Nasr periods, as well as in the 3rd millennium (early Sumerian dynasties, Akkadian period, Ur III dynasty), to Anatolia, the Aegean region, Southeastern Europe and the western Mediterranean regions (1. e book 2. fcj.). Thus, the Sumerian-Greek word parallels presented in book J. and now obviously did not enter the ancient Greek that developed only much later by a direct route, but undoubtedly through the mediation of the various dialects of the Anatolian-Aegean language community. In this regard, we must take into account Minoan, Pelasgian, Western Anatolian Lydian, Carthaginian and Lycian, as well as the spoken language of the Mycenaean cultural circle, from which the ancient Greek language eventually developed under the influence of Aeolic, Ionian and Doric superstratification.

We can see how complex ethnic and linguistic mixing and superstratifications consolidated the individual, already concretely identifiable languages and language groups at the time, and how naively 19th-century linguistics imagined their origin, reducing all these complicated paleolinguistic processes to simply linear language family descent and regular sound development.

Looking further west, however, the components of the Proto-Asiatic linguistic influences change slightly. For example, Etruscan has many more Sumerian parallels than Anatolian. in the surviving memories of the more eastern languages of the @ger language group, although Etruria lies much further from East Asia than these. This circumstance, which is surprising at first glance, however, becomes easily understandable if we consider that in the Italian territories — apart from the scattered coastal foci of Cardium pottery — agriculture began much later than in the Aegean region; the majority of the East Asian colonists who arrived in Italy therefore left their homeland for the most part during the late. Ubaid period, and thus they must have brought with them a considerable amount of linguistic elements from the ancient Sumerian or pre-Sumerian spoken language of this period (cf. pp. 709-711). In other words, the primary linguistic base layer formed in the Italiabanki was not only derived from the languages of the ancient Anatolian-Syrian Neolithic cultures, as the primary substrate of the more eastern areas of the Anatolian-Aegean language group, but also to a significant extent from the late Ubaid period Proto-Sumerian. Then the groups that arrived later from the east obviously also more or less 0 VN BUS a Smer ty k linguistic material of the younger stages of development of the Sumerian language — even if they did not come directly from the Sumerian language area, since Sumerian was the lingua franca of the whole of East Asia from the middle of the 4th millennium until about 2000.

The origins of Etruscan culture, people and language, and their connections with the East are among the most hotly debated issues. The reason for this is that the

859
===== PAGE 849 =====

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In the light of recent research, it is becoming increasingly certain that the Etruscans played a key role in the development of Roman civilization and state organization, and that their language significantly influenced the development of the Latin language, especially in its initial, still malleable phase. These questions are particularly important for our present investigations, because Roman culture and the Latin language, in turn, had a decisive impact on the cultural and linguistic relations of the whole of Western and Central Europe. Therefore, we need to get to know the Etruscans a little more closely. It is well known that Etruscan culture predates the beginnings of Roman culture by centuries. Much less known, however, is that the formation of Etruscan cities was roughly simultaneous with, and in some places even preceded, it. In Greece, the concentration of villages into urban centers took place in the 8th century BC, and in Etruria this process began at the end of the 9th century, and was largely completed in the 8th century (see Grant 1981, pp. 21-22). Archaeological research, for example, in the regions of Targuinia and Populonia has revealed large-scale, industrial copper, tin and iron mining, as well as metallurgy, as early as the end of the 9th century, which only allowed for the formation of organized city-states that brought together significant masses of people.

It is conceivable that these formations coexisted. And indeed, excavations at Luni sul Mignone, near Targuinia, have in recent years revealed an urban settlement from the end of the 9th century (1. Grant 1981, p. 28). Similarly, Bologna (Etruscan name Felsina), which lies far to the northeast of the Etruscan homelands, was already a city by the beginning of the 8th century (1. Grant 1981, p. 127).

It is therefore obvious that the development of Etruscan urban civilization cannot be viewed as an adoption of Greek culture, since
Around 800-750 BC, we can still speak of a widespread Greco-Greek culture, let alone, in the true sense of the word, that the about the culture of Aronians,
Greeks could have transmitted an urban culture to the Etruscans at that time. Despite this, the prevailing opinion in international research today is that Etruscan civilization was shaped by decisive Greek influences. This ineradicably ingrained Greco-centric attitude of European scholarship and the self-contradictions that necessarily follow from it can be clearly observed even in the work of M. Grani, to which we have repeatedly referred earlier. Although, as we have seen, he himself stated that Greek and Etruscan urbanization occurred roughly simultaneously in the 8th century, and that in the area of Targuinia the

A distinctly urban settlement from the end of the 9th century was excavated. Later, however, without any transition, he begins to speak of the fact that the Etruscan cities were formed under the cultural influence of Greek merchants and received their strongest economic, social and civilizational impulses from Greek culture (l. Grant 1981, p. 40). All this

80) ===== PAGE 850 =====

and mcg explains that the Greeks of Cuboc around 775 BC commercial jyjyyyy were established on the island of Ischia (north of Capri), then between about 750-725 also in Cumae (near Naples), and 6 commercial lera catalyzed the development of the entire Etruscan culture (l. Grant 1981, pp. 47-52). In other words, in Grant's view, the settlement near Luni sol Mignone, or Bologna, which could already be called urban at the end of the 9th century (Grant's own words!) and which — also according to Grant — was already a city at the beginning of the 8th century, would have urbanized under the cultural influences of a Greek trading colony founded around 775, but which was not urban for a long time (again, only Grant's statement). We will now mention, just for the sake of completeness, that Luni sul Mignone is located more than 250 km as the crow flies from Ischia, and Bologna is almost 500 km away. Apart from this temporal contradiction and geographical distance, however, we must also point out that the excavations carried out at the site of the alleged Greek trading settlement in Ischia in question, in addition to Greek geometric pottery dating back to around 775-770, also brought to the surface a locally made Phoenician inscribed vessel, together with Syrian seal cylinders and Egyptian-Phoenician scarabs (l. Grant 1981, p. 52). In other words, it seems very likely that this Ischia

"Greek" settlement around 775 was actually a Phoenician trading post, which of course - among many other goods - produced the then fashionable geometric. It also traded in decorated Greek vessels. In any case, it is a rather unmethodical procedure to state categorically, based solely on geometric potsherds — and at the same time ignoring numerous other finds that are indisputably Phoenician — that the settlement was founded by Greeks. It is another matter that about 60-70 years later, during the large-scale colonization of Sicily and southern Italy, Ischia also fell into Greek hands, and since 700 it had been a true Greek colony. The same tone runs throughout the book. We will present a typical example. On page 54 of his work, Grant explains that the aforementioned Greek trading depot in Ischia produced most of the beautiful filigree jewelry found in Etruscan tombs, and that they are therefore the work of Greek artists. However, a few pages later we learn about the same jewelry that it is not found in Greece" (l. Grant 1981, p. 73). - Commentary — we believe — is unnecessary, The filigree work was done with a fineness never achieved before, and the granulation technique was used to create patterns and silhouettes that

When speaking of Etruscan culture, we cannot avoid briefly discussing the founding of Rome and the emergence of the Roman city-state. These are questions of paramount importance, because only by knowing the real historical situation can we fully appreciate the decisive role that the Etruscans played in Roman culture, state organization, and religious life.

861 ===== PAGE 851 =====

They played a role in the formation of cults and social order, as well as in the consolidation of the Latin language. Archaeological and prehistoric research has clearly shown for decades that the Romulus tradition, which places the founding of the city of Rome in 753 BC, is nothing more than a late historical falsification, deliberately carried out at the end of the 1st century BC, with which they wanted to kill two birds with one stone. On the one hand, the legend was intended to prove that the Romans, who were already playing a world power at that time, had an antiquity and culture on par with the Greeks; on the other hand, it also proved to be excellently suited to show the primary role of the Roman kings of Etruscan origin (Tarquinius Priscus, Servius Tullius and Tarquinius Superbus) who ruled between about 620 and 509 BC.

.in the construction and the formation of economic, social, political, and military institutions — such things could not be denied in any way, because Greek historians also mentioned them — they could be presented as if they were merely the necessary stages of development of a city that had existed and flourished for 150-200 years, which would have occurred even if the three kings in question had not happened to be Etruscans. At the same time, however, the good Romans also took care to distribute a considerable part of the eminently important changes and innovations that occurred during the reign of the aforementioned Etruscan kings proportionally among the four fictitious Roman kings installed before them, in order to create the impression of a normal and gradual development over the centuries. Thus, they attributed to Romulus the establishment of the first state institutions, the

.the establishment of the state, the division of the people into districts and the organization of the army. His successor, Numa Pompilius, would have introduced the cults of the Yalla; the third king, Tullus Hostilius, would have been given the glory of the destruction of Alba Longa, and thus the establishment of the hegemony of Rome in Latium; finally, Ancus Martius would have laid the foundations of maritime power by building Ostia (l. Propylaen Weltgeschichte TV. vol. 50. p.). We can see that the Etruscan kings would not have had much to do after this. In contrast to all this, archaeological research has proven that in the 8th-7th centuries we cannot speak of a city at all in the area of what would later be Rome: from this time only rudimentary Tälusfas settlements were found on some of the seven Roman mounds, which consisted of miserable huts made of a few woven reeds, stuck with mud, built on the surface without foundations, and the valleys between the mounds were filled with uninhabitable swamps. Then, at the end of the 600s — that is, at the beginning of the reign of Tarquinius Priscus — similarly primitive buildings appear in the depression between the Palatine and the Capitoline, in the area of what would later become Ferum, i.e. at that time they were drained 1 862 piles. Some years later, around 575, these huts suddenly disappeared.

===== PAGE 852 =====

h from 8 valleys, \$ carefully leveled in place with conscious planning They created a large open space paved with crushed stone: the Forum; the truly urban center of Rome (pr. Bloch 1970, 176. | p.). In the same period, regular rectangular houses built on stone foundations, made of mud brick, also appeared, in the ancient part of the city. It was founded by the Etruscan king Tarquinius Priscus | around 575 BC, 8 consequently 6 | the first king of Rome. So they are only transparent t= jyy Varost - inventions of later times" (l. Prop. . pyl. Weltgesch. Vol. IV. p. 52.). No Latin king ruled in Rome,

The founding of Rome undoubtedly depended on the fact that the Tiber was fordable nearby, and the fortification of a convenient and important crossing point almost offered itself to the Etruscans to secure the land route to the colonies in Campania. But the city itself also testifies to an Etruscan foundation: Is the name Rome an Etruscan word, Ruma? (l. Propyl. Weltgesch. IV, vol. 52. p.). En In stark contrast to all these facts, which have been well-known in the literature for several decades, in schoolbooks, popular works, but also in encyclopedias and summary scientific works, we still find the Romulus-like city-founding myth all over the world today, with the fictitious date of 753, and even this is usually postponed by a century or two, and the aforementioned miserable huts, which can be traced back to the 9th century on the Palatine Hill, for example, are interpreted as if the history of the city of Rome (!) began with their appearance (cf. e.g. Brockhaus Encyclopedia 1973, vol. 16, p. 61) — This should not come as a surprise to our patient readers, after all, the Romans were Indo-Germanic, while the Etruscans were not.

In connection with this question, and also in the light of the supposedly decisive Greek cultural influences on the Etruscans — which are mostly definitively established using the methods outlined above — it is very instructive that, for example, Tarquinius Superbus had a monumental Etruscan-style temple built on the Capitoline between 520 and 510, which was hardly comparable in Greece at the time. Only the Temple of Artemis at Ephesus, with its length of 55.1 m, surpassed the Capitoline temple, whose longitudinal side was 53.28 m. If, on the other hand, a relatively insignificant man, who was only 60-70 years old around 510,

If such a huge temple can be found in an Etruscan colonial city, then there is no doubt that temples of similar size had already existed much earlier in the centuries-old cities of the Etruscan motherland. The simple explanation for the lack of such examples is that no regular excavations have been carried out in the area of any large Etruscan city to date.

863 ===== PAGE 853 =====

Another necessary consequence of the Grecophile approach of European scholarship is the increasingly strong rejection of the Asia Minor, more precisely Lydian or Pelasgian origin of the Etruscans, as handed down to us by Herodotus and numerous other ancient authors. Whereas earlier research almost without exception accepted the Eastern origin, these efforts began to take on greater dimensions at the very time when the increasing archaeological finds increasingly showed that the relatively rapid development of Greek culture, referred to as the "Hellenic miracle", can be traced back mainly to the dominant influences of the surrounding earlier cultures (Crete, Asia Minor, Phoenicia, Syria), i.e. to precisely those sources on which Etruscan culture was also based. In this context, it is characteristic that the earliest monumental Greek temple, the aforementioned Artemision of Ephesus, was built around 550 with the generous financial support of the Lydian king Croesus; when the Greek cities of Asia Minor were under Lydian rule; and not without precedent, but on the site of the ancient sanctuary of the Asia Minor goddess Cybele. — Under such circumstances, therefore, there is no other way out for research with a Greco-centric approach than to deny at all costs the Asia Minor origin of the Etruscans, since in this way it can at least be argued that the Eastern elements that undeniably dominate Etruscan culture did not reach Etruria directly, but only as a Greek cultural radiation, through the simple adoption of the so-called style of the Greeks.

orientalizing"

As a result, one of the most well-known Etruscan scholars of our time, M. Pallottino, also referred to, devotes eight full pages of his book to the criticism of the Eastern origin (l. Pallottino 1950, pp. 54-62). However, if we read his explanations carefully, we must conclude that he almost exclusively dissects certain obvious errors made by previous researchers who claimed the Eastern origin of the Etruscans — those made in questions of detail — and only these he succeeds in showing that they are incorrect. Thus, for example, with exhaustive meticulousness, he refutes for pages the truly fundamentally erroneous (and, as we will see below, radically contrary to ancient traditions) position according to which the Etruscans only migrated to Italy in the times immediately preceding the orientalizing style that emerged around 700, i.e. between approx. 750-700. It is natural: this theory belongs to the last century, it is based on the then still very incomplete archaeological knowledge, and thus with the help of the results of recent excavations it can be easily shown that the beginnings of Etruscan culture are much older than the turn of the 8th/7th century, so we cannot count on any major immigration at this time. However, he does not mention that this idea is now long outdated, and that only a few of the modern critics of eastern immigration believe it. In fact, he explicitly states the question as follows:

864 ===== PAGE 854 =====

It is as if this late immigration is still the cardinal tenet, a kind of general belief, of the followers of the Eastern Creed.

In addition to the archaeological, historical and linguistic data mentioned above, there are a whole series of other oriental connections, which we do not have the opportunity to present in detail at this time. In short, these organic parallels extend to the field of religious images, myths, cultic ceremonies, superstitious beliefs (e.g. liver divination), social organization, technical knowledge (e.g. sewerage, construction, metalwork) and much more, which characteristics were largely unknown in Greek culture and could not have come from Greek mediation. Thus, we must obviously agree with the renowned French Etruscan scholar, R. Bloch, who

according to "the hypothesis of the immigration of eastern elements is very probable, considering the current state of research" (I. Bloch 1970, p. 71), and consequently Thesis of Eastern origins "it finds more and more defenders." (1. Bloch 1970, p. 83)
Let us now return to linguistic questions. The Etruscan language
It was of the ozo type, and according to the research, it is not related to
"neither with Azindo-Germanic nor with the Semitic languages. Thus, in the last century and at the beginning of our century, attempts were made to bring Etruscan into a relationship with several inflectional languages (e.g. with Basque, some Caucasian languages, Dravidian, Finno-Ugric, etc.), or more precisely, to interpret Etruscan words of unknown meaning from these languages, but these attempts all remained unsuccessful. As a result of the failure of these language comparisons, the so-called combinatorial method came to the fore, which starts from the surviving texts themselves in interpreting the Etruscan language without calling on the help of other languages (1. Pallottino 1980. pp. 190-191.). However, after it became possible to classify Etruscan with a fair degree of accuracy within the ancient Asia Minor-Mediterranean language group (1. above, p. 846.), again the question of etymological comparisons arose. In determining the affiliation of the Etruscan language within the framework of the Anatolian-Aegean-Tyrrhenian language unit, A. Trombetti achieved brilliant results, but his methodological principles were based exclusively on his own, almost unlimitedly wide linguistic

===== PAGE 859 =====

He tailored his scientific knowledge to his own, and these were too closely related to his own training and personal abilities. Thus, official science could not follow Trombetti in this direction and seems to have adopted a position of cautious waiting (1. Pallottino 1980, p. 193). As a result, the combinatorial method still dominates research today.

Etruscan inscriptions appear at the beginning of the 7th century BC. We can read the letters accurately because the Etruscans adopted a 26-letter version of the Phoenician alphabet, probably through Greek mediation — although this latter question is still disputed today (1. Pallottino 1980, p. 209). It is very important that in the earlier period (7th-5th centuries) the use of vowels is much more developed than in the later inscriptions. There is vowel assimilation. In the late texts one can clearly observe the transformation of voiceless sounds into voiced ones and voiced ones into spirants, which in some respects corresponds to the vowel shift in the Germanic languages. The absence of voiced final sounds (b, g, d) in Etruscan can perhaps be explained by the same process, that is, by their transformation into voiceless ones in prehistoric times. The most significant and striking characteristic of word stress is the strong stress on the first syllable and the consequent vowel drop in unstressed syllables. This is especially characteristic of the late linguistic stage, when we can observe the formation of complex consonant clusters (all this I. Pallottino 1980, pp. 212-213).

These phenomena are obviously signs of the slow, gradual Indo-Germanization of the Etruscan language, a process that, according to them, proceeded in parallel with the political, economic and cultural decline of the Etruscans, and with the growth of the power of the Greeks and then the Romans in southern Italy, and finally led to the complete disappearance of the Etruscan language around AD. In this context, it is also very remarkable that while the Etruscans, according to the research, originally adopted an alphabet consisting of 26 letters (1. above), in the early written records of the 7th-5th centuries it only appears with 23 letters, and in the later ones only with 20 (1. Pallottino, p. 211). This, however, most likely indicates that on the one hand Pallottino is on the right track when he assumes the existence of consonants ab, g, d in ancient Etruscan, and on the other hand that the formation of ey eiruszk 400 goes back to much earlier times than the written records that appear at the beginning of the 7th century, namely to the era when voiced final sounds were still used.

This, however, makes the assumption of the transmission of the Etruscan alphabet extremely unlikely: the Greeks themselves only adopted the Phoenician alphabet in the 8th century. In word formation, the phenomenon of the accumulation of suffixes is particularly interesting, such as the so-called double or even triple possessive case. The superimposed suffixes have different they can be of a different nature and value, as for example when 870
===== PAGE 860 =====

place-defining verbs are related to the possessive case (1. Pallottino 1980, pp. 213-214). It must be emphasized that all these features, together with the aforementioned consonantal findings and vowel deletions, are found in the same form in the Sumerian language, for example, the voiced consonants becoming voiceless or the disappearance of certain vowels in a completely analogous way to Etruscan, can only be observed there in late texts, when Sumerian was already under very strong Semitic influences and its use as a living language soon ceased. These grammatical and phonetic agreements are particularly important because they contribute significantly to a better understanding of the apparent phonological differences in the Sumerian-Etruscan word parallels to be presented below.

Pallottino gives the following example of known cases of name-changing (1. id. m. p. 215): "spur" = probably "city"; "spur-al" = "of the city"; "spur-e vi" = "in the city"; "spur-eri" = "to the city (or for the city)". In this example we can clearly observe the close linguistic correspondences between Sumerian and Etruscan. The noun itself is very likely of Sumerian origin: cf. sum. "Sub" = "fest gründen" (to firmly establish) and "uru" = obviously refers to the well-known fact that the Etruscans considered cities to be real Stadt" (city); thus in composition "sub" + "uru" = "firmly, surely founded city". The adjective cities only if they were founded with the exact observance of complex ritual regulations, that is, with "solid regularity". Since in the compound word "Sub-uru" "uru" was the essential root word, the one that really meant a city, the emphasis undoubtedly fell on this, and thus the u of the prefix "Sub" and the final -u were dropped, and ab was voiceless to p in accordance with the Etruscan sound order: spur. — This derivation seems very probable in light of the previously outlined Etruscan sound development, and can hardly be criticized semantically, but it has the weakness that the word combination in question cannot be found in Sumerian. For this reason, we do not consider the interpretation to be certain.

However, the following agreement in the root excludes all doubt: Etruscan — evi = "-ban, —ben" and Sumerian —da, —de., -di, = Locative-Post-position "in, bei, zur Seite, zu" (-ban, —ben, —nāl, —nāl, —hoz, —hez); Etruscan -eri = "-nak, —ért" and Sumerian —ra, -ar, —ur., —ir, -er = Dative-Post-position "zu, auf, für" (-hoz, -ra, -re, —nak, —rt).

The Hungarian parallels that appear incidentally are also noteworthy: the Etruscan — evi (Sumerian -da, —de, —di) suffix compares with the ancient Hungarian -d, -da, —de locative suffixes (e.g. oda, ide, Varad, Varda), and the Etruscan -eri (Sumerian era, er, etc.) suffix with our -ra, -re and —ért suffixes.
871

===== PAGE 861 =====

After these preliminary remarks, let us now see some obvious Sumerian ~ Etruscan word correspondences. For our comparisons, we relied on the glossary of Pallottino's cited work (1. Pallottino 1980, pp. 224-233), in which he lists about 170 Etruscan words with more or less known meanings. However, we did not take into account the reconstructed words (, star), the names and their derivatives, and those completely uncertain words, for which the meaning is only as important as, for example, "pronoun", or we added the two suffixes presented above and the ending -0 yfer from page 214 of Pallottino's book (these are not included in the glossary), i.e. in the end we have 141 . pronoun or adjective", etc. Thus, 138 common words with a known meaning with approximately certainty or approximate accuracy remained from the list, for which

we compared the Etruscan word with the Sumerian. The Greek letters common in Etruscology are used in the following in Latin transcription: 9 = th, p-phés x = kh. Note It should also be noted that this time we only wanted to present illustrative examples, and therefore, regarding the meanings of Sumerian words, we limited ourselves to the data of A. Deimel's "Sumerisch-akkadisches Glossar" volume, in which barely one third of the meanings of individual words can be found (Deimel lists the complete semantics in the four volumes of the "Vollständige Ideogramm-Sammlung").

Sumerian-Etruscan word parallels			
Sumerian	Etruscan 1st AG (to do, to do)	AC-as (to do, to offer) 2. AS, IS (single, sole)	AIS, EIS (deity) 3. AL-GI (to give)
AL, ALCE, ALQU (to give, to offer) 4. ME (to come)		AM, AMA, AME (to be) 5. AB, ABBA (father, grandfather)	APA (father?, ancestors) 6. A-RA (to go) 8.
AR, ARA (verb of motion) 7. ASILLA, ASILAL (joy, celebration)	ASIL (some sacred institution) 9. GALA (name of priestly class)		GAR (to do, to make, to prepare) CAR
	CEL, CELU (priestly title) 10. KUS, (to rest)		CES (to place, to be placed, to lie) 11. ES, ESE (three)
CI (three)			
12. DA, DE., DI, (in, at, there, there)	ETHI (in, —in) 13. ER, IR, UR., AR, RA (for, for)		ERI (for, to)
14. BAR (burn, fire)	VER-s (fire) 15. ZUR (to pray, to beg) 872		ZER-i (sacred act, "ritual"?)
===== PAGE 862 =====			
are, ZAL, ZALAG (feryleni, kitýnm) ZIL, ZILAC, ZILATH			
(államjvatalncck, dignity 17%. TAR (&smi), SUR, (5), DAR THEVR u (bull)		THAUR, THAUR a (sir sibrott (hasztani), DAR-DAR (lyuk, hasadék) 18 SUR, SUR (bika, sztim)	
19, DU, DE, (to do, would do THE z (tenm, to do 20. DA földal, near, bordering, close THUR (family community; ending denoting a family, " fr"			THUI (au) for something) ýý. TUR (small, young, male child)
22. ILU Gajszd, wall, complaint6), verb denoting (to complain)) 23. ITU (month) \ 13 or 15 days 94, LAGAR (minister, official) LAUC, LUC-, LUKHUM probably			ILU (supplication or offering ILU-Du,
"Áyrály") 25. LAB (past), LIBIR (past)		ITU (divisor?), IDUS (the month	
	LUPU (mortal) 26. MAH (large, quantity, mass)		MAC, MAKH (almost certainly 00) 27. MAH (big, huge)
	MAC-strev (title of state dignity; vo, Latin mamnster") 28 MAR (to plant, to insert, so-called, inserted, known 29.		MAR-u (title of state dignity), to appoint) other institution
MUD (border of land, METH-lum (state? territory or its ending), can be related to the concept of MAD (country, district) 30. MAH (large, huge)			
	MEKH (policy: or institution		
concept relating to; see ".zalath mekhi rasnal" = Eirúria praetora"), this expression is more likely to refer to the great Etruscan			
praetora" can be translated as, 31. MA,-E, MB-B, MEN, (me), MIME, MINI, ME NE (pronoun ME, MI (we) (bed)			first person "En, me") 32, MU-NA,
	MUN, MUNI (place, ravine, grave?)		
33, MUR., MURGU (cover, covering, toh) MUR,MURCE (to be somewhere, to be somewhere?, probably a derivative of MURS			hamv veder, szarkofág")

34. PAP (what)	DAD (father? or grandfather?)
35. PAR (bright, radiant)	PAR, PAR khis (Stone expressions referring to social status or position)
36. PAD, (basket?, some kind of sda or container y) yy) cistern, Lilt) 83	PAT -ua (name of vessel) PU (vessel name, "well"?)
===== PAGE 863 =====	
38,	
KUD (wooden or leather container)	
QUT-un (name of vessel)	
39. SAR (amount), SAR (many)	SA, SAR (number noun: ", four" or "six"?)
40. SAG, (j6, pious, pious, to worship) SAC (verb denoting a religious act or situation)	
41. SANGA (name of priestly class)	SAN (deceased?), SANTI (priest's title)
42. SEDI, SID (to calm down)	SAT, SATH (to lay down or to
establish?) 43. SAG (gift, dowry)	SCU (to give, to lay down, to offer?) 44. SUB (to establish firmly) and SPUR (city) URU (city)
45. SUKU (cultic room, chapel) SUC (ceremonial act)	
46. SUD (to depart, to sink)	SUT, SUTH (probably "to do" or "to be somewhere"); derived from SUTH-u (place, dwelling, grave)
47. TU, TE (to do, to make, to make)	TE-v (to show, to see?)
48. YOU, YOU (uaz)	TE (to operate)
49. YOU, YOU (uaz)	TE-s, TE-z (to take care of, to do, to do)

Note: these three Etruscan verbs are obviously derivatives of the Sumerian root TE (to do); cf. also Hungarian ten-ni, tesz, tev-ý and German tun (to do).

50. TIN, DIN (to live, life), DINGIR deity) TIN (, sun" and Tin Etruscan chief god name) 51. TEMEN (earthen mound, earthen mound) TMIA (sacred place or building: "temple"?) Note: Sumerian temples were built on ritually "cleansed" earthen mounds; cf. also Greek "temenos" (the consecrated area of a temple, usually marked by an earthen mound). 52. DUL (enclose, enclose) TUL-ar (boundaries) 53. DU, DU, (to place, to place, to hand over) TU-r (to give) 54. UD, UTU (sun, sun god), USI-1 (ýýý" and sun god) USU " (sunset) 55. BAR SI (knitting, shawl, headscarf) PHERSU (mask, masked person) 56. BAL (to sacrifice, pour out a libation) FLE-r (offering, sacrifice) 57. BAR (fire), AN (heaven) and TAG (to strike, to strike FRONTAC (lightning) down); thus in compound: BAR-AN TAG z , heavenly fire strikes down"

From the listed word correspondences, it can be concluded that very strong Sumerian linguistic influences appear in Etruscan as well: these 57 parallels represent more than 40% of the 141 known Etruscan common words. It is very important

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===== PAGE 864 =====	
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It is also interesting that, as the above glossary shows, in practically all cases where the meaning of individual Etruscan words is indeed known with certainty, the Sumerian meaning agreements are also completely uncontested, and the debatable parallels are almost always limited to Etruscan words of uncertain meaning.	
Let us now turn to the examination of the basic linguistic material of Latin. Research now unanimously agrees that the Latin language was massively influenced by Etruscan words, as well as other linguistic elements from other dialects of the Anatolian-Aegean-Tyrrhenian proto-language group. It is therefore not surprising, indeed obvious, that Latin also contains numerous Sumerian word parallels which, similarly to the Sumerian-Greek correspondences already presented, extend to the most elementary expressions, basic verbs and nouns denoting natural phenomena. Since we have already listed 50 close books in our Book I (pp. 164-165), we will now add another 50 correspondences to them.	
Sumerian-Latin word concordance (I. I.	
4 Szumer-latin szöphuzamok TUR- ris (tower) 53. KUD (cut off) (Assistant, Deputy)	4 szumer CAED-o (cut, cut off) 54. KUD (decide) latin 51. SIL, (to be unharmed) SAL-vus (whole, intact) 52. TAR (tip) de-CID-o (decide) 55. GUBUR FIND-o, perfect
FID-i (column) 58. MANA (a measure of weight MINA (mine), MANE-o (last), , __ and measure of time)	SILEN-tium (rest) 57. PAD (to chop up) MINU-tum (Minute) 59. WHERE (Hout)
CUT-is (Haut) 60. IGI (Auge) subjugate) 63. BURU (cattle)	OCU-lus (eye) 61. BIL (burn, fire), BIL-AM és PAG-o (catch, FUL-GUR (lightning) GIR, (lightning, flashing) 62. PAG (catching, imprisoning) POR-cus (pig) 64. BAR (bind) receive) FRE-no (bridle, tame) 65. GAB (receive, PEL-to (strike, push),
CAP-io (catch, receive) 66. MEN (diadem, jewelry)	MON-ile (collar) 67. BAL (beat) TER-o (thresh, grind)
FLI-go (beat) 68. GUL (smash, break) s-CUL-po (chisel), CAEL-um (chisel) 69. TAR (smash, dismember)	
DAY 70 (beat)	TANG-o, ólatin TAG-o (beat) 71. BAR (beat) 875 VER-BER-o (hit)

===== PAGE 865 =====	
72. BAR (separate)	PAR-io (trennen)
73. BAR (judge, Sprouse)	FRU-o (to thrive), VIR-ga (shoot)
74. BAR (burning of fire)	FER-veo (to glow, to burn) 75. BAR (to plead, to pray) PRE-cor (bites)
76. BAR (brew in)	FRI-go (voice)
77. BAL (turn)	VOL-vo (turn)
78. BAL (Spindle, spinning)	PLE-cto (to braid)
79. BAL (graben)	PAL (Spaten)
80. LEFT (left)	FAL-Jo (bring down)
81. BAL (becoming enemies, enmity) BEL-lum (war)	
82. GUB (bind, loop)	CAP-istro (halter, harness) 83. GAR (fence in, enclose) HOR-tus (garden); szöszerint
"Enclosed"	
DAY 84 (weaving)	TEX-o (weaving)
85. BATH (open)	PAT-eo (open)
86. HUL (to spoil, to despise)	CUL-pa (sin)
87. GIL, (running)	CEL-er (fast)
88. GIL, (light)	CAL-or (heat, embers)
89. GAL (a large jug)	CAL-ix (Kelch)
90. GUL (cut)	CAEL-o (carving)
91. GUR, (running)	CUR-ro (running)
92. GUR, (cut)	CUR-to (to shorten, to cut off) 93. GUR, (a measuring vessel) COR-tina (Kessel)
94. GIR, (the flash, Blitz)	CRE-mo (burn)
95. ZAL (bright, shine, glow)	GROUND (Sounds)
96. SIL (cut off)	SUL-co (cut in)
97. SILA, (a measuring vessel)	SOL-ium (Wanne)
98. SER, (Curse, Ban)	SOR-deo (to be despised)
99. SAR (cut off)	SER-ra (saw) 100. SUR (a vessel) SER-ia (Tonne, Fass)

90% of the presented matches clearly belong to the basic syllabary: they name the simplest actions and objects indispensable in everyday life at the Neolithic-Early Metal Age stage. This undoubtedly indicates that the Latin language, in its deepest basic layer, is an organic part of the pre-Indo-Germanic Neolithic-Early Metal Age Mesopotamian-Mediterranean language community, and through this it is also very closely related to the Proto-Asiatic-Sumerian linguistic koine.

From all this, it follows that the dialects of the ancient Anatolian-Aegean-Tyrrhenian language group must have contained a large number of words derived from the vocabulary of the Proto-Asian-Mesopotamian language koine, since it is obvious that the overwhelming majority of the above Sumerian-Latin and Sumerian-Greek word correspondences are not directly related to the Sumerian-Greek language.

===== PAGE 866 =====

\ abst was not included in Latin or Greek, but on the one hand from Etruscan, Ligurian and other ancient Italian languages, on the other hand from Minoan Cretan, the spoken language of the Mycenaean culture circle and the Proto-Hellenic Anatolian dialects. In all probability, the dominant influences of this more or less uniform Asia Minor-Mediterranean Nycivi base layer can also explain the fact that within the Western Indo-Germanic language groups / Greek and Latin are closest to each other,

Looking further west, we find ourselves in a more difficult linguistic situation in the Iberian Peninsula, because few traces of the languages spoken there have survived. All that is certain is that Basque is the descendant of an ancient Iberian dialect, and for its part shows close parallels with several Caucasian languages, and partly with Sumerian (cf. e.g. Bouda 1938, but also book 1. 6, pp. 846 and 848). This is not much, but it still allows us to attribute the Iberian dialects to the Anatolian-Aegean-Tyrrhenian language groups.

(1. e.g. Pallottino 1980, p. 35), and through this we can also connect it with the Proto-Asian linguistic koine. This classification is largely confirmed by the already described archaeological finds in Spain and Portugal, which testify that the cities built in 6 BC, as well as smaller or larger hinterlands, belonged to the interior of the mainland (1. pp. 780-781 and 809-810),
And indeed, if we take a closer look at the only deciphered text of the few known Iberian vessel inscriptions from the 4th-3rd century BC, the Sumerian parallel immediately becomes apparent. The inscription consists of two words: "gudua deitza", which can be clearly interpreted from the Basque language as "call to battle". Since the vessel depicts a naval battle, the translation is clearly linked to the image (1. Altheim 1951, p. 165). Well, in the Sumerian language "gu-de," = "call, roar, call out loudly", and the Iberian word "gudua" fits perfectly with this.

Let us then attempt to outline the paleolinguistic picture that can be developed from the contexts we have known so far. Looking back in time, the first, with some probability, more or less common language that can be grasped, expanded to larger areas, can be localized in the western half of East Asia; - more closely, it can be localized in the area of the Inner Anatolian (Hacılar, Catal Hüyük), Kitikta (Mersin, Tarsus) and West Syrian coastal (Amuq plain, Ugarit, Byblos) Coreolithic cultural circles. A broader date can be given; - that is, to the time of the consolidation of the mentioned cultures, the development of their closer relations with each other. This dating is supported by the archaeological finds of the region in question, which in the second half of the 7th millennium BC already showed widespread mutual

877

===== PAGE 867 =====

They prove contacts and regular contacts, and they are supported to a very high degree.
In such an ancient age and with the relatively low level of education at that time, we cannot of course count on a truly unified, commonly understood language being spoken across the entire vast area in the 7th millennium, but rather, initially, only a relatively narrow common vocabulary required by contacts, which could only include the most important expressions of everyday life. This ancient linguistic state, whose common vocabulary was undoubtedly dominated by the linguistic elements of the bearers of the most advanced Central Anatolian Hacılar-Catal Hüyük cultural circle at that time, can be called "Ancient Anatolian" or rather - in accordance with our terminology used in the Pre-Asiatic linguistic koine (1. of this book, pp. 708-713) - the Hacılar-Catal Hüyük linguistic stage. We can consider the boundaries of its spread to be identical with the area of the aforementioned Coreolithic cultural circles, and we can date its general use to the time of the flowering of these cultures, between about 6500 and 5000. During this 1500-year period, due to the rapid growth of the population and the ever closer relations, the linguistic equalization also took on ever greater dimensions, and thus in the 6th millennium there was undoubtedly a real Anatolian-West Syrian , we can speak of a lingua franca, the former existence of which is clearly proven only by the fact of the Aegean-Tyrrhenian language community originating in Anatolia.

This areally balanced Anatolian language community included the earliest Neolithic agricultural colonists of southern Europe, who appeared between 6000 and 5500 in Crete, the southern Balkans, and the Mediterranean coasts of Italy, France, and Spain (proto-Sesklo, Cardium pottery). Then, around 5000 — as we already know — the continuity of finds in the central areas of Anatolia was interrupted (cf. 6 book 672. yy.) and the bulk of the population migrated west and northwest, establishing, among others, the Kremikovi and Starévo-Körös cultures in the Balkans and the Carpathian Basin. The limits of the expansion of the Anatolian colonists can be drawn with the help of the Anatolian-Aegean-Tyrrhenian geographical names already mentioned (1. above, p. 846). In Asia Minor itself, only the western coastal strip (e.g. around Beycesultan), Cilicia and the easternmost region (the region of Elazığ) remained a significant population, and a part of the eastern Anatolian population seems to have occupied some areas of Transcaucasia (cf. e.g. Burney-Lang 1975, p. 73).

However, we must not forget that in the southeastern immediate vicinity of the Proto-Anatolian Communicative Language, in Syria and Northern Mesopotamia, the cultural and linguistic koine of the Samarra-Tell Halaf civilization developed starting around 5500. (1. of this book, pp. 709-710.), and around 5000 it had already spread to Eastern Anatolia and Cilicia. This, in turn,

878

===== PAGE 868 =====

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means that in the second half of the 1st millennium we must reckon with significant cultural and linguistic contacts between these two civilizations. Thus, the above-mentioned connections of the Unatdliai-Egel Tyrrhenian Os-language community can be traced, on the one hand, with the languages of the Eastern Anatolian Transcaucasian region AND the Laerian-Hepysian region (South Caucasian languages, Hurrian, Subir, Lullu, Gut, Kassu, Eldmi), and on the other hand = through the Halafian connections — with Sumerian (y. 847-849, p.), Approximately BC, 5000 and 3300 "About the core areas of the TEITHalaf, then This Anatolian-Mediterranean language originated and the late Ubaid civilization was repeated many times. During the waves of mass migration and the resulting ethnic conflicts, the mixing (these are discussed in T. 2 of this book)

head) again a very significant amount. Halafian and late Ubaidian (Early Sumerian) linguistic mother ovulated, i.e. primarily its vocabulary apparently increasingly approached the Proto-Asiatic language koine Halafian, then to the spoken language of the late Ubaid (pre-Sumerian) stage. Finally, ca. Starting from 3300 — the Sumerian urban culture was consolidated. The establishment of a trading house of the Sumerians in the "Greenlandic region, on the western European coasts and in some interior areas of Europe is also archaeologically highly probable, with a high degree of probability, of new pre-Asian, partly Mesopotamian-Sumerian ore prospecting, metal processing and trading colonists, who in many places, in the regions and around the ore deposits, established smaller and larger city-state-like power organizations, winning over or subjugating and organizing the indigenous population of a lower cultural level. The colonial groups, which also appeared in Europe in several waves between about 3200 and 2000, certainly brought the Sumerian language with them: either as their mother tongue, or as a common pre-Asian trade language of that time

language, which they were forced to use even if they did not come directly from the Sumerian-speaking area. Indeed, they could undoubtedly have joined the long-distance trade network organized by the Sumerians and maintained continuously for many centuries with the Sumerian language. Therefore, taking into account and carefully considering the relevant archaeological findings that can be counted, we must conclude that those languages belonging to the Anatolian-Aegean-Tyrrhenian language group, which Around the turn of the 2nd/1st millennium BC and after, we can more or less grasp (e.g., Minoan, Mycenaean, Etruscan, Ligurian, Iberian), in addition to the original Anatolian base layer, they were already largely saturated with the linguistic elements of the yyj- Asian Nyelian Koine, Halafi, Late Ubaid (Elo-Sumerian) and Sumerian stages, which were layered multiple times. In other words, the pre-879

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The migrations and ethnic mixing that occurred during the Asian cultural flood, which can be proven many times by archeology, provide an impeccable explanation for the massive Sumerian linguistic correspondences found in Ancient Greek, Etruscan and Latin.
However, in the ancient and modern languages of the areas surrounding East Asia from the east and north, we find just as massive Sumerian parallels as those we have seen above in Southern Europe. In this connection, we recall the numerous Sumerian-Hungarian-Finno-Ugric, Sumerian-Hungarian-Iranian and Sumerian-Hungarian-Altaic word correspondences presented as illustrative examples in our Book III, and the prehistoric and paleolinguistic conclusions that can be drawn from them (cf. pp. 448, 513-517, 529-532, 553-559, 574-575, 594-599 and 630-645).

In addition, we outline some of the methodologically satisfactory and almost generally accepted results of international linguistics in the field of research into the more distant connections and highly probable kinship relations of the Sumerian language. This time, we will not address the issues of earlier, mainly 19th-century studies of the parallels between Sumerian and the Uralic and Altaic languages, since, on the one hand, the current prevailing school of thought in international research generally rejects such language comparisons outright or considers them unfounded, and on the other hand, we intend to deal with this topic in more detail later on. Thus, we will now only present excerpts from those language comparisons from the 20th century that compared Sumerian with other languages and language groups and are generally considered well-founded in international linguistics.

G. Sigwart compared Sumerian with Etruscan, and derived Etruscan itself from the Caucasian languages (Etruskisches und Sumerisches, Glotta VIII. 1916). He pointed out the series of Sumerian parallels of Latin words that could not be interpreted from Indo-Germanic, which he attributed - very obviously and reasonably - to Etruscan origin. he explained them as loanwords in Latin. In addition, he showed the Sumerian equivalents of many of the Etruscan words with only a few known meanings at that time, as well as one or two morphological and syntactic parallels. All this indicates, he writes, that the Etruscans were in contact with the Sumerian culture in their ancestral homeland in Asia Minor, and that they adopted much from it. However, the correspondences are not sufficient to assume linguistic kinship (see Varga 1942, pp. 361-362).
Sigwart's findings in this direction were essentially confirmed by further research: B.Hrozny (1940) found Latin word parallels in Hittite, and J. Friedrich (1939) in Hurrian, which evidently entered Latin through Etruscan mediation (1. 92 and 424-425). But we have already seen above that Etruscan was in close contact with the Caucasian languages and Hurrian through the Anatolian-Aegean-Tyrrhenian language community (v6, vol. e. pp. 847-849).
880

===== PAGE 870 =====

F. Bor k (Das Sumerische eine kaukasische Sprache; Oricntalische Literaturzeitung, XXVII. 1924.) Sumerian correspondences found in Caucasian languages, especially in the field of general language structure and verb conjugation (1. Varga 1942, pp. 367-368.). K. Bouda examined the connections of the Sumerian language more broadly. He found correspondences indicating kinship between the West Caucasian languages &Sabask, which are partly also present in Sumerian, and at the same time occur in Tibetan. He lists a total of 87 word correspondences, almost half of which can also be found in Sumerian. In his conclusion, he cautiously suggests that in prehistoric times we may have to reckon with a vast proto-language-like formation, which also included Sumerian (1. Bouda 1938.).

Bouda's results mean that the ancient Anatolian-Mediterranean language community can be linked, through Sumerian, to the languages of the areas north and east of Mesopotamia, meaning that it would be a top-notch task for research to systematically examine the basic layer material of the ancient and modern languages of this region, as well as

as this has already partially begun in the case of Anatolia and the Mediterranean. All the more so because recent archaeological finds have factually proven that during the Pre-Asian cultural flood, Pre-Asian agricultural settlers appeared en masse in these areas, who obviously brought with them the dialects of their original homeland (cf. Chapter 2 of this book).

Some researchers have also dealt with the possibility of Sumerian-Indo-Germanic linguistic contacts. H. Hein, for example, wrote three studies on this question (Sumerisch-Indogermanisch, Altona 1919; Sumerer und Indogermanen, Mannus XI-XII. 1919—20; Die ältesten indogermanischen Sprachreste, Orientalische Literaturzeitung 1921). He compiled a long Sumerian-Indo-Germanic vocabulary list and evaluated the parallels as signs of former contacts and cultural influences (1. Varga 1942, p. 368). C. Autran supported the Sumerian connections of the Indo-Iranian group of Indo-Germanic languages in a much more definite form and with more thorough investigations (Sumérien et Indo-Européen. L' Aspect morphologique de la question, Paris 1925.). In addition to numerous word correspondences, he also showed the correspondence of some case endings and pronouns. Accordingly, he writes about the reserved position of researchers who proclaim the unrelatedness and isolation of the Sumerian language as follows; "Such an attitude may be professional and prudent, but from a scientific point of view it is not even half-satisfactory." (1. Varga 1942, p. 368) He finally summarises his results as follows: "... a) From the point of view of language: Sumerian represents in any case one of the elements that contributed a long time ago to the formation of the Indo-European language group; b) from the cultural point of view: Sumerian culture is to a large extent the basis of our culture." (see Varga 1942, p. 369). Consequently, he took the position that Sumerian was an ancient linguistic complex.

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xum should be considered as a basic language that goes back to the distant past, and its effects are clearly recognizable in later languages. We can see that already in the 1920s, Autran confidently pointed out the real roots of the Indo-Germanic languages, and even specifically the most important, oldest common basic layer of those ancient language complexes, which modern Indo-Germanic studies only began to take into account much later in their paleolinguistic studies. It is characteristic, however, that even today the development of the Indo-Germanic languages is explained only by the "Finn-Ugric", "Caucasian" and "Equator", starting sometime at the beginning of the 2nd millennium BC (cf. pp. 407-427 and 549-550), without even taking into account the generally widespread commercial, cultural, cultic and scientific language that prevailed throughout East Asia at that time, i.e. Sumerian. Although you yourself realize that the common ancient source of the linguistic parallels connecting Indo-Germanic with Finno-Ugric and Semitic languages must probably be sought in some "third, unknown language" (cf. p. 414).

A classic example of this — methodologically completely incomprehensible — conceptual confusion is V. Christian's study "Die sprachliche Stellung des Sumerischen", in which he essentially arrived at findings identical to Bouda's previously mentioned results, but from these he drew conclusions of diametrically opposite sign. Christian collected Sumerian correspondences found in the Caucasian, Hamito-Semitic, Uralic, Altaic, Sudanic and Tibeto-Burman languages, and, reversing the actual linguistic, cultural and historical chronology, he explained these linguistic connections by saying that Sumerian was a language mixture that was grammatically and structurally Caucasian, in vocabulary and word formation Hamito-Semitic, and in phonology and language type Tibeto-Burman, Uralic-Altaic and African Sudanic languages, sometime in prehistoric times, well before the 3rd millennium BC.

It is hardly necessary to refute these absurdities in detail. It is sufficient to point out that, according to the archaeologically proven pre-Asian cultural influences, migrations and colonizations presented in this book, the partial Sumerian linguistic parallels found by Christian, spanning three continents, undoubtedly originate from Sumerian and not vice versa. Ultimately, therefore, by searching for the connections of the Sumerian language that can still be found in distant regions, Christian in this work unintentionally contributed to the fact that the investigation of the more distant influences of the Sumerian language could once be started using objective methods.

However, we must briefly address another aspect of Christian's study in question - also fundamentally unmethodical. In this

882
===== PAGE 872 =====

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In this paper, for the first time, formulated in detail and systematized to a certain extent, appears the view that, based on "phonetic considerations" (this learned phrase is already well known to us yyyyyyy-s), attempts to derive numerous Sumerian words from the Mesopotamian Semitic languages, Akkadian and Assyrian, which appeared much later. This trend, which is in every way the equivalent of the already known "ahistorical" school of Indo-Germanic studies, later developed into an entire school in Semitology (cf. what has been said about the "proto-Akkadian" theory; 1. pp. 706-708), despite the fact that the Sumerian linguistic monuments are in the Akkadian language
They appear in inscriptions a full millennium earlier, and cultural-historical and archaeological facts clearly prove the absolute primacy of Sumerian culture over the Akkadian by well over a thousand years. In other words, the real situation was exactly the opposite: apart from a smaller number of late Semitic loanwords and a few so-called "Akkadianism", which were incorporated into Late Sumerian in the second half of the 3rd millennium and the beginning of the 2nd millennium, during the period of close Sumerian-Semitic coexistence and language mixing, in the last centuries of the living Sumerian language, the Akkadian-Babylonian language is teeming with Sumerian loanwords, even elementary expressions, in the field of the so-called basic vocabulary - which the vast majority of researchers have always recognized, and still recognize today. During this period, thousands of Sumerian words were incorporated into the Old Semitic Akkadian-Babylonian vocabulary, in a more or less distorted, pidginized form, their phonetics adapting to the characteristic phonetics of the Semitic languages. Moreover, during this process, Akkadian itself became significantly assimilated into Sumerian, especially in the field of phonology (1. pl. Bottéro: Das erste semitische Grossreich, Fischer Weltgeschichte 2. Band).
In his essay "Beiträge zur sumerischen Grammatik" (1957), Christian further complicates these illogical threads. From the well-known fact, for example, that words with similar meanings in Sumerian are usually built on identical consonant frames, and that vowel changes often only vary semantically related meanings, he immediately draws the conclusion that since according to this the basic meaning adheres mainly to the consonants, one of the ancient components of the Sumerian language must therefore have been close to the Hamito-Semitic languages, in which the consonant frame also determines the meaning of the word (1. Christian 1957, p. 12). Aldit does this despite the fact that, according to common knowledge, the meanings of the words of most languages adhere to the consonants, and at the same time, Semitic vowel changes (the so-called symbolic vowel changes) - just like Indo-Germanic "Ablaut" — on the contrary, they do not serve to form new words with related meanings, but have practically only a grammatical and internal word-formation role. If the situation were as Christian claims, then the Semites could not have written only

883
===== PAGE 873 =====

consonants, because the meaning of the words would become uncertain if the vowels were omitted. For example, when an Arab reads the consonant cluster ktb, he immediately knows that it only denotes the concept of "to write", not "something similar", and that the different vocalizations merely vary the distinct grammatical forms of the verb "to write": KaTab = "schreiben", aKtuB = "ich schreibe", KuTiBa = "wurde geschrieben", KaTaBa = "schrieb", KaTiB = "Schreiber", ma-KTuB = "geschrieben" (cf. also Book I, p. 133). - In contrast, it is characteristic of inflectional languages — such as Sumerian — that vowel changes are used to form word variants that differ to some extent from the meaning of the base word, but are semantically related to it (cf. e.g. Hungarian lik or luk, lék, lak; also 1. Book I, Chapters 4-5). - In other words, Christian argues with something that can hardly be called anything other than a conscious misrepresentation. Based on the above, the paleolinguistic facts directly testify that the situation was exactly the opposite: it was not that Proto-Sumerian was "close" to the Hamito-Semitic languages, but that a series of Sumerian linguistic characteristics were transferred to the latter in ancient times, but only in a stunted, distorted and pidginized form.

A very promising research trend has recently emerged with a more in-depth study of the Sumerian relations of the Indian Dravidian languages. This is particularly important for us because, on the other hand, as the American researcher MA Durbin writes, SA Tyler recently succeeded in proving the genetic relationship between the Dravidian and Uralic languages by systematically comparing 153 word pairs (Tyler 1968). On the other hand, starting from Tyler's results, Durbin also included the question of Sumerian-Dravidian linguistic relations, which had been debated since 1913, in his studies and — as he himself emphasizes 28 — right from the start, with only two hours of searching in the Sumerian and Dravidian dictionaries, he found 16 similar word pairs. Consequently, he considers the relationship between Sumerian and the Dravidian and Uralic languages to be a serious possibility (see the above in I. Durbin 1971). The importance of these studies is also greatly enhanced by the fact that in recent years the language of the bearers of the Indus culture has also begun to be revealed. It has been possible to decipher about 100 of the 419 syllabic signs of the Indus script, using the homonyms found in the Dravidian languages, i.e. words with the same or very similar sound but different meanings, by applying the so-called "rebus principle". This means that the population of the Indus civilization spoke a Dravidian language (1. Fairservis 1983). Thus, the eastern and northern connections of the Sumerian language are gradually coming into the field of international scholarship, and we can rightly hope that a wider range of studies will soon begin.
paleolinguistic studies involving Sumerian,
The theoretical and methodological basis for solid, scientifically demanding language comparisons is that Sumerian shows more or less similarities with many Eurasian languages, even at first glance.
obvious at a glance — match, pártu-884

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zamot; > mainly in the field of vocabulary, but also several times in grammar. This fact should not come as a surprise to our readers, given the results of our study series so far, since we already know that the over-proliferated ethnic groups of the 6si 6 Asian primary cultural circles continuously and in large numbers migrated to the surrounding nearer and more distant regions over the millennia, and in almost all of Western Eurasia they exerted decisive cultural and ethnogenetic influences on the sparse native populations of the far lagging behind culturally in areas (cf. Book I, Chapter 1 and Chapters 1-2 of this book).

In the light of these new prehistoric discoveries, it is of course no coincidence that the same human race, the so-called "Caucasian-Europid" or "great race", dominates almost all of Europe and Western Asia. This undoubted fact, beyond the archaeological evidence, clearly demonstrates, even in our time, on the level of anthropology, that the population of these areas, which can be considered relatively uniform from an anthropological point of view, ultimately has a largely identical origin, mostly originating from the populations of the pre-Asian primary Neolithic and early Metal Age civilizations, and developed in its present form through repeated, overwhelmingly pre-Asian admixtures.

It can therefore be expected that more or less massive Pre-Asiatic linguistic influences, superstratifications and admixtures will also be detectable in the most diverse languages of Western Eurasia, in close parallel with the massive Pre-Asiatic admixtures that can be established archaeologically and in many places anthropologically, in almost any part of the region.

Since Sumerian was very likely the dominant living cultural language in all respects in the Stone Age and Early Bronze Age Near East for about 2,500 years (between about 4,500 and 2,000), and even if calculated minimally — undoubtedly and demonstrably — for two millennia (from about 3,800 to 2,000), and then for another 1,500 years it played the same role as medieval Latin in Europe, it can be assumed with a probability approaching certainty that in the case of these more distant linguistic connections we must first and foremost reckon with the influences of the Sumerian language. This is also due to the fact that the Sumerians developed the first truly high culture on Earth, maintained an organized long-distance trade network spanning far-flung regions for many centuries (1. pp. 772-773), their religious, mythological, cosmological ideas, symbols, artistic motifs, outlook on life, superstitious customs, legends, and tales were universally spread throughout East Asia without a trace, and can even be found in large numbers in most of Western Eurasia today (le book, chap. 4). Furthermore, they organized the very first states, and then, starting from about 3200 BC, empires that encompassed the whole of Mesopotamia (1. pp. 748-750), i.e. the Sumerians were the first peoples to form a world-885

===== PAGE 875 =====

It was the first language in history to be used uniformly across large areas as an imperial, administrative, commercial, cultural and, last but not least, cultic language. In other words, it was during the era of the Sumerian hegemony in Pre-Asia that the basic conditions were first realized in the history of humanity, which, according to the results of recent paleolinguistic research, are indispensable for a high degree of linguistic equalization to occur with an effect on larger areas, which would enable the development of a truly unified administrative, commercial, literary, scientific and cultic language (cf. e.g. Altheim 1951, p. 2; ~ cited pp. 408-409).

These archaeological, ethnogenetic, cultural and linguistic data also reasonably explain the fact — commented on only sarcastically by our Hungarian linguists and paleontologists — that Sumerians are fond of emphasizing it, without considering that this superficial "it has been compared with almost every language in the world"; — as most of our authoritative researchers point out as a frightening example judgment only testifies once again to the shocking one-sidedness of the academic trend: that the Finno-Ugric school, far from examining new archaeological and paleontological discoveries, does not even take them into account. We could not undertake more in presenting these extremely significant proto-linguistic connections this time, because a detailed description of the effects of the Proto-Asian linguistic koine on all the vast areas reached by the Proto-Asian agricultural settlers and mineral-exploring colonists would literally explode the framework of this volume. This is enough, however, to show what a paramount methodological requirement it would be for paleolinguistics to finally pay attention to the extensive Proto-Asiatic connections revealed by archaeology in recent decades in the southern half of Europe, but also in other areas surrounding Proto-Asia, and in the future not only to struggle with the fruitless task of deducing hypothetical proto-language forms (the futility of these attempts has, incidentally, already been recognized by modern Indo-Germanic studies; I. pp. 407-427), but to primarily base its studies of language formation and development on the ancient Sumerian linguistic monuments, which are now available in the tens of thousands. This would place paleolinguistics on a much more concrete foundation, and researchers would be able to draw much more certain conclusions about the nature of past linguistic relations, as well as the development and evolution of modern languages, thanks to the linguistic evidence-based support of the recognized parallels.

886
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889

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The eight maps at the end of Volume 2 come from the work of D. und. R. Whitehouse "Lübkes archäologi-
scher Weltatlas" (1976). The book was originally published in English.
It was published in 1975 by Thames and Hudson Ltd., London, under the title "Archaeo-logical Atlas of the
World".

- 1) Earliest farmers in Western Asia (I. Whitehouse 1976, pp. 62-63)
 - 2) Neolithic and Chalcolithic cultures in Mesopotamia (I. ibid., p. 67)
 - 3) Neolithic sites in Eastern Europe (I. iott, pp. 136-137)
 - 4) Early coppersmithing in Europe (I. iott, pp. 156-157.)
 - 5) Western Anatolia and the Cyclades: from the Neolithic to the Bronze Age (I. iott, p. 91)
 - 6) Anatolian Neolithic to Bronze Age (I. iott, pp. 72-73)
 - 7) Megalithic structures in Europe (I. iott, p. 149)
 - 8) The cultures of bell-shaped vessels and the spread of battle-axe cultures, as well as the spread of globular worms (I. ibid., p. 159)
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890
===== PAGE 880 =====

BOOK V
ON THE CONTRADICTIONS OF THE THEORY OF THE GENETIC LANGUAGE FAMILY OF THE URAL-FINNO-UGORIAN LANGUAGES
===== PAGE 881 =====

Chapter 1
HYPOTHESIS OF THE PRIORITY OF EUROPEAN Neolithic OVER PRO-ASIA

A few words about radiocarbon dating
During a thorough examination of the annual rings of the long-lived Californian pinyon pine, it was noticed that the absolute archaeological age determinations obtained by measuring the radiation intensity of the radioactive
carbon isotope with atomic weight 14 — that is, the measurement results of the n. C* method — could not be reconciled with the age of the samples determined using annual ring studies, i.e. with the so-called
dendrochronological age determinations.
It turned out that the reason for these differences was to be found in the temporal fluctuations of the C* content of the air. This undermined one of the most important pillars of the ýý" method, which was that the constancy of the
C* content of the air was assumed from the outset and the measurements were naturally carried out based on this assumption.
Following this recognition, Hans E. Suess developed a graph of this dendrochronological correction of C* measurements back to 5000 BC — this operation is the so-called dendrochronological calibration — according to which
the older a material containing C* is in relation to BC, the more years we need to add to the result of the C* measurement obtained by the traditional method in order to obtain the real age of the sample in question. This
difference exceeds 800 years in the 4th-5th millennium BC.

From the birth of Christ to 5000 BC, we can convert the C* measurement results obtained by traditional methods to the so-called true ages obtained by dendrochronological calibration using the following formula: T
(True age = true age in calendar years) = 1.4 R (= radiocarbon age) - 1100. So: T = 1.4R-1100 (I. Narr 1975, p. 22)

However, in addition to this first general — systematic — source of error in the CV method, there are also other sources. The most important of these are the following:

- 1.) Errors or inaccuracies in measurements or measuring instruments.
- 2.) Contamination of the samples examined.
- 3.) Wood, bone, peat, etc., according to experience, do not have sufficient reliability in terms of measurement results; we must be very careful especially with age determinations obtained from the shells of mollusks, which may
even differ between certain freshwater and saltwater species.
- 4.) The half-life of the radioactive C* atom is also uncertain. Initially, it was 5568 years

893
===== PAGE 882 =====
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half-life has been calculated, more recently mostly 5830 years, which will
This represents a difference of 3%. (For the above, see I. Narr 1975, p. 22).

These corrections lead to serious discrepancies, especially in the ancient Egyptian absolute chronology, compared to previous age determinations, i.e. in the chronology of that culture, for which prehistoric
research has ancient written sources in the form of the so-called Sirius periods, and what is more, astronomically verifiable and provable absolute chronological data.

From this single concrete example alone, it can be unequivocally stated that for the time being, such, often extremely high C* dates must be treated with extreme caution — with particular regard to previous
experience with C* measurements — Narr states (I. Narr 1975, p. 23).
Applying this dendrochronological calibration to the megalithic archaeological cultures of Western Europe - especially those of Brittany - some researchers believed that some megalithic tombs did not date from around 3000 BC
or even later - as archaeology had generally dated these cultures until then. The measurement results also seemed to contradict the traditional archaeological-prehistoric belief that —, but from around 4500 BC. These
the megalithic cultural complex was ultimately
It spread from the Middle East to Western Europe; first to the island of Malta, then to the Iberian Peninsula, then to the western coasts of France, and finally from there to England, Ireland, Northern Germany and
Scandinavia. The results of the French — Brittany — C* measurements showed that they were older than the Iberian ones.
Again about C* measurements

It is also worth mentioning that the boundary that Renfrew marks as the endpoints of the furthest west-northwestward expansions of the pre-Asian cultures, in his opinion, is practically identical point by point with
the other boundary along which another very important source of error, the so-called "C" measurement results, appears significantly.
A "C" fault line runs through this zone "fault line" runs along the northern border of European Turkey and then through the northern provinces of Greece
in a semi-circular shape and means that the vast majority of C* measurements to the north and west of this zone (but by no means all, because there are also quite a few "normal" measurement results) result in dates
1000, and sometimes even 2000 years earlier than archaeology would expect based on the numerous specific pre-Asian — European stratigraphic connections and the clear stylistic parallels that can be demonstrated in
many cases in the find complexes. At the same time, however,
894
===== PAGE 883 =====

The results of the C¹⁴ measurements in the West Anatolian region, directly bordering the aforementioned belt from the east, but also in all of East Asia and Egypt, correspond to the stratigraphic relationships and to the traditional chronological determinations based on them, although considerable uncertainties occur in 6 regions (1. below). The cause of this C¹⁴ fault line has not yet been satisfactorily explained. (See above 1, Makkay: Sumerian Signs from Transylvania? and Makkay: The Whiskey Test.) It is therefore quite obvious that Renfrew's supposed most distant pre-Asian cultural emanations are not due to this ominous coincidence, but to its striking coincidence with "break line C" cannot be undoubtedly this mysterious "fault line" also played a role in Renfrew's ideas about Western European cultural priorities. Going further, however, there can be no doubt that the mere existence of "fault line C" alone absolutely requires an exact natural scientific explanation, since the "C" method also claims scientific exactitude. And only if we can explain the cause of this "break line" satisfactorily in a scientific manner, and this explanation proves to be compatible with the 1000, and sometimes even 2000-year age differences suggested by the C¹⁴ measurements in the relations between the archaeological cultures of Europe and Near East Asia, which can otherwise be paralleled many times and existed simultaneously according to all other data at our disposal, can we build the absolute chronology of the European Neolithic and Early Metal Age cultural development solely on the results of the C¹⁴ measurements.

Colin Renfrew's theory of the autochthonous European origins of the Neolithic
Colin Renfrew, who at the time was teaching at the University of Sheffield (and later took over the head of the Department of Archaeology at the world-famous University of Cambridge), also started from these C¹⁴ data in the second half of the 1960s and developed a global hypothesis that the megalithic cultures of Western Europe were autochthonous local developments, and even much earlier than most of the civilizations of the eastern Mediterranean, such as the cultures of Egypt, Cyprus, Crete, and Greece.
Renfrew also claimed at the same time that the cultural influences of the ancient cultures of the Near East had spread west and northwest only as far as Egypt, Greece, and the southeastern Balkans, and that the archaeological cultures that are located beyond this border to the west and northwest were all local developments — or rather, they were emanations of the megalithic cultures of Western Europe to the east. (I. e.g. Renfrew 1967, 1969, 1970.) We can see that Renfrew published these assumptions between 1967 and 1970. It is characteristic, however, that in October 1971 — in an article published in the 10/1971 issue of Scientific American — arrol pa-895

==== PAGE 884 =====
It is unfortunate that archaeologists and Östörtänesz's colleagues do not acknowledge these new discoveries, but continue to use the old absolute age determinations.
For shedding more light on the circumstances surrounding the origin of Renfrew's hypothesis, we would like to express our gratitude to our dear friend László Rajkay, who lives in Schwabmünchen, who was kind enough to compile the above details for us.
Well, as a result of our own literature research on this issue, we can only say the same thing that Renfrew himself noticed in 1971; namely, that Renfrew's hypothesis outlined earlier has no serious resonance in the recent archaeological and prehistoric literature available to us. Thus, for example, in the IV. The professional works published after 1970 in the bibliography of our book mostly do not acknowledge Renfrew's hypothesis, and the author who mentions it immediately and unequivocally rejects it, primarily on the basis of the objective sources of error of the C¹⁴ method that have not yet been explained, and on the other hand with the completely convincing and obvious argument that the few ancient graves found under or next to some of the many thousands of megalithic monuments cannot in any way be considered evidence that the megalithic structures in question were erected at the same time as the burials in question. This view is also supported by the archaeological fact that has been observed thousands of times, and with a very high degree of probability, that the graves excavated inside megalithic dolmens are mostly from the Copper Age, and some even belong to the Bronze Age. (see Müller-Karpe 1974, (pp. 266-294)

In accordance with this methodological situation, the most representative, internationally generally recognized works of the relevant archaeological and prehistoric literature — as we have briefly mentioned above — mostly remain silent about both Renfrew's hypothesis and the similar theories of some of his followers — here we should note in passing that Renfrew's initiative in this direction soon found a lively response in certain circles — or they categorically reject such ideas.

KJ Narr's position on Renfrew's hypothesis
For example, the position of KJ Narr's handbook "Handbuch der Urgeschichte" on this issue is very typical, which he briefly summarizes at the beginning of the discussion of the Iberian Neolithic and the megalithic monuments there, in the preface to the relevant chapter (1. Narr: Handbuch der Urgeschichte, IL Band Jüngere Steinzeit und Steinkupferzeit, p. 505). But it is clear from the text that he only describes so much about this topic because
896

==== PAGE 885 =====
to explain why his manual does not deal with these assumptions further.
Narr first mentions that recently some people have questioned whether the so-called "colonies" (meaning: Middle Eastern colonies) in the Iberian Peninsula would indeed be the results of cultural and ethnic radiations from the Eastern Mediterranean. He then outlines the unfoundedness of the assumption. He emphasizes that even the few C¹⁴ data that the proponents of this hypothesis mainly refer to are not clear, because there are also early C¹⁴ measurement results in the Eastern Mediterranean that are completely consistent with the Iberians. Of course, we also have to correct (i.e. calibrate dendrochronologically) these Eastern Mediterranean C¹⁴ dates — he notes in this regard. Narr's last remark is extremely important (that is why we have highlighted it) — we will find out why below. Consequently — in order to eliminate any possible conscious or subconscious misunderstandings — we include the relevant original text here in a verbatim quote: „Die Radiokarbon- daten, auf die man sich dabei in der Hauptsache beruft erscheinen allerdings bislang nicht zwingend: Einige weinge Messungen im Bereich der Iberischen "Kolonien" (2345-55, 2430+120 und 2210+110 v.Chr.) {würden zwar nach der Korrektur} ("Eichung": vgl. S. 22 f.) früh sein als die Daten, die für das östliche Mittelmeer aus Verbindung mit dem ägyptisch-vorderasiatischen Raum zu gewinnen sind (cf. S. 553); indes würden damit zwei ungleichwertige Datierungsverfahren verglichen; und bei der Heranziehung von Radiokarbon daten aus dem östlichen Mittelmeer ist nicht zu übersehen, dass auch für die mittlere frühhelladische Periode (Frühhelladisch II) neben jüngeren (zB 2262+56, 2120+65 und 2027359) einige Daten vorliegen (zB 2431+58 v. Chr.), which correspond to the western "Colonies" (and wie diese zu noch unklar, ob nicht comprable erscheinungen auf den Kykladen früher einsetzen als auf dem griechischen Festland." 1. Narr 1975, p. 505.)

correct" are). Furthermore, is

In Hungarian: "However, the radiocarbon data, which are mainly referred to in this question, do not seem convincing at present. Some measurements in the Iberian "colonies" area (2345+85, 2430+120, and 2210+110 BC) would, after correction (authentication; calibration: cf. pp. 22 and k.), be earlier than the dates that result from the connections for the eastern Mediterranean with the Egyptian-Pre-Asian region (cf. p. 553); however, we would thus be comparing two dating methods of unequal real value; However, if we also consider radiocarbon dates from the eastern Mediterranean region, it immediately becomes apparent that they are from the middle part of the Early Helladic period (so-called Early Helladic II) alongside younger ones (e.g.

897
==== PAGE 886 =====
2262+56, 2120+65 and 2027+59) we also know some dates (e.g. 2431+58 BC) that correspond to the dates of the western "colonies" (and like the latter these must also be "corrected"). Furthermore, it is not even clear whether comparable phenomena do not appear earlier in the Cyclades than on the Greek mainland.
Végül pedig Narr leszögezi: "A simple reversal" of the previous assumptions does not yet constitute a satisfactory solution. (Among other things, it presupposes the independent invention of copper metallurgy on the Iberian Peninsula—which is certainly not impossible in itself—and must generally assume diverse independent origins for other elements as well.) Proof of an autochthonous origin of "the colonies" cannot be provided in important respects, although a common African-Southwest European substrate can readily explain some things. The reversal, according to which some of the common features reached the eastern Mediterranean from the west, is not followed here in the relevant article (pp. 545 ff.); however, it should nevertheless be mentioned as one of those newer hypotheses—which result from an—admittedly partly selective—acceptance of the (corrected) radiocarbon data." (1. Narr 1975, 505. old.)

In Hungarian: "A simple reversal of the previous ideas does not yet constitute a satisfactory solution. (This reversed approach presupposes, among other things, the independent invention of coppersmithing on the Iberian Peninsula — which is certainly not impossible in itself — but it must also assume the independent emergence of numerous other cultural elements.) The autochthonous origin of the "colonies" cannot be justified on several important points, although a common African — Southwestern European base layer would explain certain phenomena without strain. The reverse theory, according to which some of the cultural phenomena corresponding to each other reached the eastern Mediterranean region from the west, is not discussed in the relevant chapter of this work (pages 545 et seq.), but we mention it nevertheless as one of the new hypotheses that result from the acceptance of (corrected) radiocarbon datings - partly selective (our emphasis) of course.

In the same sense, on page 298 of volume II of Narr's same handbook, the co-author of the handbook, the renowned French archaeologist Jean Arnal, also writes about the assumption of the autochthonous origin of the Neolithic Age in Brittany and the megalithic culture there: "For Brittany, PR Giot speaks of a "primary Breton Neolithic" and denies the existence of the Chasséen in this area. We cannot agree with this, since the basic forms present here are more likely to be attributed to a group or side-effect of the Chasséen. (Radiocarbon dates lie in the same order of magnitude as those of the other Chasséen: e.g. Kercade 3890+300 BC, Barnenez 3800+150 BC)"

898
==== PAGE 887 =====
\\ In Hungarian: "In the case of Brittany, P, R, Giot speaks of a "primer Breton djökör"- N and denies the existence of Chasséen in this region. However, we cannot accept this, because the basic forms occurring here rather belong to a group or subsidiary phenomenon of " Chasséen. (The radio | carbon dates are also of the same order of magnitude as at other Chasséen sites; e.g. Kercado BC 3890+300, Barnenez BC 3800+150)"
The "Chasséen" mentioned in the quote, or more precisely the "Chassey-Lagozza-Cortailled cultural complex" is the name of the most widespread cultural circle of the middle phase of the Western European Neolithic, and dates from approximately 3800 to 2400/2300 BC. The Early Neolithic in the Western European region is represented by the Cardium-Kerdmia or Impresso-Kerdmia (i.e. impressed pottery) groups from the 6th-5th millennium BC, which are, however, only found in the southern, Mediterranean coastal regions of present-day France and Spain, while in the more northern areas the Cardium-Kerdmia era was still Mesolithic, and the Neolithic in the more northern region only begins around 3800 BC.
Chasséen"-nel

Giot, the only one mentioned by Arnal in this context, is therefore clearly one of the most famous representatives of the autochthonous Breton Neolithic hypothesis. It is particularly characteristic, however, that the same PR Giot, who serves as the editor, is remarkably cautious in his own hypothesis on page 35 of the In the study "Menhire und Dolmen", which serves as information for tourism in Brittany booklet he edited. He does not even mention the assumption of the priority of the megalithic buildings in Brittany, but rather explicitly refers to the fact that they may have had predecessors elsewhere, which were several centuries older." Then, below, he approaches the measurement results of :C" with the same "glove hand": technical reasons for using only data "before today" and for avoiding the reference to "Christ's Birth", which would include comparisons with historical data.

In Hungarian: "... For technical reasons, it is advisable to only talk about "pre-modern" dates here and "For the Birth of Christ" it is better to avoid comparisons, because this would mean comparisons with historical dates." The attentive reader can therefore notice for himself: the representatives of such theories really refer mainly and primarily only to the C" measurements, and even from these they only pick out the raisins that can be inserted into their hypotheses, as we have seen this extremely important moment from the Narr quote above. In addition, it is also an integral part of their "method" that in a European context, without exception, they always publish dendrochronologically calibrated C" dates - i.e. several centuries, sometimes almost a whole millennium older - and these are used as a basis for their own theories.

===== PAGE 888 =====

The fantastically high age determinations are tacitly compared with the uncalibrated Near Eastern C" measurement results. In this regard, we must be aware that the international archaeological literature - precisely in view of the so far unexplained sources of error of the C" method - by common agreement publishes traditional "C" measurement results (cf. e.g. Narr 1975, p. 25). Thus, the insufficiently informed, less expert always prefers the uncalibrated Thessalian Proto-Sesklo culture, which already original, i.e. the readers do not even notice the sloppiness, when the supporters of the hypothesis of the priority of the European New Age, e.g. The lowest layer of the early Neolithic contains pottery, with an uncalibrated C" age of 5800 BC, is simply dated to 6600 BC without any reference to this (this is the calibrated age of the layer) and in this way they try to "prove" the primacy of Thessalian pottery over the pottery of the Anatolian Hacilar settlement, which is similar to Thessalian pottery up to the point of articulation, and whose lowest layer dates to around 6000 BC — but of course without calibration.

Narr also referred to precisely such manipulations when he noted in the above quote that if we want to make absolute chronological comparisons between the ancient cultures of the Iberian Peninsula and the eastern Mediterranean, then we must also correct (i.e. calibrate) the C" measurement results of the eastern Mediterranean region with the help of dendrochronology, and not just the data of the western C" measurements.

In connection with all this, however, it must be mentioned that this is a complete confusion of the absolute ages of the aforementioned "method" can also be applied indirectly, effectively" archaeological cultures, and an almost arbitrary change of their actual relationships to each other. It is not necessary to resort to such open falsifications as we mentioned earlier in the relationship between Thessaly and Hacilar. It is more than enough if the interested lay or self-taught readership is filled with their own European C" dates, which are always published in a calibrated manner, and therefore appear to be extremely old. In this way, over time, readers become "familiar" with such extremely high dates of European Neolithic and Early Metal Age cultures. If in the meantime they come across one or two prehistoric or archaeological works on Near Eastern subjects, they involuntarily compare the — of course uncalibrated — C" measurement results found in them (we mentioned that the international archaeological literature uniformly uses uncalibrated C" measurement results) to the already well-known and "usual" extremely old European C" dates, not suspecting or noticing that the Near Eastern dates always involve uncalibrated C" measurement results. This optical illusion then often not sufficiently informed in the field of archaeology

900
===== PAGE 889 =====

leads the vast majority of the readership to accept these ideas about the primacy of the European Neolithic over that of East Asia. Again, only about the C" measurements As for the actual evidential value of C" measurements, or rather their degree of accuracy, we ourselves have individual experiences in this methodological issue. These experiences also led us to immediately and without any hesitation accept the unanimous and clearly expressed negative opinion of the professional works we studied regarding hypotheses that are primarily and mainly based on C" dates. We know well how unreliable the measurement results of such radioactive microradiations are.

During my university studies, as a so-called demonstrator at the Institute of Biophysics, during the muscle metabolism studies conducted at the Institute, we had the opportunity to become familiar with the method of measuring the radiation intensity of the radioactive isotopes of potassium and sodium. The method was therefore, in principle — but also in practice — largely identical to the method of C" measurements, even in that it was just as much a micromethod as the latter. Well, experience showed that, first of all, the measurement results were only comparable and evaluable if the entire series of experiments was prepared and carried out by the same person. This is natural — upon closer inspection — since everyone has slightly different working practices (e.g. the method of pipetting, etc.), which slightly influences the result at each stage of the experiment.

These small inaccuracies then add up during the experiment and can ultimately result in significant differences. However, this is only insignificant in the case of a single experimenter, because the same person makes his or her own usual so-called "standard error" at every experimental moment, and this always results in the same "summary standard deviation" when the experiments are repeated several times, the consideration and correction of which then does not pose a serious problem in the final evaluation. However, if each experiment is carried out by a different person each time, then these different collaborators all introduce their own separate, different "standard errors" into the series of experiments, and these various errors usually result in significant differences that are very difficult to eliminate.

Secondly, it must also be taken into account that the working hypothesis itself, which is often formulated in advance, can strongly influence the measurement results in a given case, even unconsciously: it is a well-known psychological phenomenon that in doubtful cases, people are more willing to interpret individual data in favor of the subconsciously expected result than

901
===== PAGE 890 =====

despite that. And such dubious cases - let's remember: we are talking about microtechnology! -- are indeed quite common. We can see very good concrete evidence of these internal, laboratory-technical uncertainties and sources of error when we take a closer look at the C" measurement results of samples obtained from the Coreanolithic settlement of the site called Oa!at Jarmo (Dzsarmo) located in the western foothills of the Zagros Mountains in eastern Iraq. The American research team that excavated the site - led by RJ Braidwood - could afford the "luxury" of having numerous samples examined with the C" method, sometimes several from the same layer, the ages of which were then determined in several different laboratories. KJ

Narr's "Handbuch der Urgeschichte" (Handbook of Prehistory) volume II, page 24, presents twelve such Jarmo! C" measurement results in a summary table. The capital letter abbreviations and numbers in the first column of the table indicate the laboratory where the CS age of the sample in question was determined, as well as the laboratory serial number of the measurement; the Roman numerals in the second column indicate the stratigraphic section, and the Arabic numerals within the stratigraphic section indicate the individual layers from which the sample in question comes, supplemented by Früh" (early) and the "Oben" (above), "Mitte" (middle), , "Basal" (base) with clarifications, which also refer to the position of the samples in the strata.

The table presents the following picture:

1.) C 744 II, 1 Above 5266+450 BC 2.) W 607 BC 5.) C 743 BC 9.) II, 4 Send 9040+250 v. Chr. 3.) W 651 8830+200 BC 4.) F44 II, 5 Middle 11400+300 BC 6650+170 in.
W 652 BC 6695+360 v. Chr. 6.) W 665 795+200 11200+200 v. Chr. 7.) W 657 8.) H 491 I, 7 Early 6606+330 BC 12.) F45 I, 8 Basal 6575+175 v.
v. Chr. 10.) C113 67073320 v. Chr. 11.) C742 6570+165 v.

First of all, it can be clearly seen from the table that the measurement results obtained by the individual laboratories differ irreconcilably from one laboratory to another in almost every case. Particularly in the laboratory marked with W, there were obviously very significant "difficulties" in the field of measurements. In fact, the situation is that even in the case of samples from the same site, the measurement results of different laboratories show very significant differences.

902
===== PAGE 891 =====

Secondly, however, we can also notice that even the measurement results of the same laboratory do not form a single continuous series that would reflect a relative chronological order corresponding to the stratigraphic relationships, but e.g. in the laboratory marked y C, the determination of the sample taken from the 5th layer of the II. stratum section (sample number C 743 in the table) resulted in a date of 6.98 BC, while the two samples from the 7th layer of the I. stratum section, which are several layers deeper and therefore older, determined The same phenomenon can be observed in the laboratory marked C, which once gave a practically identical date (6. 6707+320 BC; sample number C 113), and the second time resulted in a date that was up to a hundred years later (6606+330 BC; sample number C 742) in the 5th layer of the II. layer section. The same phenomenon can be observed in the two measurement results of the laboratory marked F: in the II. The age of the sample C" taken from the 5th layer of the stratum profile is 6650+170 BC according to the F laboratory, while the determination of the sample from the 8th basal, i.e. lowest layer of the oldest layer I. section lying below II. in the same laboratory resulted in a date of 6570+165 BC. Therefore, the sample lying several layers deeper, i.e. undoubtedly originating from much earlier times, was again considered almost a hundred years younger in the light of the C" method than the previous sample taken from the much later layer. sample. From these specific examples from Jarmo, it becomes clear to every thinking person why international archaeology and prehistoric research take a skeptical stance towards all prehistoric ideas that their representatives base solely or primarily on CS measurement results. From the above quotes, we have become acquainted with, for example, the unequivocally dismissive opinion of KJ Narr and his co-authors regarding such hypotheses.

H. Müller-Karpe's opinion on the hypothesis of an autochthonous European Neolithic Let us now see how another renowned representative of international prehistory research, H. Müller-Karpe, views the reliability of the C" measurements. Müller-Karpe discusses the archaeological significance of the C method on pages 13-14 of volume III of his monumental handbook, Handbuch der Vorgeschichte (Kupferzeit 1974). In the introduction, he explains that the prerequisite for the historical connection of Copper Age cultural phenomena is a reliable chronology. In this direction, research has succeeded in continuously refining and consolidating the network of regional steps and interregional contemporaneities, so that we have a solid basis at our disposal in the essential points. Then, literally,

folytatja: "However, some Oý provisions contradict this. Above all, these are extremely high values on the one hand for the West European megalithic culture and on the other hand for early-903

===== PAGE 892 =====

ramic complexes of East Asia that would suggest that these two cultural phenomena arose independently of the cultural development of the Near East..."
In Hungarian: "However, some C" measurement results contradict this." These are primarily expressed in extremely high datings, on the one hand in relation to the megalithic culture of Western Europe, and on the other hand to the early ceramic culture complexes in East Asia, which would testify that these two cultural phenomena arose independently of the pre-Asian cultural development..." Finally, Müller-Karpe notes the following: "Hier sieht sich der Prähistoriker heute in einem Gewissenskonflikt: soll er dieser naturwissenschaftlichen Methode strict vertrauen, obgleich er sieht, dass immer neue Fehlerquellen dieser Methode gefunden und eine Menge Bestimmungen bekannt sind die irgend etwas, nur nicht das Alter in Sonnenjahren ausdrücken, oder soll er in Hinblick darauf es vorziehen, in facheigener Ver-antwortung sächst den historischen Quellen und Methoden zu vertrau-en und naturwissenschaftliche Bestimmungen nur soweit zu wenden, als die dabei angewandten Methoden hinreichend erprobt sind und ihre Anwendungsart in einem überprüfbaren Verhältnis zu den erigelten Ergebnissen At this point, the scientist today finds himself in a conflict of conscience: should he blindly trust in this scientific method, even though he sees that new and new sources of error in this method are always being discovered, and a host of age determinations are known that could mean anything but the age calculated in solar years; or, seeing all this, should he, in the awareness of his professional responsibility, first believe the historical sources and methods and only use scientific determinations if the methods used in them have been sufficiently tested and their application can be verified?"

"Is it in relation to the results achieved? In the current state of research, the latter approach seems more appropriate."
So Müller-Karpe also rejects absolute age determinations based solely on C" measurement results, until the new and new sources of error that arise one after another are given a plausible explanation - which is undoubtedly the only logically acceptable position.
Accordingly — in full accordance with the position quoted above — Müller-Karpe writes on page 246 of the second volume of his same handbook (Jungsteinzeit, 1968) about the beginnings of the Neolithic: "That grain cultivation in Europe depended on pre-Asian agriculture and spread through colonization processes is now considered certain." In English: "That grain cultivation in Europe depended on pre-Asian agriculture and spread through colonization processes is now considered certain."

===== PAGE 893 =====

"To this, on the same page, in note number 4, he adds: it can no longer be maintained," . The previous assumption that the alakor might have been cultivated independently in the Balkans is now disproven.

From our point of view, it is also very important to mention that on page 277 of the same volume, Müller-Karpe states in relation to the origin of the Sumerians: "Research has put forward very different hypotheses about the origin of the Sumerians, their possible immigration to Babylonia and the role of a hypothetical indigenous population. Although a final decision is not yet possible, we can say that based on the results of recent excavations, we can now rule out certain theories that previously seemed acceptable; instead, the view that at least a significant element of the Sumerians of the Uruk period was indigenous can now be considered certain."

Similarly, H. Quitta: In his study "Die Radiocarbonaten und ihre historische Interpretation", which is a German translation of D. Srejovic's book "Lepenski Vir" (1973) published as an appendix to his edition, explains on pages 276-278 of the book that the C" content of the air was not al- Jand6. Before 600 BC, the C" content of the air gradually increases, and finally reaches 10%. This leads to dating differences of about 500 years in the 3rd millennium BC, and about 800 years in the 4th-5th millennium BC. However, the estimates are only approximate, since the fluctuations of the C" content of the air are cyclical and we cannot determine their course in more detail. This circumstance was noticed in dendrochronological calibrations carried out using the annual rings of the spiny pine. Finally, Quitta states that the C" method can certainly only be used to determine relative chronology (emphasis added).

In the same study, Quitta (on page 279 of the book mentioned by Srejovic) presents a very instructive comparative graph-table of the ages of numerous sites of the Early Neolithic in South-East Europe and Pre-Asia determined by the C" method. On the one hand, this table clearly shows the 2-3000-year priority of the Near Eastern Neolithic over the earliest European Neolithic, the so-called proto-Sesklo culture, and moreover, the gradual, wave-like spread of Neolithic cultural development from Pre-Asia to increasingly distant surrounding areas can be practically traced step by step.

Then on pages 280-281 Quitta explains that the explanation of the origin of Lepenski Vir from a local substrate cannot be justified: the site is 2000 years later than the Pre-Asian Aceramic Neolithic, and even the ceramic groups of Cardium pottery and proto-Sesklo (Nea Nikomedeia) are older than it. :- According to these, Lepenski Vir was also a similar ecological island, as we often find in the area of the Central European Bandkeramik cultural circle (more

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===== PAGE 894 =====

1. below), where the Mesolithic fishing-hunting indigenous population continued to live in isolation for a considerable period of time next to and among the Neolithic agricultural settlements.
R. Pittioni's theory on the European origin of the band-decorated pottery culture

However, the supporters of the theory of the European priority of the Neolithic Age do not pay any attention to all these uncertainties regarding the C" method and the sources of errors that have not yet been explained, but develop global, worldwide prehistoric concepts, relying solely on the results of the C" measurements. As we have already mentioned above, this hypothesis was first systematically developed by Colin Renfrew in the late 1960s and developed into a comprehensive prehistoric theory, although some experiments in this direction can be found in the literature earlier. Such was, for example, Pittioni's theory, briefly described on page 723 of our book IV., which claimed the autochthonous European origin of the Central European band-decorated pottery culture, and in close connection with this, disputed the dating of the cultural complex to be older than the Körös culture. We have already shown in the aforementioned place, however, that on the one hand Pittioni was completely alone in this idea, but on the other hand — and this is the most important moment in this issue — it can be undoubtedly established from the archaeological finds that in the areas with ribbon decoration, the survival of the Tardenoisian-type Upper Paleolithic-Mesolithic primitive population — at the fishing-hunting economic level — alongside the agricultural-animal husbandry population using ribbon-decorated ceramics, and in larger spots between them (in areas that were unsuitable for agriculture), can be detected in many places for a long time, without the two peoples at different levels of culture having mixed with each other.

KJ Narr writes on this issue: can be . A sharp distinction between the bearers of the Banded Pottery culture and the local Tardenoisium Mesolithic fishermen-hunters-gatherers demonstrated. The separation is so obvious that, although there was a large overlap between the Tardenoisian and the early Ribbon culture, no Mesolithic substrate can be recognized in the narrower settlement areas of the Ribbons. There is no semi-convincing sign that the Ribbon groups developed from indigenous hunter-gatherer complexes, and even the idea - which is still conceivable as a partial way out - that certain groups of people switched or migrated from some other biosphere to loess soils and other soils comparable in their properties and quality to loess is not supported by practically anything in the available evidence. (Unfortunately, anthropological evaluation does not help us much further:

906

===== PAGE 895 =====

The type of Mesolithic population is hardly known, but the few evaluable finds differ strongly from the population of the Banded Ware culture.)" (L. Narr 1975, p. 327)
According to these, the culture of the ribbon-decorated pottery arose through the direct radiation and expansion of the Starcevo-Koros culture (1. Narr 1975, p. 330, also), that is, it was not formed by autochthonous European 6760010, but ultimately by the migrations of the Starcevo-Koros pottery colonies originating from East Asia to the west and north.
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On page 119 of the same book, Narr discusses the pre-Asian origin of the beginnings of European Neolithic cultural development. He explains that the Neolithic groups of Southeastern and Eastern Europe are essentially continuations of the so-called. . They constitute the areas of expansion of the "Middle Eastern cultural current". This finding is particularly relevant to the painted pottery Neolithic groups of Southeastern Europe and their
However, this does not mean - Narr continues - that there could not have been movements that took place in the other direction. "However, the idea that the contacts of the various Balkan cultural groups with Asia Minor and the Eastern Mediterranean did not originate from the East, but from European territory, or even the idea that the oldest copper mining and copper processing groups in and around Hungary were ancient centers of metal processing and influenced the higher cultures of the East, and not the other way around, as has been the generally accepted belief of researchers, must be left on hold for the time being."

Narr then explains that at this point the discrepancy between the traditional relative chronology and the chronology based on C" measurements is most pronounced. The so-called "C" fracture line" ("Verwerfungslinie") consists in the fact that east of Greece and Crete the results of C" measurements do not deviate significantly from the traditional chronology, while north and west of this strip, the C" measurement results of European layers determined to be contemporaneous with Near Eastern cultural layers based on traditional archaeological style parallels and stratigraphic correspondences are generally about a full millennium older than the expected ages. He then writes verbatim: This would significantly shift the relative correlations of the chronology of the two regions (of course not always presented correctly) and with it the degrees of antiquity of the cultures, and would even partially reverse them!

Neither by extending the relevant cultural groups in time (e.g. Vinca, cf. p. 17) nor by reversing the direction of expansion assumed so far can the difficulties be effectively eliminated. (The gap that exists between the Late Neolithic of the Aegean and the Early Hellenic and Minoan cultures is also striking, 1. pp. 527 and 530)."
This is all we needed to know about the first attempts at European cultural priority ideas and the truly calm, methodical approach to the past.

===== PAGE 896 =====

from the reaction of Seti and Östörteneti research to these ideas. However, when developing his theory disputing the primacy of the European Neolithic, Renfrew placed the main emphasis not on the cultural circle of banded pottery, nor on the Southeastern European Coreneolithic cultures, but primarily on Western Europe, namely on the monuments of the megalithic cultural circle, and within these, most notably on the Brittany group, since there were the graves, some of which were determined to be around 4500-5000 BC by dendrochronologically calibrated C" measurements. Not long after Renfrew's hypothesis became known, several other researchers joined this idea, and some even attempted to develop and expand it further. The best known of these researchers is perhaps J. Dayton, who published "Minerals, Metals, Glazing and

Man", published in 1978, an expensive album-like thick book illustrated with a multitude of lavishly displayed multi-coloured intaglio photographs, he claims nothing less than that, in addition to the development of Neolithic culture, the knowledge of metalworking also originated in Europe, more specifically from Bohemia, Silesia and the Carpathian Basin, and from there it reached all other parts of the world, including East Asia, through the migrations of Indo-European metalworking groups (see 1. below).
The prehistoric approach of B. Perlov and A. Kifisin

To our knowledge, these ideas first became known in Hungarian circles after the article translated from the Russian by Kecskemét. The "Source" November 1981 issue "Tatarlaka's Message", authors of the article — B. Perlov and A. Kifisin — argue more or less the same thing as Renfrew and Dayton, and their Their only evidence is the dating of the Tatárlaka finds to around 4500-4000 BC, which they present as an indisputable fact without any further explanation or other support.

It would be interesting to examine a little more closely what conclusions Soviet authors draw from the dating of the Tatárlaka finds to between 4500 and 4000 BC. If we understand the basic tone of the article, it immediately becomes clear what non-scientific driving forces lie behind the heated debate, which at first glance seems purely academic and devoid of any concrete significance, about whether Neolithic cultural development and the introduction of metallurgy occurred earlier in Europe or in East Asia.

In the introductory pages of the article, Perlov only repeats the fact that the Tatárlaka tablets, according to the C* definition, are more than a thousand years older than the Mesopotamian Dzhemdet Nasr period, with whose Sumerian pictographic signs they are practically identical. From this 908
===== PAGE 897 =====

For now, he only draws the conclusion that according to these, the creators of the Balkan Vinéa culture penetrated Kurdistan and Khuzestan in the 5th millennium BC through Asia Minor, where they settled as Sumerians around 016. So the discoverers of writing were not the Sumerians, but the inhabitants of Transylvania — so far, János Perlov, Mindehzezz Makkay notes the following on page 120 of his book "The Tartary Finds": "It is clear that this wild-flowered linguistics and historiography, and even archaeology, is not only a Hungarian specialty. It is not difficult to recognize the starting point in the early conquering path of the Vinéa culture, spreading culture and writing, starting from the Balkans: Renfrew and Gimbutas' theories about the development of independent European civilization in the 1st millennium BC, which disregarded fundamental facts. "From the 6th millennium."

A On the last two pages of the article mentioned in "Source", the alleged Soviet Sumerologist A. Kifisin comments on Perlov's previous discussions. We must say that this Kifisin is indeed a "strange Sumerologist", who immediately states bluntly that in 2852 BC (that's right! - it's a good thing that he doesn't give the month and day) the nomadic Avars invaded China from the northwest and brought a fully developed writing system with them. This writing system, Kifisin continues, along with all the other writing systems, originated from a writing system originating in the 9th-8th millennium BC, which in turn can be traced back to an Upper Paleolithic writing system. The disintegration of this ancient unified religious symbolism system occurred, according to the evidence of the Tartar tablets, well before the 5th millennium, and somewhere outside the territory of Sumer.

However, this dense jumble — which (and we must state this) has no archaeological or other substantiation in the world — is by no means the end of the program of the Soviet "Sumerologist" prehistoric fairy tale afternoon, although this would be more than enough to classify this elaboration by Perlov and Kifisin in the literary category where it belongs: namely, among the horror novels of pulp literature. But so much absurdity obviously does not even remotely satisfy Kifisin's confabulation demand. In the following, we also learn from him that in the 7th-6th millennium BC the Neolithic revolution, the transition from gathering to farming, took place in Western Europe (sic! cf. Renfrew's hypothesis), and as a result a demographic explosion followed. be: the population increased from 5 million to 85 million in a few centuries. This led to a large-scale migration of the population to sparsely populated areas where the Neolithic Revolution had not yet taken place. The attack took place from the north along the Danube southwards through Asia Minor, the Middle East, North Africa and Spain. Taking advantage of their vast numerical superiority, these ancient Semites pushed the pre-Indo-Europeans into the far north.

909
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- Now let us pay close attention, because all these absurdities served only as an introduction for Kifisin to then put the crown on the dazzling primitiveness of his "prehistoric view" that he had received from the air. Let us quote the lines in question verbatim from the article in "Forrás": "Visual images of this struggle of peoples have survived in Celtic mythology. The ancient names of the Celtic gods testify that the ancient Slavs who did not surrender to their enemies remained as a warning sign in the eyes of the ancient Celtic population of France, and they were considered gods. The Danaan-ancient Slavs, who came from the Gorla tribe (i.e., mountaineers"), subdued the Garc-ancient Greeks, and then began a long struggle with the Semitic peoples of the Danube cultures. This is reflected in the Indian (Manu-Szvarozsics) and Greek myths."

We believe that any further commentary on these lines is unnecessary: they speak for themselves. Therefore, we will only note that, according to Kifisin, the ancient Celts already considered the ancient Slavs to be gods, and that these same ancient Slavs already subdued the ancient Greeks and began a long war with the Semites when — also according to Kifisin's own definition — there were only pre-Indo-Europeans, who were pushed far north by these ancient Semites (quotation 1 above).
Now, since, to the best of our knowledge, all the Slavs, the Celts, and the Greeks are Indo-Europeans - and this fact is obviously not something Kifisin wants to doubt - if all these peoples had already subdued each other at a given —, time, or considered each other to be gods, then they could hardly have existed at the same time.
at the same time, there were also Proto-Indo-Europeans who had not yet separated from each other, and even had not yet finally developed, as Kifisin claims.
However, these nauseating outpourings of Slavic chauvinism are not only of interest to us in themselves, but are also very instructive for us in that it is difficult to recognize the starting point behind the above muddles of both Perlov and Kifisin, as if in their intellectual background, as János Makkay, quoted above, very aptly put it: the theories of Renfrew and Gimbutas, which disregard fundamental facts, about the development of an independent not European civilization from the 6th millennium BC.
In other words, the fentick — and many other such archaeological and prehistoric absurdities — are not the intellectual pursuits of some obsessed person in a vacuum, but rather the authority of the world-renowned professor of archaeology at Cambridge University. In fact, it is this intellectual environment that makes Perlov and Kifisin's prehistoric vegetable garden truly dangerous, because their statements, taken in themselves, are astonishing, and no one would take them seriously. In such circumstances, however, many people think that if one of the professors at Cambridge University were a supporter of similar ideas, and even a whole science,

===== PAGE 899 =====
established a school of thought for the widest possible propagation and dissemination of such hypotheses, then there may be some concrete | prehistoric reason for this hype. And there are many more such people who respect authority than anyone would think. | However, we must once again draw the attention of our esteemed readers to the fact that these ideas, which are based exclusively on European C* age determinations, which often provide measurement results referring to fantastically old times, are neither paid attention to nor acknowledged by the vast majority of authoritative experts, and those who do mention them at all, as a curiosity, immediately reject them outright, since the radiocarbon method currently has so many undiscovered and unexplained sources of error that it is completely unsuitable for absolute age determinations. As typical examples of these flatly negative opinions, we have already described above the evaluation of KJ Narr's internationally recognized prehistory handbook (I. Narr: Handbuch der Urgeschichte II. Band 1975, pp. 505 and 298) and H. Müller-Karpe's equally fundamental handbook on the same subject (I. Müller-Karpe: Handbuch der Vorgeschichte 111. Band 1974, pp. 13-14) on the ideas of Renfrew and his colleagues regarding the European priority of Neolithic development. However, it is particularly characteristic of the shocking lack of method on the part of the representatives of these theories that claim European cultural priority that they even select among the C* measurement results: namely, they only take into account those measurements that point to particularly old times, and they simply ignore the other radiocarbon dates that do not fit into their hypotheses - as Narr also pointed out in his opinion quoted above, this eminently important methodological (i.e., actually lack of method) moment. We believe that it is not necessary to prove in more detail that this procedure can be called anything but scientific.

not.
Renfrew's turn
In addition, on the evening of January 4, 1985, at 8:15 p.m., Austrian television broadcast a prehistoric program on the development of the Neolithic Age. C. Renfrew also appeared on the program again. After initial debate, he finally admitted that the hypothesis that he and his followers had until recently advocated for the European priority of the Neolithic Age had become outdated, because the results of the latest archaeological research clearly demonstrated that the Neolithic development began in Near East Asia and spread from there to Europe. Renfrew thus retracted his theory of the European origin of the Neolithic development on television, "coram publico", i.e. publicly.
911
===== PAGE 900 =====

Incidentally, it should also be noted that Renfrew's reference to "the results of recent archaeological research" is quite obviously can only be considered a cheap excuse, with which his theory is clearly tried to somewhat conceal or embellish his complete bankruptcy, since the relevant archaeological literature before 1970 -- i.e. Renfrew's hypothesis before detailed elaboration -- anyone can conclude from his published publications that the alleged "latest" archaeological results, which undoubtedly proved the millennial priority of the Near East in the field of Neolithic development - and to which Renfrew refers, we must refer, in which we can find many times the excavation results proving the --, were already known before his theory was formulated. In this regard, it is sufficient to refer to the works listed in the bibliography of our Book IV, published before 1970. indisputably millennial priority of the pre-Asian Neolithic civilizations, which Renfrew referred to on television in 1985 as "latest".

J. Dayton's hypothesis on the European origin of metalwork
Going further, Renfrew's clear statement about the withdrawal of his hypothesis naturally strongly shakes the credibility of his followers, such as J. Dayton's similar ideas. But let's not rely only on this indirect refutation, but rather take a closer look at what Dayton claims in his aforementioned book, published in 1978, what he bases his ideas on and what evidence he can list to support his idea.

As we have briefly mentioned above, Dayton places the invention of the production of real bronze alloyed with tin in the Carpathian Basin and especially in the Bohemian-Silesian region. (It is almost unrecognizable in this location that at the beginning of our century he was constantly drawn to prove the high culture of the ancient Indo-Germanic people.)

and the scientific reminiscence of the so-called Aunjetitz bronze culture, which was praised to the skies and considered the oldest bronze artefact in Europe at that time, or Dayton's barely concealed attempt to rekindle this long-debunked and archaeologically refuted theory of former Indo-Germanic cultural priority.) He bases this hypothesis mainly on the fact that, in his opinion, tin is not found anywhere in the Middle East (there is tin only in Egypt - he writes - but the site there was only discovered in modern times.)

In contrast, the real situation is that both in Eastern Anatolia (in the eastern range of the Taurus Mountains), in the Caucasus and in Azerbaijan, there are already anciently mined self-deposits (1, RJ Forbes: Studies in ancient Technology, Volume IX., p. 136, map). Similarly, tin was mined in the Mesed region of northern Iran in ancient times.

912
===== PAGE 901 =====

and according to the research, the early Sumerian bronze industry mainly obtained its tin needs from there (1. Müller-Karpc 1974, p. 126). Dayton's argument 16 is therefore nothing more than a common mystification.

In his questionable book, Dayton also wants to introduce a new absolute chronology in the Middle East. However, this is absolutely necessary because, in his opinion, the previous determinations of time in the Middle East are practically based only on the data of the historical work of Manetho, an Egyptian priest who lived in the 3rd century BC, and on the chronology developed by Egyptologist F. Petrie at the beginning of this century on the basis of these Manetho-like data. In this connection, Dayton directly speaks of the time when it would be time to shake off the "shackles of Manetho", as a result of which, for example, Petrie dated the beginning of the First Egyptian Dynasty to 5546 BC. The Egyptian chronology is therefore, according to Dayton, completely outdated and requires a reduction of drastic transitional periods (the called , must be completely subtracted from the traditional Egyptian confusing and obscure periods between the Old and Middle Kingdoms, and the Middle and New Kingdoms), during which, for example, the so-chronology.

It would not hurt to examine Dayton's claims in more detail on this issue. First of all, it is immediately apparent that Dayton is quite obviously banging on open doors when he loudly demands liberation from the "fetters of Manetho". This process has been taking place in Egyptology for several decades, and no one dates the beginning of the First Dynasty to the middle of the 6th millennium, as Dayton claims, but rather to around 3000, or more recently to around 3200-3100, with certain Mesopotamian origins, from the late Uruk period there, ca. 3300-3200 BC, found in prehistoric Egyptian tombs of the so-called Nagade II period, and which date the beginning of the First Dynasty to 3200-3100 BC (our Book 1, IV, pp. 750-751). It is very typical of Dayton's "methodical" approach that he first claims that Petrie dated the beginning of the First Dynasty to 5546 BC (l. above), and later says that Petrie dated the beginning of the First Dynasty to 3000 BC.

"It is obvious here that Dayton himself does not recognize himself in the fabric of his hypothesis and literally refutes himself. It is no less instructive when, in order to refute Petrie's other datings, he argues that pottery from the New Kingdom period of Egypt was found in Abydos, so Petrie was obviously mistaken when he dated the tombs of Abydos to the early dynasties. -- On the contrary, the real situation in Abydos seems to be that around one of these very old royal tombs there, which tradition later considered to be the tomb of the god Osiris, precisely because of the antiquity of the tomb, a real "place of pilgrimage" developed during the New Kingdom, and during the annual ceremonies held at the tomb in honor of Osiris, the believers broke masses of clay pots, commemorating

===== PAGE 902 =====

to the killing and dismemberment of Osiris. This custom can be traced back to the Ptolemaic period in Abydos. On this basis, one could even assume that the tombs of Abydos—that is, in fact, the earliest Egyptian dynasties—are later than Alexander the Great, since a large number of Hellenistic potsherds have also been found in Abydos. We believe that it will gradually become clear to the patient reader what unqualified manipulations Dayton is working with.

As for the so-called "transitional periods" in Egypt, that is, the confusing and obscure times between the Old, Middle and New Kingdoms, which Dayton says we should deduct from the traditional Egyptian chronology, well, the situation is that they are constantly shrinking, as more and more dateable remains are discovered during excavations.

inscriptions emerge from these transitional periods, which gradually fill in the previously missing, i.e. undatable, years — and, incidentally, they mostly confirm the (if not detailed, at least global) accuracy of King Manetho's list.

Let us look at a concrete example of this shrinkage. The transitional period between the Middle and New Kingdoms was estimated to be about 300-400 years 40-50 years ago, but today the vague, undated period does not even amount to 200 years, including the rule of the Hyksos. From this anyone can gauge how unfounded Dayton's demand that we should deduct the transitional periods from the Egyptian chronology is. This is something like someone claiming, for example, that we should deduct the century from Hungarian history from the conquest to St. Stephen, because we can only accurately date a few years of this period with written sources.

In addition to all this, however, Dayton also summarily demands that approximately 400-500 years should be deducted globally from the accepted absolute chronology of the Middle East, i.e. he wishes to drastically reduce the chronology of not only Egypt but also the entire Near East. Let us not ask now on what basis Dayton comes to this conclusion, i.e. what are the specific archaeological facts that would require such a rather large and general chronological reduction in the entire Near East. Apart from the fact that we simply do not know such facts, we can conditionally even start from the approximately 500-year reduction of the Near East chronology proposed by Dayton, and even in this case the millennial priority of Near Asia over Europe in the field of metalwork is clearly demonstrated.

Let's look at some specific archaeological sites.

1.) Cold-hammered copper tools from around 7000 BC (uncalibrated C* age!) were found in Cayönü in Central Anatolia.

914
===== PAGE 903 =====

2 In western Iran, smelted copper appeared around 6600 BC at the site of Catal Hüyük in Anatolia and the nearby site of Suberde.

Around 6400 BC, copper and lead were already being cast.

And so on, at dozens of East Asian sites. We emphasize that the dates just reported are all uncalibrated C* measurement results.

If we now conditionally reduce these traditional chronological data by 500 years, as Dayton claimed, taken from the air, then we get the following dates: cold-hammered copper in Cayönü around 6500; smelted copper in Ali Kos around 6100; copper 65 lead casting in Catal Hüyük and Suberde around 5900. -- On the other hand, even Dayton himself only states in his book that European metalworking began at the beginning of the 4th millennium BC, i.e. around 4000-3900, and that copper was only used to a greater extent in Europe from the beginning of the 3rd millennium BC, i.e. around 3000-2900. After this, it is completely incomprehensible to us, and even downright mysterious, how Dayton can claim, given such archaeological findings, that metalworking began in Europe. After all, the generally accepted and applied division of periods in international archaeology speaks of a Copper Age in Pre-Asia starting from 5500 BC, because after the aforementioned beginnings in the 7th millennium, around 5500 the use of copper was already widespread there, from Western Anatolia to the Zagros Mountains. If, in accordance with Dayton's chronological reduction, we now conditionally correct the date of the beginning of the Pre-Asiatic Copper Age by 500 years downwards, then we still arrive at around 5000 BC, and where from this are the beginnings of European metalworking dated by Dayton to 4000-3900 BC and its greater spread around 3000-2900 BC? Even so - even if Dayton's half-millennium chronological reduction of Pre-Asia, which he claimed without any concrete basis, is taken into account conditionally - Pre-Asia is 2500-3000 years ahead, both in terms of the earliest processing of copper and the greater prevalence of copper tools.

A characteristic light is shed on the deeper roots and wider connections of Dayton's stubborn attempt to prove the priority of European metalwork despite the facts presented above, by the fact that he, with barely concealed greed, immediately connects European metalworking, which he believes is older than Proto-Asian metalwork, with the Indo-Germanic peoples, who have recently been fondly called Indo-Europeans since 1945 (the term "Indo-Germanic" has not had a good sound since Hitler's Germanic, "Überrnensch" theory). However, Dayton is not satisfied with the mere reference to the fact that, in his opinion, these Indo-Europeans invented metalwork, but instead re-rides almost every one of the - for the most part, long since obsolete - horsemen of Indo-Germanic studies of the beginning of our century, e.g. the worldwide Indo-Germanic-915

===== PAGE 904 =====

migration theories (of which, unfortunately, archaeological traces have not been found to this day), the notorious "Herrenschicht" theory, according to which higher civilization and culture were introduced to all worthwhile ancient peoples by different Indo-Germanic ruling strata ("Herrenschicht") that migrated to all corners of the world. Moreover, in this effort to proclaim an Indo-Germanic cultural mission, he goes far beyond all previous experiments in this direction. For Dayton, from the Sumerians through the ancient Egyptians to the bearers of the Indus culture, everyone was "Indo-European", according to the Sumerian question, and the ziggurats and pyramids were of course also "Indo-European" according to Dayton. Reading this, the question involuntarily arose: are we not facing a renaissance of the primitive Indo- non-existent Germanic racial ideology of the ill-remembered Hitler times in cases of such outbursts? Of course, we do not want to assume "ruling race" (no mistake: literally!) built it.

this about Dayton, but we must definitely say that in any case, the global parallelism of the approach with the chauvinistic Slavophile nonsense of the Soviet A. Kifsin described above, which is completely identical in every way, and which calls the ancient Slavs gods. Apart from this, however, it is clear from all these absurdities that Dayton is an unreserved supporter of the Indo-Germanic school which the "Reallexikon der Assyriologie" calls an ahistorical trend. We have already briefly mentioned this school of Indo-Germanic studies on page 77 of our book I, but due to the eminent importance of the question, we will quote the text of the Reallexikon on this subject in full below: , We can no longer speak of a common opinion of Indo-Germanic studies. The uncertainty that has arisen over the Hittite verbal system, which has been at the heart of every inflected language since the twenties and thirties, has now led to any doubt about the reconstruction of the Indo-Germanic language and to almost any reconstruction variant. Parallel to this is the increasing ahistoricity of lđg.-istik (whose task is of course not exhausted only in the — here interesting — question of origin) while simultaneously giving acquittal to legal evidence." (see Reallexion der Assyriologie und vorderasiatischen Archäologie, V. Vol., 1976-1980, p. 91. under the heading "Indogermanen") In Hungarian: "Today we can no longer speak of Indo-Germanic studies being in agreement. The uncertainty caused by the Hittite language since the twenties and thirties, the most important central question of all dialectal languages, the Indo-Germanic verb system, has in the meantime led to arbitrary doubts and almost arbitrary reconstruction versions regarding the reconstructibility of the Indo-Germanic base language.

916
===== PAGE 905 =====

In parallel with this, an increasingly ahistorical Indo-Germanic studies (whose task is of course not limited to the examination of the question of origin, which interests us here) developed (literally: runs; note from us) with the simultaneous absolutization of sound-law proof procedures."

We believe that from what has been said above, anyone can clearly recognize the specific weight of "evidence" Dayton uses when he tries to support the hypothesis of the European primacy of metalwork.

However, there are other, clearly refutable, absurd claims in Dayton's argument. For example, he gets involved in astonishing chronological contradictions in close connection with his own hypothesis. Among other things, he claims that the chariot models and wheel finds of the Baden-Pécel archaeological culture appeared simultaneously with the introduction of metalworking in Europe. Dayton, on the other hand, dates Baden-Pécel — chronologically correctly — to the second half of the 3rd millennium BC, while otherwise he disputes that metalworking has been generally widespread in Europe since at least 3000 BC, and dates the beginnings of its introduction to around 4000-3900.

So when did European metalworking begin? At the very beginning of the 4th millennium, or during the Baden-Pécel culture, around 2400-2200? We get no answer to this tricky question from Dayton, because as we can see, he argues sometimes one way and sometimes the other.

Then, after having thus brought the chronology of his own hypothesis to absurdity, he brings up his "Baden-Pécelian Indo-Europeans" who he then has dispersed throughout the entire Middle East, saying that they were the ones who introduced metalworking everywhere in East Asia. It is extremely regrettable that he forgets to add to all this that while the Baden-Pécelian culture belongs to the second half of the 3rd millennium BC - according to Dayton's own dating! (1. above) - on the other hand, at all those East Asian sites that he lists in this connection as Baden-Pécelian cultural emanations, the knowledge of metalworking can be archaeologically dated back to at least the 4th millennium BC, and even more than once to the 5th millennium BC, in large numbers, by the mass of metal finds. Just one example: according to Dayton, Tepe Gaura (in his English, written as "Tepe Gavrá") also These "Indo-Europeans" from Baden-Pécel brought the knowledge of metalworking. In sharp contrast to this, however, the actual findings show that copper objects were found in Tepe Gaura in layers dating back to the 5th millennium, a mass of metal finds were discovered around 3500 (gold, silver, electron, copper), and around 3200 real tin-bronze was already being cast (for more information, see p. 695 of our book, or Lloyd 1981, pp. 102 and 104; Amiet 1977, p. 509). However, these dates -- even taking into account Dayton's 500-year reduction to the Elo-Asian chronology, which he claimed without any basis -- are much earlier than the Baden-Pécel culture, which Dayton himself dated to only around 2400-2200.

===== PAGE 906 =====

Time of the Dayton's, Dayton's shockingly primitive chronological confusion is once again on full display.

After this, we can already begin to guess what trick Dayton used in formulating his hypothesis. Namely, all indications point to the fact that he implicitly reduced the development of Pre-Asian culture to the period of the urban Sumerian high culture — presumably counting on the fact that most interested but uninitiated readers have only heard something about the Sumerian high culture in connection with Pre-Asia, and are not familiar with the earlier, many millennia-old Neolithic and Chalcolithic civilizations there. Therefore, Dayton completely ignores these civilizations — along with their countless metal finds — and by this ignorance suggests to his good-faith readers that human civilization in Pre-Asia only began with the urban high culture of the Sumerians, ca. BC. around 3500, or only around 3000, taking into account the 500-year reduction of the pre-Asian chronology he demands. — Looking at the question from this perspective, we now understand why it is so important for Dayton to shorten the absolute pre-Asian chronology by 500 years: simply because it is the only way he can make his idea that European metalworking preceded pre-Asian metalworking acceptable, or rather semi-believable, at least to the vast majority of the generally educated, but far from expert, readership interested in such prehistoric questions. As a final conclusion, we can therefore rightly state that C. Renfrew and his followers — such as J. Dayton 1s — with their unfounded, and even downright unscientific, hypotheses significantly hindered the progress of the 70s-80s in several respects for about 10-15 years.

It was therefore high time that Renfrew himself finally — in the first half of the 1980s — recognized that his theory could no longer be maintained, because it was in direct contradiction to most archaeological finds and relied solely on the C¹⁴ measurements, which were still of very uncertain reliability — and even then only on a "purposefully" selected fraction of them.

Explanation of errors in AC¹⁴ measurements

Although we have already dealt with the C¹⁴ measurements in more detail above, we must now bring them up again, and in even more detail, because they constitute the alpha and omega of any European cultural priority idea: we have already repeatedly pointed out that every hypothesis of this kind practically tries to support the theory of the European priority of the Neolithic Age or the Neolithic period with the results of the C¹⁴ measurements alone. The issue of the reliability — or rather, unreliability — of the CS method and at the same time the dating of the Tātárlaka pictographic tablets to the 1st century BC.

918
===== PAGE 907 =====

Makkay provides a very instructive overview of the very high improbability of dating CI between 4500-4000 in his issue, "Sumerian Signs from Transylvania?" 68 in his "Life and Science"? 1980, vol. 41, and 42. János studies entitled "The Whiskey Test", and especially in the article entitled "The Whiskey Test", it is clearly seen that the results of the C¹⁴ measurements do not only differ very significantly in the case of the Ta Tarlak tablets - in 67 cases by more than 10 years - from the age determinations based on traditional, historical connections, precise stratigraphic observations and detailed site parallels - but this discrepancy occurs in almost all cases. With a very sharp insight into the problem, Makkay touched on the innermost essence of the question when he states that the cultural memories that appear to be simultaneous through the Ces datings are therefore very different from other real connections based on the formal similarities of the finds, the trade threads, the cultural influences, etc.

Sometimes CS finds connections and contemporaneity between groups of finds whose culture of creation shows no material or intellectual connection or kinship | in fact, they are separated by a thousand or more years. (1. Makkay: The whiskey test). This is the case, in addition to many other cases, with dates derived from C¹⁴ analyses for the Mykonian period, which often differ by half a millennium from chronological data from well-known historical sources and verified with the help of Egyptian records. For example, in the case of the determination of the date of the destruction of the Minoan culture after the famous volcanic eruption of Thera, which, according to historical documents, must have occurred between 1450 and 1500 BC, while the C¹⁴-cs dates show a fluctuation of 700 years, writes Makkay. In other words, it is practically the same situation as with the C¹⁴ dating of the Choralolithic settlement of Jarmo discussed above, where the CS measurement results literally turned the actual excavation stratigraphic relationships upside down.

Makkay then deals with the so-called "chronological fall line" appearing in the southeastern European-Aegean region, the essence of which, as we already know, is that from a wide strip stretching from the Black Sea coast of Turkey to the south and east through the Turkish-Bulgarian border region and Central Greece, the C¹⁴S measurement results are mostly, although poorly, compatible with the relative chronology determined by the traditional method of archaeological stratigraphic comparisons and parallelism of finds, and with the absolute chronology obtained by modern astronomical conversion of the chronological data thus obtained from astronomical phenomena preserved in Egyptian records (the so-called

Síriusz peridu sokzrkal) as well, but on the other hand, north and west of 6 bands, i.e. in the northern areas of the Balkan Peninsula, in the Carpathian Basin, and in the rest of Europe, the dates obtained with the C¹⁴ method are mostly significant.

===== PAGE 908 =====

They appear to be considerably older than the ages determined by traditional methods and are completely incompatible with the traditional chronology. Dendrochronological calibration then significantly ages these already probably old European C¹⁴ dates. The cause of this "break line" is not yet clearly explained, Makkay notes. Finally, he writes: "ys y 1681 and in many respects more refined dates cannot be discarded until the new method of dating is beyond doubt and can be interpreted clearly." (1. Makkay: The Whisky Test)

Well, what about the previously discussed South Regarding the "break line", a very obvious and completely satisfactory scientific explanation has recently emerged.

Balkan C¹⁴ in relation to this phenomenon — and not only this, but also other, hitherto incomprehensible and unexplained sources of error in the C¹⁴ measurement results, such as the 700-year fluctuation mentioned by Makkay in the dating of the C¹⁴ volcanic eruption on Thera, which is still the so-called Cycladic island, about which we wrote above that the results of the C¹⁴ falls on the "Middle Eastern side of the fault line", that is, on the area where measurements there can largely be inserted, for better or worse, but still somehow, into the absolute chronology established by traditional archaeological methods. However, from the Thera example we can see that the C¹⁴ method often provides completely unacceptable dates even on the Near Eastern side of the "tör&svonal". The aforementioned explanation is so astonishingly simple and almost self-evident that one can only wonder that no one thought of it until recently. In short, the point is that a fundamental, theoretically significant source of error has been ignored in C¹⁴ measurements up to now. The most important basic assumption of the C¹⁴ method from the beginning was, and still is, that when the organic matter under study dies, the ratio of C¹⁴ isotopes of the carbon atoms contained in it was assumed to be identical to the C¹⁴ isotope ratio of atmospheric carbon dioxide, i.e. this ratio was and is considered to be a known constant starting point. However, in reality, this is not the case. The ratio of non-radiating carbon atoms with an atomic weight of C¹⁴ incorporated into the samples to be examined and the ratio of radiating isotope atoms with an atomic weight of C¹⁴ is very different in living plant organisms, depending on the proportion of the incorporated carbon atoms that actually originate from atmospheric carbon dioxide (via the so-called photosynthetic plant metabolism, during which the carbon atoms of the carbon dioxide in the air are incorporated into the plants under the influence of sunlight energy, naturally together with the radiating C¹⁴ carbon atoms present in the air, (in proportion to their occurrence), and in what proportion are carbon atoms of other origins present in the relevant plant organism, i.e. those that come from mineral sources, namely those abundantly present in all soils.

===== PAGE 909 =====

They originate from carbonate rocks. Namely, not only are the carbon atoms of the carbon dioxide in the air incorporated into the nomenclature, with their specific percentage of radiating C¹⁴ isotopes, but also the carbon atoms of carbonates from rocks and minerals of different ages that decompose in the soil enter the plants through the root system. And from this fact follows the most important, fundamental, systematic source of error in C¹⁴ measurements that has not been taken into account so far. Carbon-containing minerals from the soil practically do not contain radiating carbon atoms with an atomic weight of C¹⁴, since the C¹⁴ atoms of the carbon dioxide in the air entered the rocks and minerals millions of years, even hundreds of millions of years ago, and thus long ago they all decomposed one by one into non-radiating C¹² carbon atoms, considering that the C¹⁴ radiating carbon atom its half-life is extremely short compared to the durations of geological periods, only 5568 or, according to more recent calculations, 5730 years. Thus, these carbon-containing materials of mineral origin did not contain practically a single radiating C¹⁴ carbon atom when they were incorporated into the plant organisms from which the samples intended for C¹⁴ measurements originate a few millennia ago. (See above 1. Hajdú-Kristó-Róna-Tas; Introduction to the sources of Hungarian prehistoric research, volume IV, p. 25)

The enormous differences this previously unconsidered fact leads to in C* age determinations can perhaps be most instructively illustrated by a simple example. We can illustrate this with the help of a model calculation. Let us take as an example a sample of plant origin, which — for the sake of simplicity of the calculation — contains exactly 1,000,000,000 (one billion) carbon atoms. Also for the sake of simplicity, let us assume that exactly 5 percent of the carbon atoms originating from the air in this sample are 60,656 radioactive carbon isotopes, i.e. 50,000,000 atoms in the practice of the C* method so far. If we determine the C* age in this case, then this sample — again for the sake of simplicity — should contain 25 million radioactive C*. carbon atoms, e.g. after 5730 years, i.e. after the radiation half-life of the C* carbon atom has elapsed, according to the assumption used so far in the C* method. In other words, if the radiation intensity was exactly half the radiation intensity assumed to have occurred when the plant in question died, then the measurement using the previous C* method would have resulted in the sample being exactly 5730 years old, the half-life of the C* carbon atom.

If, however, only 80% of the carbon atoms found in the previous sample, for example, come from the carbon dioxide of the air containing 5% C*-radiating carbon atoms, and 20% of them entered the organism of the plant in question from the carbonates of the soil containing zero% C*-radiating carbon atoms, then the sample was already originally — at the time of the death of the plant in question —921

===== PAGE 910 =====

in - instead of 50 million, it contained only 40 million radiating C*4 carbon atoms, since 20% of the mineral-derived carbon atoms did not contain radiating carbon atoms even during the life of the plant in question, which corresponds to exactly 10 million missing C* radiating carbon atoms in the sample in question. Therefore, if the C* age determination is carried out with the aforementioned sample according to the previous practice, and the measurement results in a radiation intensity corresponding to 25 million radiating C* carbon atoms, then the results of the previous measurements have shown that the sample is exactly 5730 years old, since the amount of radiation is exactly half of the radiation intensity of 50 million radiating C* carbon atoms assumed at the time of the plant's death. However, the real age of the sample in this case is not 5730 years, but significantly more than that - 20 %, i.e. 1046 years — less, i.e. 4684 years, because the sample originally contained not 50 million, but only 40 million C*-radiating carbon atoms, and of this quantity, 25 million were still radiating at the time of the measurement, so only 15 million radioactive atoms had decayed since the death of the plant sample. This also means that 20% less time has passed than the 5730-year half-life. However, the previous C*-measuring method in this case — as mentioned above — already calculates 5730 years after the death of the plant in question.

This cardinal source of error, which is of a theoretical nature and leads to millennial differences, is dwarfed by not only the other sources of error and inaccuracies introduced so far, but also the additional uncertainty factors listed in the aforementioned Hajdú-Kristóf-Róna-Tas publication. For example, the fact that carbonates of mineral origin can also enter the animal and plant food chain through other sources (e.g. in the form of dissolved salts in drinking water). Unfortunately, the proportion of the sources of incorporation cannot be verified afterwards, since numerous factors, such as the current geological, hydrological, climatological and bioecological conditions of the environment, the degradation zones that are currently in effect in local sedimentation, possible changes in the food forms of animals from time to time, etc. (All of this is from I. Hajdu-Kristó-Róna-Tas; ref., vol. IV, p. 25)

From the above, we can see that this completely ignored circumstance, which, according to the authors of the chapter in question of the aforementioned work, Magda Járainé Komlódi, László Kardos and János Tardy, was personally communicated to the editorial staff by László Jakucs, provides a reasonable and obvious explanation in every respect for the sources of error of the C* method, which were previously considered inexplicable, and for the large deviation of the C* datings from the time determinations obtained with traditional archaeological methods, 922

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For example: The so-called "C fault line", which has been completely mysterious and inexplicable until now, which stretches across the border of European Turkey and North Greece in a wide strip, first west, then southwest and finally south, can be easily interpreted by the fact that in the East Asian region (Mesopotamia, Syria, Anatolia, Egypt) due to the natural precipitation, there is much less carbonate salt dissolved in water in the mostly very dry soils than in the much more humid European soils, which are flooded by much more rain (and snow); as a result, in East Asia, soil carbonates are usually only incorporated into plant organisms in a small percentage, while in Europe this incorporation rate is almost everywhere much higher. However, the more carbon atoms originating from the carbonate salts of the soil in a plant sample, the more — and in parallel — the fewer the number of carbon atoms incorporated into the organism from the carbon dioxide in the air, the higher the age of the sample in question determined by the C* method will appear, often by 1000, and sometimes even much more, years.

Thus, the mystery of the European C* measurement results, which result in fantastically high age determinations and are often 1000, and even many times many more years older than dates obtained with traditional archaeological methods, is almost solved by itself: the higher the proportion of carbon atoms in the sample in question that come from mineral carbonates, and are therefore practically radiation-free — because they do not contain C* atoms — the higher the age of the sample in question determined based on the previous C* method becomes 15.

According to these, the merit belongs to László Jakucs for having given a clear and at the same time global scientific explanation for all the errors of the C* method so far, and in an exhaustive and logical manner, one might say self-evident. Under such circumstances — we emphasize once again — we can only wonder how it happened that no one had thought of the otherwise well-known fact until now that plants do not only live from the air, but also from the mineral salts of the soil, which they absorb from the soil in a water-dissolved state through their roots and incorporate into their cells.

In other words, it follows from the above that, due to the fundamental, theoretical and methodological error recognized by László Jakucs, the C*** method cannot yet be called an objective, scientific procedure today, because we do not have any method for calculating the numerical ratio of mineral-derived s-enatoms. However, it also follows from this fact that all kinds of assumptions that primarily derive from these completely 923

===== PAGE 912 =====

starts from unreliable C*4 measurement results and builds its ŷy ideas on them, it cannot yet form the basic pillar of any theory claiming to be scientific.

After this C* excursion, we can conclude with regard to Dayton's previously described book that since its entire concept and fundamentals build its conclusions and theory on the results of C* measurements, it cannot be considered a scientifically founded work.

I. Velikovsky, "New Egyptian Chronology" In addition, however — for the sake of completeness and to shed more light on the scientific standard of Dayton's work — we must also mention that he enthusiastically refers to Velikovsky's "new Egyptian chronology" as "proof" of the necessity and drastic reduction of the Egyptian chronology he demands, which he considers interesting and worth dealing with. This reference sheds a sharper light than any criticism from outside on the intellectual environment in which Dayton's work belongs. The aforementioned Velikovsky — whose full name is Immanuel Velikovsky — became known, or more precisely, notorious, in the orientalist branch of prehistory research because, based on the ultra-conservative fundamentalist trend that believes in the absolute authenticity of the Old Testament books of the Bible down to the most minute details, he developed — or rather invented — a global hypothesis that in the 15th century BC the planet Jupiter would have ejected a giant comet, which would have brushed the earth with its tail twice as it passed by. Velikovsky needed this cosmic catastrophe solely so that he could use it to explain the various miracles of the Bible, primarily the miraculous events preceding and following the exodus of the Israelites from Egypt, one by one, and to interpret them in a more or less believable way for today's reading public, creating the appearance of natural science. The dating of the catastrophe was also successful precisely in the 15th century BC, because, for example, the otherwise historically quite obviously impossible statement in the Bible, according to which King Solomon's temple in Jerusalem was built 480 years after the exodus from Egypt, could also be perfectly supported, i.e. its authenticity would be probable. Solomon reigned between approx. 960-926 BC, so the temple was also built during this period. Counting back 480 years from 960-926 BC, we arrive at 1440-1406 BC, or — we could say: exactly the 15th century, the time of the catastrophe assumed by Velikovsky. Well, this is actually not at all amazing, because all available data clearly indicates that

"in a wonderful way" —

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The Velikovsky catastrophe was placed in the 15th century precisely to explain the 480-year period between the construction of Solomon's temple and the exodus from Egypt, as mentioned in the Bible. This is all the more obvious because, for example, Velikovsky, in his hypotheses, 52 years later, again scolds the earth with his Comet's tail, precisely at the same moment when the comet 0101181 8 commands the sun to stop. And indeed - Velikovsky writes with deadly seriousness - the sun did indeed stop at Joshua's word: the comet temporarily stopped the earth's rotation around its own axis. Although one cannot tell whether one is awake or dreaming when reading such things, there is - from a purely prehistoric point of view - not much trouble with Velikovsky's hypothesis. The real confusion actually begins only when Velikovsky evaluates every single detail of the biblical narratives as a real, actual event, and in his effort to prove this at all costs, he turns the entire Egyptian and pre-Asian chronology upside down, and mixes it up by about 600 years, in such a way that in his opinion, in the archaeological-prehistoric chronology of Egypt, about 600 years appear twice, i.e., in Velikovsky's view, international Egyptology discusses the same series of events in Egyptian history again 600 years later as new events. Of course, Velikovsky is forced into this new chain of assumptions again because he had previously committed himself to the exodus of the Israelites from Egypt in the 15th century BC, which date, in turn, compels him to mix up the Egyptian chronology. In the 15th century, at the height of the power of the 18th Egyptian dynasty, not only all of Palestine, including the Sinai Peninsula, but also the greater part of Syria was an Egyptian colony; at this time, therefore, > comet tail here, comet tail there — it would have been in vain for the Israelites to flee to the Sinai Peninsula, and later to Palestine, because here and there they would have reached Egyptian territory, that is, they would still have been within the borders of Egypt. In other words: the whole exodus would have made no sense. Therefore, for Velikovsky — if he did not want to cast doubt on the 480-year data of the Bible on the time between the exodus from Egypt and the construction of Solomon's temple — there was no choice but to date the so-called collapse of the Middle Kingdom of Egypt, which in reality occurred in the 18th century, according to the definition of Egyptology. With the help of this chronological manipulation, he could freely migrate the Israelites wherever he wanted during the turbulent times that accompanied the collapse of the Middle Kingdom, because they

% In those times, Egypt lost all its Asian colonies, meaning that there would have been a real historical and political opportunity for the Israelites to be expelled, 925

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Velikovskiy's correction of the Egyptian chronology, however, had endless consequences. For if he had dated the collapse of the Middle Kingdom to the 15th century, or even to its end -- as we saw above, according to the Bible's questionable 480-year data, the exodus from Egypt would have occurred between 1440 and 1406 -- then he was also forced to place the Hyksos rule in Egypt sometime after the 15th century in Egyptian history. Since, according to the data of real Egyptian history, the Hyksos were already driven out of Egypt around 1580 BC, and the Egyptian New Kingdom was established around 1440-1406 in the 17th century, was at the height of its power under the rule of the 1st dynasty, Velikovskiy had no other option than to overturn the chronology of the entire New Kingdom of Egypt, along with the actual period of Hyksos rule. Thus, according to Velikovskiy's "New Egyptian Chronology", the Hyksos would have occupied Egypt only after the exodus of the Israelites, sometime around the turn of the 15th/14th century, and would have ruled there for more than 400 years (in reality, the Hyksos' rule lasted only about 100-120 years, between about 1700 and 1580 BC), and only during the time of the Israeli kings Saul and David, that is, around 1000 BC, would they have been driven out of Egypt by the first pharaohs of the 18th dynasty, but not alone, by their own strength, but primarily with the effective help of the armies of Saul and David, without the intervention of these allied armies, the Egyptians - at least according to Velikovskiy - would not have been able to shake off the yoke of the Hyksos.

As a result of this chronological correction, the Egyptian queen Hatshepsut of the 18th dynasty, who actually reigned around 1500, also gets her role in Velikovskiy's "new Egyptian chronology": Velikovskiy names her the Queen of Sheba, who, according to the Bible, visited Solomon in Jerusalem, because - Velikovskiy simply explains - the sea expedition sent by Hatshepsut to Punt can obviously be nothing more than the journey of the Queen of Sheba to Solomon. And here Velikovskiy is quite openly and blatantly mystifying: he claims that Hatshepsut herself traveled to Punt, while the Punt inscription in question clearly and unambiguously states that the queen sent an expedition to Punt. And it is only natural then -- since a truly genuine and genuine "story" definitely requires a certain amount of well-dosed piquancy -- that Hatshepsut, in her role as the Queen of Sheba, according to Velikovskiy, incidentally also becomes Solomon's occasional concubine, but this time not even based on the Bible -- in which there is no mention of such a thing, following the rabbinical Talmud and Haggadah tales, which Velikovskiy refers to as first-rate historical sources on this issue. He only notes in passing-926

—, but something, then two millennia later, a knowledge

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Let us note that the Egyptian artist depicts blacks as the inhabitants of Punt in the picture next to the Punt inscription in question. The hardest nut to crack for Velikovskiy's theory, however, is not even the absurdities listed so far - although these are rare indeed - but the cuneiform tablets in Akkadian-Babylonian found near Tell el Amarna in Egypt, which contain diplomatic correspondence from the Egyptian court archives between two pharaohs of the 18th dynasty, Amenhotep III and Amenhotep IV, circa 1410-1360 BC. In these letters, among others, the correspondence of the independent kings of Babylonia-Kassia, Assyria, Mitanni and Cyprus, as well as of a host of petty kings and city-princes of Palestine and Syria, who were in vassal relations with Egypt, with the two mentioned pharaohs has been preserved for posterity. These Amarna letters are in sharp contrast to Velikovskiy's "new Egyptian chronology" because several Mitanni, Babylonia-Kassia and Assyria kings appear in them by name, and these kings named without exception ruled in the period between the end of the 15th century BC and the middle of the 14th century, and not only based on the Egyptian chronology - the validity of which Velikovskiy doubts - but also completely independently of it, according to the lists of the Assyrian and Babylonian king lists that have survived to us. In other words, there can be no doubt that the Assyrian, Kassu-Babylonian, and Mitanni kings mentioned by name in the Amarna Letters actually ruled during the 15th and 14th centuries BC.

It will therefore not be uninteresting if, in the light of this real historical situation, we examine more closely what logical somersaults Velikovskiy uses to reconcile the Proto-Asian kings mentioned by name on the Amarna tablets with his "new Egyptian chronology" and to date them from the 15th-14th century BC to the 9th century, more precisely between 870 and 840 BC, i.e. to identify them with the Assyrian and Babylonian kings who ruled between 870 and 840 BC. The name of the Mitanni empire has not been mentioned in this context because it is known that independent Mitanni existed only until about 1350 BC, when it was divided between Assyria and the Hittite empire. For this reason, Velikovskiy does not even attempt to identify Mitanni kings with the 9th century BC. In the 9th century, but instead, "with noble generosity," he overlooks the fact that several Mitanni queen names occur in the Amarna letters, and in his 9th-century identification attempts he simply "forgets" about these Mitanni queen names.

For this "rejuvenation" of the queen names by more than 500 years, Velikovskiy primarily uses the well-known fact that the kings of the ancient East used several names. For example, the pharaohs usually had five wives (throne name, personal name, and three adjective names, so-called "epithets"). As a result, Velikovskiy writes, there is a limit to the number of wives.

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The possibility that we find the names of the kings of Palestine in the Amarna letters in the form in which we know them from the Holy Scriptures has been raised. (L. Velikovskiy: Vom Exodus zu König Achnaton 1962, p. 254) With this rabulistic approach, he simply overcomes all constraints, especially the fact that none of the proto-Asian queen names in the Amarna letters resemble - not even approximately - the names of 9th-century Palestinian, Babylonian or Assyrian rulers. At the same time, his attempt 15 to connect the whole subject primarily to the names of the dozen Palestinian kings is evident, thereby diverting attention from the much more important fact that the Mitanni, Assyrian and Babylonian-Kassu kings mentioned by name in the Amarna letters appear under the same names in the Assyrian and Babylonian king lists, and even in their own inscriptions, as they are called in the Amarna letters. Velikovskiy's second trick on the issue of queen names is that he tries to identify the serial, uninterrupted wars of the 9th century between Judah, Israel and the Aramaic kingdom of Damascus, mentioned in the Bible, with the constant local hostilities described in the Amarna letters, which mainly raged without interruption between Abdi Asirta of Amurru (Syria) and the various Palestinian city-kings. The background to these local conflicts, as clearly demonstrated by the numerous testimonies of the Amarna letters, was the king of the northern Syrian state of Amurru (the name is the same as the name of the Semitic Amorites who gradually took possession of the whole of Mesopotamia between about 2100 and 1800 BC and finally united the old Sumerian and Akkadian territories under the rule of the Amorite Babylonian king Hammurabi), the aforementioned Abdi Asirta, who was an Egyptian vassal, but with an excellent political sense, sensed the weakening of the Egyptian colonial administration already during the reign of Pharaoh Amenophis III and tried to make himself independent of the Egyptian overlord. In addition, however, he also tried to dissuade the other Egyptian vassal kings in the neighboring northern Palestine from Egypt, sometimes with fine words, sometimes with force. The other Egyptian vassal kings mostly reported their activities in this direction to the Egyptian court "by return mail", and as a result, these constant accusations against Abdi Asirta are a recurring theme in the Amarna letters. In this endeavor, Abdi Asirta relied on the Hittite empire in Asia Minor, which was gaining strength at that time, as we can read in several letters found in Amarna. Meanwhile, Abdi Asirta did not forget to assure the pharaoh again and again of his unwavering loyalty in letters he wrote to Egypt, which are literally dripping with humility and have also survived in the Amarna archive. - This small digression was necessary mainly because it shows the details

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The Amarna letters provide great information about the political situation in the Middle East at that time, and how well we know the conditions there in the 15th-14th centuries. Thus, we can also gauge how much trouble Velikovskiy has hit when he wants to date all these events to the 9th century. After all, we can no longer be surprised that Velikovskiy, in this forced situation, does not hesitate to name the supporter of Abdi Asirta's independence aspirations, who, according to the overwhelming evidence of the Amarna letters, was the Hittite great king, as the 9th century Assyrian king and to identify him with Sulman III Asaredu (Salmanassar). He does this despite the fact that -- we repeat -- the Amarna letters clearly and without any ambiguity state that Abdi Asirta's secret ally and powerful supporter was the Hittite great king, and not the Assyrian king, who - also according to the evidence of the Amarna letters - was then, in the first half of the 14th century

In the middle of the 1st century, he had just shaken off Mitanni's centuries-long supremacy over Assyria, and could not even think of waging war beyond the immediate vicinity of the city of Assur or of interfering in Syrian-Palestinian affairs. Similarly, the Phoenician king Gubla (Byblos), who often appears in the Amarna letters, becomes, in Velikovskiy's hypothesis, the "alter ego" of the king of Israel, from Gubla, i.e. the famous Byblos commonly known throughout antiquity, the city of Jezreel; from another Phoenician city, also appearing in the Amarna letters, from Sumer, Samaria. Furthermore, the expression "sa-gaz-mes" used repeatedly in the Amarna letters, meaning "robbers", with the ending "mes" indicating the plural, which expression is used to designate the nomadic allies of the Palestinian kings Abdi Asirta who wrote the Amarna letters, is, according to Velikovskiy's interpretation, none other than Mesha, the king of the Ammonites living east of the Jordan River, of whom we know an inscribed stele from the 9th century, in which the text tells of his victory over Israel. -- All this green stuff can be read in Volume I of the Velikovskiy series "Zeit alter im Chaos", which has a separate title: királyiq).

From the Exodus to King Akhenaten" (A kivonulastól Akhenaten

In another volume of the series ("Ramses II und seine Zeit" -- Ramses II and his time) Velikovskiy names Ramses II as the contemporary of the 11th Babylonian king Nabu-kudurru-usur (Nebuchadnezzar), i.e. he places him from the 6th to the 13th century BC to the 7th-6th century BC. From the well-known peace treaty of Ramses II with the Hittite great king Hattu-Silis III, of which both Egyptian and Hittite versions have survived to posterity (the Hittite version was found in Boghazköy in Anatolia, the former Hittite capital), and in which all the names and titles of both rulers are listed in full, Velikovskiy imagines a Neo-Babylonian-one-ipto-929

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what treaty will be, and Ramses II will of course be Nebuchadnezzar's contemporary, Pharaoh Necho II. And Velikovskiy does all this despite the fact that in the text of the aforementioned Egyptian-Hittite treaty every single name of Ramses II is listed, and among the names there is no name that can be somehow connected with the name "Necho". After this, of course, we are not surprised when Velikovskiy also changes Ramses II's battle of Kadesh against the Hittites into the battle of Carchemish of Necho II, in which the Egyptians fought against the Neo-Babylonian army. As a result, the year of the Battle of Carchemish, 605 BC. "of course" the Battle of Kadesh, which took place in the 1290s BC, is also transferred to the

Velikovsky's so-called "evidence" with which he tries to support this "reconstruction" that is completely out of thin air is also particularly characteristic. These "evidence" are all limited to transparent general correspondences that can be found consistently in almost every war in world history. One of his main arguments for identifying the two battles, for example, is that the Egyptians lost the battle in both cases, which, according to him, also indicates that they are the same event. The attentive reader may have noticed from what has been said so far that the existence of the Hittite empire does not fit into Velikovsky's concept at all, because if he were to admit its former existence, he would have difficulty in claiming that Ramesses II was identical with Necho II and that Ramesses II's battle of Kadesh was none other than Necho II's battle of Carchemish against Nabu-kudurru-usur II in 605 BC. Thus, Velikovsky can naturally only bring up this part of his hypothesis at all at the price of having to deny outright the existence of the Hittite empire in the 14th-13th centuries BC and Hattusilis III, Ramesses II's ally — having no other choice, he is forced to present him as the "alter ego" of Nabu-kudurru-usur I. In other words, once again — who can remember how many times — with this transparent mystification he tries to overcome the ever-recurring "difficulty" of his concept, that in the surviving written records the names of the various Middle Eastern rulers he interprets as the same persons are always different from each other without exception. This is when he comes up with the "alter ego" evasion. In identifying Hattusilis III with Nabu-kudurru-usur II, however, he has again forgotten the fact that, apart from Ramesses II, all the names of Hattusilis III are also listed in the text of the Egyptian-Hittite peace treaty mentioned above, and among the names, no name comparable to the name Nabu-kudurru-usur occurs.

Another very typical example of Velikovsky's rabulistic "evidence" procedures is the following. From the Hittite great king Hattusilis III, the aforementioned ally of Ramesses II, who ruled from about 1286-1260 BC to 930 AD

===== PAGE 919 =====

reigned, we know of a letter he wrote to the then Babylonian king Kassu, Kadasmandililinc 11 (reigned ca. 1274 to 1268), act

The Hittites had been allied with him since the reign of his predecessor, Kadasmanurgu (c. 1292-1274), against the rising Assyria. In this letter, Hattusilis tried to persuade II. Kadasmanlil, that they would attack Assyria together, and - according to the international diplomatic custom of the time, which we can prove many times from the Amarna letters, 8 he naturally called the independent Babylonian king, who was equal to him, "my brother", as was the etiquette of the time in the exchange of letters between equal rulers. Well, from this Hattusilis, he was the brother of Muvatallis, so he would be identical with the From the address "my brother", Velikovsky concludes that the Babylonian king is therefore a blood relative of Hattusilis III, namely Hittite king Urhitesup (reigned between ca. 1293-1286), the predecessor of Hattusilis III, whom Hattusilis dethroned and exiled to an island. Hattusilis III himself informs us about all this in an inscription. According to Velikovsky's idea, the above phantasmagoria would prove that the Hittite empire was identical with the Babylonian empire. In this mystification, the only thing that is shocking is not that Velikovsky simply identifies the Anatolian Hittite Empire, which was at the peak of its prosperity in the 14th-13th centuries BC, with the so-called Neo-Babylonian or Late Babylonian Empire, which existed in the 7th-6th centuries BC, but even more so is the open, undisguised cynicism with which Velikovsky naturally ignores the decisive fact in this regard, that III.

Hattusilis II wrote his letter of inquiry to Kadasmanlil as a Hittite great king, at a time when his brother's son, Urhitesup, was not ruling anywhere but was living in exile on an island. Velikovsky was an unfortunate exile on an island, deprived of all thrones, and at the same time, under the Despite the efforts of the "alter ego", it is therefore obvious that this Urhitesup could not have been simultaneously name of Kadasmanlil II, the king of the Babylonian part of the joint Hittite-Babylonian empire that Velikovsky imagined.

The absurdity of Velikovsky's "new Egyptian chronology" and the untenability of his assumptions are also clearly demonstrated in the fact that he completely disregards the fact, indisputably proven by contemporary Egyptian records, that Ramesses II reigned for 66 years. Velikovsky places Ramesses II on the throne in 610 BC. Ramesses. It is essential to note here that he cannot date the beginning of Ramesses' reign earlier because he had already committed himself to the date 610, when he identified the Battle of Kadesh with the Battle of Carchemish, which we know for sure took place in 605 BC, and on the other hand, it can be stated from contemporary written Egyptian sources that Ramesses fought the Battle of Kadesh in the 5th year of his reign. Therefore, since on the one hand it is undoubtedly certain that Ramesses

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It is possible to say that the Battle of Kadesh took place in the 5th year of Ramesses, but on the other hand there can be no doubt that the Battle of Carchemish took place in 605 BC. Velikovsky was only able to reconcile these two dates if he dated Ramesses II's accession to the throne to 610 BC. Going further, we also know for sure that the leader of the Babylonian army in the Battle of Carchemish was the then heir to the throne, Nabu-kudurru-usur, and that shortly after the battle he received the news of his father's death, i.e. Nabu-kudurru-usur's accession to the throne occurred in 605. Velikovsky does not doubt this. However, the confusion in this area begins when Velikovsky begins to talk about the fact that during the madness of Nabu-kudurru-usur, in the last years of his reign, Pharaoh Merneptah II Ramesses' successor invaded Phoenicia. — In passing, we should note that the alleged madness of Nabu-kudurru-usur II is only mentioned in the Bible, no contemporary source knows about it. — What is much more important in the present question, however, is the fact that y. Nabu-kudurru-usur ruled for 45 years — some say a maximum of 48 years — which means that — counting 48 years — his last year would be equal, at least in Velikovsky's "new Egyptian chronology," to the 53 years of Ramesses II. In other words, Ramesses II would have to outlive Nabu-kudurru-usur by at least 13 years, even if he had really ruled during the time of Nabu-kudurru-usur, as Velikovsky claims. Thus, Ramesses II's successor, Merneptah, could have had difficulty in entering Phoenicia during the last years of Nabu-kudurru-usur's reign. At the end of the volume entitled "Rameses y und seine Zeit" (pp. 1, 235-240), Velikovsky tries to resolve this capital contradiction by referring to Virchow, a renowned 19th century medical professor, who, according to him, The mummy of Ramesses is not very old. The 66 years must therefore mean, Velikovsky reflects, that Ramesses II was his father's co-ruler from an early age and counted the years of his reign from the beginning of his co-reign. However, this soon turns out to be a cheap excuse and a side story on closer examination. The contemporary written sources, in addition to speaking unanimously of Ramesses II's reign of 66 years, also state — as we have already mentioned — that Ramesses II in the 5th year of Ramesses' reign, he fought the Battle of Kadesh, or according to Velikovsky's "New Egyptian Chronology", the Battle of Carchemish, which took place in 605 BC. However, we also have contemporary written information that Ramesses II himself personally commanded the Battle of Kadesh, or according to Velikovsky, the Battle of Carchemish, and even significantly influenced its outcome with his personal valor when, standing at the head of a chariot attack, he broke through the ranks of the miserable Hattians - as he himself writes. Therefore, we must start counting the years of Ramesses II's reign without any doubt 5 years before the Battle of Kadesh, as he himself

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Velikovsky also 10521, in 610, i.e. 5 years before the Battle of Carchemish, which he believes can be identified with the Battle of Kadesh — enthroned Ramesses II. Then, in his volume "The Sea Peoples" ("Die Seevölker"), Velikovsky places Ramesses III (reigned between c. 1198 and 1166 BC) in the 4th century BC, the period when the Egyptians temporarily shook off the Persian yoke (between c. 390 and 340 BC). Accordingly, Velikovsky's so-called "Sea Peoples", who repeatedly attacked Egypt between about 1220 and 1180, become mercenaries in Persian service, and the "Sea People" called Peleset (these are the well-known Philistines from the Bible, from whose name the country "Palestine" derives) are reduced to "Persians", on the grounds that in Egyptian writing there was no distinction between "P" and Peleset = Pereset, i.e. Persian. — Incidentally, and very instructively, Velikovsky does not take the Bible literally this time, in which, as is probably well known, the between the r" sound, so Philistines appear before the kings Saul and David, i.e. before 1000 BC. Let us think of the legend of Samson and the Philistines.

It is clear here that for Velikovsky even the Bible is not authoritative if it contradicts his own ideas. But apart from all this, in view of Velikovsky's brilliant explanation of the name, it is also quite incomprehensible that the name of the former country in the lawsuits is not Palestine, but Palestine. He also In his volume "Die Seevölker" (in the appendix), Velikovsky uses fabricated excuses to cast doubt on the fundamental pillar of the Egyptian absolute chronology, the mentions the so-called "Sirius periods". It is typical of his "argumentative" manner, for example, that he wants to cast doubt on the credibility of the ancient authors Censorinus and Theon, who both recorded when a new Sirius period began in Egypt, by saying that in his opinion both authors were epigones, so their statements do not deserve credit. However, he "factually" overlooks the fact - which is of paramount importance in this regard - that the data provided by the two ancient authors regarding the beginning of the Sirius period agree to within a year (according to both of them, 1321 BC was the beginning of a Sirius period). He then lists in detail the enormous sources of error of the "C" method, even under normal circumstances, and especially when, according to Velikovsky's own theory, the date of the beginning of the Sirius period is 1321 BC. We also take into account the repeated huge ionizations caused by the cosmic catastrophe that occurred twice in the 15th and 14th centuries. All this is correct in itself. However, it is completely incomprehensible that immediately after this, without any transition or explanation, he suddenly starts talking about how he hopes to definitively prove his "new Egyptian chronology" with the help of "C" measurements, i.e. his idea that the individual dynasties in Egyptian history are 500-800 years younger than Egyptology has shown,

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Recent developments around Europe's cultural priority

The "methodology" of those attempts to date the beginning of the European Neolithic to an earlier date and to make it as independent as possible from the Near Eastern Neolithic, i.e., to evaluate the earliest Neolithic archaeological cultures of Europe as - at least relatively - an independent, autochthonous local development, is very instructive in the recently published (1989) collection entitled "Siedlungen der Steinzeit" (Spektrum der Wissenschaft, Verständliche Forschung series), although it should be noted that the author of every article in which the above question arises, in principle, without exception, and clearly takes the position that Neolithic development, i.e. agriculture and animal husbandry, spread to Europe from East Asia. Despite this repeatedly and unambiguously stated principle, however, in more than one article of the volume, some authors strike rather strange tones — and very significantly different from the above unanimous principle.

For example: In their study "Die Bandkeramik in Mitteleuropa: von der Natur- zur Kulturlandschaft" Jens Lüning and Petar Stehlí discuss that since in one or two Rhineland settlements of the cultural circle of banded pottery, the beginnings of which can be dated to around 5300 BC based on uncalibrated "C" measurements, cardium ceramics of the Western Mediterranean type were also found, therefore the cultural circle of banded pottery did not arise through the further expansion of the agricultural cultures of the South-East, but was to some extent the result of an independent development, since cardium ceramics originate from the Western Mediterranean region. — That however, the cardium ceramics of the Western Mediterranean are also of Pre-Asian, more specifically Syrian and Cilician origin (1. Our book IV. p. 716 or Müller-Karpe 1968. p. 271), just as like the agricultural cultures of southeastern Europe mentioned by the authors — from which Lüning and Stehlí so zealously try to separate the cultural sphere of ribbon-decorated pottery — also

They come from the East, but of course they prefer not to talk about it. Here again, one aspect of the astonishing illogicality that we have already presented in its full form in the theories of Renfrew and Dayton described above is clearly perceptible. But we can also find very nice concrete examples of the unreliability of C* measurements in the aforementioned volume. For example, in their paper entitled "Die ersten Bauern in Polen" Peter Bagucki and Ryszard Grygiel date the strip-decorated pottery settlement near Brzesc Kujawski they excavated between 5300 and 4800 BC based on calibrated C* measurements, while according to the aforementioned article by Lüning and Stehli, the age of the strip-decorated pottery culture is uncalibrated, i.e., according to the C* measurements, which show an age of a good 800 years younger than the calibrated measurement results, it is dated between 5700 and

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4900, which dating is dendrochronologically pushed back, as we said earlier, to about 800 years earlier, i.e. about 6500-5700 BC. So there is clearly something wrong with the C measurements here.

Likewise, in John M. Howell's article "Jangsteinzeitliche Agrarkulturen in Nordwesteuropa" - also in the aforementioned volume, the complete confusion of the C* measurement results is more clearly visible. According to Howell, approx. Starting around 5500 BC, the banded pottery groups spread north and west from their original Carpathian Basin habitats, reaching the Paris Basin around 4500 BC. Howell also uses calibrated C* dates in this article and dates the earliest beginnings of the banded pottery culture circle to around 5500 with 626% confidence, while — as we saw earlier + Lüning and Stehli date the beginnings of the banded pottery culture circle to 5700 based on uncalibrated C* measurements, and the appearance of the banded pottery settlements in the Rhineland to 5300, which dates, calibrated for this period, would result in around 6500 BC and 6100 BC, respectively.

There are also striking contradictions in the chronological data of some of the articles in the volume when it comes to dating the megalithic culture of Western Europe. According to Glyn Daniel (C, Renfrew's predecessor in the Cambridge Department of Archaeology), in his article "Megalithische Monumente", the oldest megalithic structures are the megalithic temples of Malta, which, based on calibrated C* measurements, belong to between 4000 and 2000 BC. He dates the megalithic tombs of Spain, Portugal, England and Ireland, also based on calibrated C* dates, between 3800 and 2000 BC, and he writes about Stonehenge that its oldest version was built around 2800 BC. Daniel then concludes from these dates that all these megalithic monuments are of local origin, because they are older than the Egyptian or Sumerian cultures. Of course, the fact that such a thing can only be assumed at all at the cost of the inexplicably high C* dates of the European areas lying west and north of the so-called "C* fault line", plus the two-stage, completely unmethodical manipulation of the unilateral dendrochronological calibration of the European C* measurement results, while the related Pre-Asian C* age determinations are compared without calibration with the European dates obtained in the above way, well, Daniel is deeply silent about this unpleasant fact. On the contrary, he makes even more astonishing statements. Some menhirs are shaped like humans, he writes.

According to his own dating, these date from the end of the 3rd and 5th millennium BC (i.e. around 2200-2000) and according to Daniel: these — are the earliest sculptures in the round in the history of Menschheit." Which means in English: "... are the earliest Valdi in the history of mankind.
935

===== PAGE 924 =====
sculptural works." Glyn Daniel, the world-famous professor of archaeology at the University of Cambridge, has apparently never heard of, for example, the statue of Pharaoh Djoser (around 6,2780 BC), not to mention the numerous, even earlier Mesopotamian Sumerian sculptural works, such as the remains of a god found in Uruk from the IV, archaic layer, i.e. from the end of the Uruk period (1. Reallexikon Assyriologie, volume IV, p. 534), which date to around 3200 BC, both according to uncalibrated "C" measurements and based on the traditional stratigraphic comparison and stylistic parallel dating method. So, even uncalibrated, these Uruk god statue remains are a good thousand years older than the menhirs mentioned by Daniel and dated to around 2200-2000 BC with dendrochronologically calibrated dating.

In contrast, Colin Renfrew — According to his article "Die Megalith-Kulturen", in which we can also read dendrochronologically calibrated dates, the oldest megalithic monuments are found in Brittany and belong to around 4500 BC. Who should we believe now? Daniel or Renfrew? Where did the earliest megalithic monuments appear and when? On the island of Malta around 4000 or in Brittany around 4500? But there is another irresolvable contradiction between Daniel's and Renfrew's articles. We have seen earlier that Daniel dated the oldest version of Stonehenge to 2800.

Now, in Renfrew's article, we first read that the earliest construction phase of Stonehenge can be dated to around 4000 BC, but one page later he writes that the so-called "henge" monuments (such as Stone-, henge") appear around 3000 BC. However, all these inconsistencies and contradictions in Renfrew's study are not so significant if we take into account the much more important circumstance that Renfrew again explicitly acknowledges in this article that agriculture and animal husbandry spread from Greece and the Mediterranean coasts to Central, Western and Northern Europe. In doing so, he also formulated in writing the oral statement he made on Austrian television on January 4, 1985, in which he admitted that his hypothesis about the priority of the Neolithic in Europe over East Asia had been refuted by the latest excavation results, at least as early as January 1984; Renfrew's article under discussion was first published in German in the January 1984 issue of "Spektrum der Wissenschaft". Unfortunately, we were unable to find out the date of publication of the original English version before this. In any case, with this publication, Renfrew's hypothesis about the European priority of Neolithic development can be considered definitively settled,

936
===== PAGE 925 =====

Chapter 2
THE INDOGERMAN (INDOEUROPEAN) ÖSHAZA LEGÜJABB „VÄNDORLÁSAT",
Renfrew's new hypothesis

Anyone who carefully studies the theories presented above about the European priority of the Neolithic or metalworking will sooner or later be forced to recognize that behind all these ideas, quite obviously, lies the theory of the cultural priority of peoples speaking Indo-Germanic languages, which was widely promoted in European scholarship in the first half of the last century, or more precisely, the renaissance of this idea, a thesis that lost much of its former popularity after 1945 under the influence of the tragic consequences of the Germanic "Übermensch" ideology of Hitler's Germany.

Below, we will show with the help of specific examples that this is indeed the case with certain recent research trends. Our readers are already familiar with one or two frightening examples from Perlov and Kifsin's brainchild "The Message of the Tatars" and from Dayton's book discussed above, for example, when, according to the two Soviet authors, the Celts considered the ancient Slavs to be gods, or when Dayton speaks of the ziggurats and pyramids being built by the Indo-European "dominant race", But this moment is also illustrated in the study of Colin Renfrew, who was the "The Origin of the Indo-European Language Family" founder and most enthusiastic — "Spektrum der Wissenschaft" was published in German in the December 1989 issue. In this paper, Renfrew, who not so long ago had promoter of the thesis of the priority of the European (by this term, clearly perceptibly and unspokenly, "Indo-European") Neolithic over the Paleolithic, now at once turns his own previous theory on its head and fully recognizes without any restrictions that the knowledge of agriculture and animal husbandry undoubtedly spread to Europe from Paleolithic - more precisely from Anatolia, from the region of the Catal Hüyük cultural circle - and this cultural diffusion was accompanied by large-scale colonization by Anatolian farmers. In other words, he essentially says the same thing that we ourselves presented ten years ago in the volumes of "The Sun Rises in the East" published so far. In fact, the similarities go so far that Renfrew explains these large-scale colonizations originating from East Asia with exactly the same reason as we did: namely, the demographic explosion that necessarily accompanies the spread of the agricultural-animal husbandry economic form, the increase in population density.

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with a rapid increase in population, which forced the surplus population of the Proto-Asian agricultural cultures into repeated periodic migrations. So far, Renfrew has outlined the same global historical concept in this article, which is already well known to readers of his previously published volumes of "The Sun Rises in the East". However, when it comes to evaluating the linguistic effects and consequences of these mass colonizations originating from East Asia, our paths diverge sharply. And at the same time, Renfrew's actual goal, the inner driving force of his new theory, is clearly revealed here. Renfrew draws the astonishing conclusion from the archaeological situation outlined above that according to them, the ancient focus of the Indo-Germanic peoples and languages was probably in Central Anatolia, and it was from this region that the Indo-Germanics spread to Europe, via the migrations of agricultural colonists originating from Anatolia. Accordingly, according to his idea, the very first

The Indo-Germanic language was spoken in Europe by the carriers of the Thessalian proto-Sesklo culture who arrived from Anatolia, and according to Renfrew, this language would have been the direct predecessor of ancient Greek. Then, through further migrations and the accompanying geographical separation, Renfrew continues his line of thought, new and new dialects would have continuously developed or broken away from the common Indo-Germanic tribes, such as the proto-language of the Starévo-Körös-Karanovo cultural circle, the language of the ribbon-decorated pottery cultural circle, the proto-language of the Tripoli culture, the proto-languages of Italy, Iberian, France, North German-Scandinavia and the proto-language of England. Renfrew also presents this language separation hypothesis on a map.

It is already clear from this brief sketch that Renfrew clearly noticed the first-rate circumstance that was the main flaw of the previous Indo-Germanic concepts of the ancestral language and ancestral homeland — but of course also of the Finno-Ugric theories of the ancestral language and ancestral homeland — and which we ourselves have repeatedly emphasized in our previous works: namely, that at the fishing-hunting economic level the population density is necessarily extremely low — ranging between about 0.01 and 0.09 people per square kilometer — and this very rare population makes it practically impossible for some kind of semi-unified ancestral people, perhaps a common ancestral language, to develop under fishing-hunting conditions. It is quite obvious that this realization prompted Renfrew, after the hypothesis of the European priority of the Neolithic Age had become definitively untenable in the face of the constantly emerging world of contradicting archaeological finds, to make a surprising 180-degree turn and now simultaneously relocate the ancestral homeland of the Indo-Germanic peoples and tribes from Europe to Anatolia, that is, to a region that was one of the earliest centers of Neolithic farming - apparently in the sense of the saying that if the mountain doesn't move, the

938
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to Mohammed, then Mohammed goes to the mountain (i.e. if the beginnings of the Neolithic Age can be sought without any artifice in the region of the Indo-European ancestral homeland, which has been generally assumed until now, i.e. in Europe, as Renfrew has tried with all his might to point out for more than ten years –, then logically the ancestral homeland of the Indo-Europeans must be relocated to one of the earliest regions of Neolithic development. All signs indicate that such considerations are ultimately behind Renfrew's sudden turn of events. However, let us take a closer look at Renfrew's new Indo-Germanic homeland. It is immediately obvious that the hypothesis of an Anatolian Indo-Germanic homeland - of course, only considered in itself and abstracted from all other circumstances - would be much, much more logical than the previous conceptions of the Indo-Germanic, Finno-Ugric or other language family homelands, which did not even take into account the aforementioned demographic fact, but for almost two centuries have always sought the ancestral homelands of the supposed Indo-Germanic and other language families in areas where, in the times when the settled homelands were dated, we find only sparse fishing-hunting hordes, but no trace of agriculture or animal husbandry.

Despite all its internal logic, however, Renfrew's theory of an Anatolian Indo-Germanic homeland has a fundamental theoretical obstacle that brings the whole hypothesis to a standstill. This obstacle lies in the fact that Indo-Germanic studies have repeatedly and clearly demonstrated since the turn of the century — starting with Kretschmer — that the oldest geographical and toponyms, especially those of Anatolia and the Aegean region, as well as the Balkan Peninsula, the Carpathian Basin, and to a lesser extent the western Mediterranean — which number in the thousands — cannot be classified by any artificial means in the vocabulary of the Indo-Germanic languages, but rather belong to the so-called Anatolian-Aegean-Tyrrhenian language community, the languages of which language group were commonly used in the Mediterranean region before the spread of the Indo-Germanic languages to the Mediterranean. We have already dealt with the question of this ancient, pre-Indo-Germanic Anatolian-Aegean-Tyrrhenian language community in more detail (1. IV. of our book, pp. 845-851 and 858-880). In this connection, it is absolutely necessary to mention that the Indo-Germanization of the Mediterranean areas was a relatively late process. It only ended with the formation of the Roman Empire around the birth of AD. This is therefore a significant reason for exclusion that cannot be ignored by any excuse and is of such a nature that it makes impossible the idea that Anatolia was the ancestral home of the Indo-Germanic languages, but also the hypothesis that Indo-Germanic languages were spoken in Thessaly, the Balkan Peninsula, the Carpathian Basin, Italy and the Iberian Peninsula thousands of years before the birth of AD.

939
===== PAGE 928 =====

population. Renfrew's theory of an Anatolian Indo-Germanic homeland is therefore, in our opinion, not scientifically sound and cannot be taken seriously due to this absolutely excluding reason.

In contrast, however, certain other, one might say "side-insights" of Renfrew's, which he lists in his article to support his thesis, should be taken very seriously. For example, he flatly rejects the theory, which was almost universally accepted by Indo-Germanic studies until recently, according to which the Indo-Germanic peoples and languages spread to Europe and Southwest Asia through the conquests of nomadic steppe horsemen or charioteers. In connection with this issue, he deals separately with the criticism of Marija Gimbutas's "pānkurgān theory", which we ourselves have presented and refuted in detail in our book "Boncold is only a linguist!" (1. pp. 431-439). Renfrew also considers Gimbutas' hypothesis unacceptable. It is particularly important in this question that the archaeologist Renfrew also rejects Gimbutas' ideas on the basis of archaeological considerations, i.e. for the same reasons that we have mainly criticized Gimbutas' theory. Furthermore, it is very thought-provoking and instructive in Renfrew's article that, when discussing possible cases of linguistic change, he deals extensively with the issues of pidginization, creolization and the so-called "barbarian languages", as well as the possibilities of the development of lingua francas in areas where there was significant trade. All these topics and the linguistic phenomena related to them, which have not been given any attention by traditional European comparative linguistics, point again to the same perspective that we ourselves represent, with the only difference that we are looking for the antecedents of the Indo-Germanic languages in the various languages of certain European and West Asian Paleolithic-Mesolithic cultures, i.e., fishing-hunting economic groups, who then partly adopted the dialects of the agricultural colonists who arrived in large numbers and in several waves from East Asia as a lingua franca and partly pidginized them. As a result of these massive, repeatedly repeated convergent East Asian linguistic influences, so-called isoglosses developed between the originally different languages of the indigenous peoples; that is, in smaller or larger geographical areas, a certain degree of linguistic equalization occurred, with a greater or lesser number of common linguistic features; with vocabulary similarities, or parallels and grammatical correspondences, in the sense of Trubetzkoy's areal linguistic equalization, language association 018161016.

These areally balanced linguistic alliances were then named language families by modern European linguistics, which began to develop in the first half of the 19th century; from the similarities in vocabulary, point-940

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In other words, drawing the conclusion - now clearly visible, very hasty + from the parallels and grammatical correspondences - that the languages in question, which were indisputably related to each other, must have been the family tree of a common ancestral language of a common ancestral people. They arose through the branching of languages. This is how the various "language families" hypotheses were developed to explain the relationships between languages considered to be related to each other in terms of descent, that is, genetically related, not only to Indo-Germanic, but also to explain the similarities between the Uralic-Finno-Ugric, Altaic-Turkic and Hamito-Semitic languages.

At this point, however, we would like to point out the very important fact that at the time in the 19th century, when the aforementioned concept of "language families" as languages understood by those who were related by descent to each other was born, prehistoric and archaeological research practically knew nothing about those huge migrations, or more precisely, colonizations, repeated over millennia, during which, after the emergence of the Neolithic Age, masses of pre-Asian agricultural settlers flooded into almost all of Europe and Western Asia. Also unknown to research were the demographic data according to which the fishing-hunting economic stage could only ensure the subsistence of a negligible population density. This population density, as we have already mentioned above, ranges between 0.01 and 0.09 people/km², depending on the ecological environment of the given geographical region. In other words, this means that only about 1-9 people can make a living from fishing and hunting per 100 km²

Under such circumstances, we cannot accuse 19th-century linguistics and prehistory research of not taking into account circumstances or exclusionary reasons when formulating its language family concepts that were still completely unknown at that time. However, we must accuse our contemporary linguistics and prehistory research of not being willing to thoroughly rethink and revise the genetic language family theory of 19th-century origin to this day, even though the mass of archaeological finds that has accumulated in the meantime clearly showed a good 50 years ago that the concept of genetic language families was fundamentally flawed and in need of revision in every respect. In the last 50 years, these archaeological data that clearly refute genetic language family theories have multiplied into an almost innumerable collection of finds, and the aforementioned demographic data have also become well-known, but linguists still do not are willing to draw the necessary conclusions and continue to promote the idea of genetic language families unchanged. — Renfrew, despite all his other insight, does not recognize the need for this revision, because he continues to

941
===== PAGE 930 =====

He thinks in the category of Indo-Germanic languages that are genetically related to each other and flatly rejects Trubetzkoy's thesis that the Indo-Germanic languages arose through a process of unification similar to a dialectal language leveling, saying that Trubetzkoy's theory is not accepted by most linguists. In sharp contrast to Renfrew's opinion, however, in our previous works we presented a whole range of internationally recognized capacities of Indo-Germanic linguistics — including J. Pokorny himself, one of the editors of the most recognized Indo-Germanic etymological dictionary to date, quoting extensively from their publications, from which anyone can conclude that Trubetzkoy's theory of areal language leveling was indeed accepted by many renowned Indo-Germanic linguists, and at the same time the earlier language family tree theory was roundly rejected (1. e.g. our book, pp. 94-95, 406-407 and 549-559). Through careful methodological analyses, these researchers came to the conclusion that the attempted reconstruction of the previously assumed common Proto-Indo-Germanic basic language using linguistic tools, namely the theses of "sound laws" and "regular sound variations", is in principle impossible - even if such a common proto-language had ever existed. They also established that an Indo-Germanic primitive people never existed and that the term "Indo-Germanic" is a purely linguistic concept that has no specific ethnic meaning (1. in the places cited above).

In the light of these data, we can conclude that Renfrew — although he shows surprising insight in several detailed questions (and here we consider it important to point out that the cases in which Renfrew's insight is demonstrated almost always belong to his narrower profession, archaeology), but in the most essential question of the content of the study, namely the discussion of the origin and realistic kinship relations of the Indo-Germanic languages, he completely adheres to the Indo-Germanic school that the Reallexikon der Assyriologie calls the ahistorical trend (1. our quote above in this book). Renfrew apparently does not want to acknowledge that in addition to this ahistorical school, there is another Indo-Germanic trend, which is precisely what he calls a commonplace

It is based on Trubetzkoy's theory of areal language equalization, which was rejected outright with a meaningless and even unrealistic statement, and denies the former existence of the Indo-Germanic ancestral people and the common ancestral language, relying on convincing, concrete archaeological and ethnographic evidence of such weight that the "ahistorical" Indo-Germanic school and the genetic Indo-Germanic language family theory have never been able to present during their entire century and a half of existence.

942
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However, in the concluding part of his article, Renfrew again reports very thought-provoking observations. He explains that just as he assumes that the ancient Neolithic center of Central Anatolia (Catal Hüyük, Hacilar) is the place of origin of the Indo-Germanic languages, so too the two other, oldest agricultural-animal husbandry regions known today, namely Palestine-Syria (Jericho, Mureybet) and the Zagros Mountains region (Ali Kos, Gang Dareh Tepe, etc.) were presumably the birthplace of two other language families: Palestine-Syria, he believes, may have been the area of origin of the Hamito-Semitic-Berber (or Afro-Asiatic) language family, and the Zagros Mountains region of the Dravidian languages. Finally, Renfrew emphasizes that the idea presented in the article is a 088% hypothesis that still needs to be supported from several sides and the final result will obviously be much more complex than the picture he has outlined in the study. , However, I am certain — Renfrew concludes his article — that the spread of agriculture from Anatolia to Europe will form an essential part of the overall picture developed by future researchers." — We completely agree with this last sentence, but of course in the sense that the migrations from East Asia to Europe form a very essential part of the extremely complicated process of language development and language equalization, the final result of which was the formation of the Indo-Germanic languages.

If we now carefully follow Renfrew's line of thought that the three most ancient Neolithic foci known today could have been the areas of development of three so-called "language families" in West Eurasia, then the discussions that we discussed on pages 708-710 and 858-859 involuntarily come to mind as a comparison. There, it was discussed that the ancient Neolithic foci known today in Neolithic East Asia, or more precisely, the languages that gradually developed from them over time, Within the boundaries of the Neolithic and Chalcolithic archaeological cultural circles, which had visibly more and more uniform finds as they progressed and at the same time expanded to larger and larger areas, the enormous increase in population and the increasingly intense permanent close contacts had to lead to the development of more or less areally equalized common languages — lingua francas — sooner or later. In the places mentioned above, we have outlined in detail the most probable such ancient linguistic communities. Namely, in the area of the Catal-Höyük-Hacılar cultural circle, the ancient Anatolian language was spoken between about 6000 and 5500 BC (this is precisely the areal language community that Renfrew considers to be the common basic language of the ancient Indo-Germanic language family), then in the Samnarra-Tell Halaf cultural circle, the Syrian and Northern Mesopotamian language community. get c., BC, between 5500-4500, and also in Southern Mesopotamia at Eridu-943

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The 1st and 2nd periods of the Ubaid civilization produced a "lingua franca" roughly contemporaneous with the former, then between ca. 4500-3700 the 3rd and 4th periods of the Eridu-Ubaid civilization, i.e. the so-called northern 1108107 cultural circle, the lingua franca spoken throughout Mesopotamia, Syria and Western Iran, which was in all probability already Sumerian (or Elo-Sumerian), and finally from ca. 3700 onwards the Uruk culture also produced a "lingua franca" used in the latter vast region, which was certainly Old Sumerian, in addition to which the Elamite language also played an important role in southwestern Iran.

According to these, in ancient Pre-Austria, during the Neolithic and Early Metal Age, there was, with a near certainty probability, initially a more or less intelligible communicative language that arose through two or three areal language equalizations. These languages can be broken down into several linguistic states that are successive in time, based on the archaeological findings and the guidance of certain archaeologically well-defined cultural circles with uniform findings. Over time, these languages themselves became more and more uniform, parallel to the gradual unification of the cultural circles, which can also be clearly traced archaeologically, and finally, around 4500 BC, with the emergence of the northern Ubaid cultural circle, which extended from the Mediterranean to Central Iran, practically to the whole of East Asia, a single vast linguistic koine - lingua and so-called franca - developed in the East Asia region, which in all probability was initially identical to the direct predecessor of the Sumerian language, and later to the Sumerian language known from cuneiform inscriptions, until about 1800 BC it played a dominant role in the entire Middle East as the generally used state, commercial, cultural, scientific and, last but not least, sacred cultic language of religious ceremonies.

If we want to bring Renfrew's ideas regarding the proto-linguistic nature of the three most ancient Neolithic centers into line with the archeologically highly probable picture of a proto-Asian proto-linguistic system just outlined, then this operation is only possible with certain very important corrections. First: the Anatolian Neolithic center is an ancient transport language could not in any case have been the common Proto-Indo-Germanic language, as Renfrew believes - and indeed not the "Indo-Germanic, the basic language of the language family", but not even the areally equalized communicative linguistic antecedent of the Indo-Germanic languages in the sense of the arealistic approach, because Anatolia was the center of the so-called Anatolian-Aegean-Tyrrhenian proto-linguistic community, which international linguistics has generally defined as the pre-Indo-Germanic linguistic state, and from whose language those ancient, proven non-Indo-Germanic geographical and toponyms originate, which most often contain the following endings or stem extensions: —nt-, —nth-, —nd-, —ss-, —s-, —mn-, —re, -m-944

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get, wI, and —t-. e.g. Korinthos, Lakraunda, Halikarnassos, Kephisos, Methymna, Atabyron, Pergamos, Athenai, Kydre/os, Mileros (all this in more detail in our 1st book, 845-850 and 858-880, pp.)

Secondly: the oldest Greek language known specifically from written records from Anatolia, the so-called Proto-Hatti language spoken in Anatolia in the 3rd millennium BC, has nothing to do with the Indo-Germanic languages, as it is an agglutinative language. Thirdly: the ancient spoken language of the Syrian-Palestinian Neolithic center - as far back in time as we can look back with the help of archaeological finds - was certainly the direct predecessor of the spoken language of the Tell Halaf cultural circle, but this language, according to the evidence of the vast amount of data available to us, could not have been an inflected language (like the Hamito-Semitic languages), since the linguistic koine of the northern Eridu-Ubaid cultural complex developed largely from it, which - being the direct predecessor of the Sumerian language, and more than likely the Sumerian language itself - was undoubtedly an agglutinative language, just like its successor, Sumerian. Fourth: the question of the ancestral language of the ancient Neolithic center in the Zagros region is by no means as simple and straightforward as Renfrew presents it. It seems certain that the Elamite language developed from it, and it is also a valid statement that the Elamite language, in turn, is related to the Dravidian languages. In addition to these two aspects, however, we must also add that, on the one hand, the Elamite language is at least as closely related to Sumerian as it is to the Dravidian languages, but on the other hand, we must not forget that, according to recent research, the Sumerian language itself is related to the Dravidian languages (Book I, pp. 884-885). Moreover, we cannot leave out the Gutian, Lullubi, Kassu, Subar and Hurrian languages, which were once spoken in the northern part of the Zagros Mountains, from the paleolinguistic development of this region. It is extremely important in this respect that the languages just listed were all, without exception, inflectional languages and were to some extent related to Sumerian and Elamite. In the case of the presumed proto-language of the ancient Neolithic center in the Zagros region, the linguistic situation is therefore quite complicated, since we know several clearly distinct independent ancient languages, crowded together in a relatively small area — presumably due to the mountainous nature of the region, since the factors resulting in separate language development are already present in the valleys, which are more or less isolated from each other, as is proven by the linguistic diversity that has survived to this day in the Caucasus, for example.

From all this, we can draw the following lesson: if someone wants to draw conclusions about the proto-linguistic relations of the Zagros Mountains region, then, in our opinion, it is much more appropriate to use a kind of-945

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It should be stated that the proto-language of the ancient Neolithic cultural center in the Zagros region could quite obviously have been the predecessor of Sumerian, Elamite, and the other ancient languages of the Zagros region listed above, but at the same time it can most likely also be considered the crystallization center of several later inflected languages -- such as Dravidian.

The new theory of Th. W. Gamkrelidse and VV Iwanow

Just a few months after the publication of the sensational study just discussed by C. Renfrew, also in the columns of "Spektrum der Wissenschaft", in its May 1990 issue, Thomas W. Gamkrelidse and Wjatscheslaw W. Iwanow, in their paper "Die Frühgeschichte der indoeuropäischen Sprachen", also place the Indo-Germanic homeland in Anatolia, although, unlike Renfrew, they believe that it can be discovered not in Central Anatolia, but 1/- further east, in the present-day Armenian territories, in Northern Mesopotamia and Transcaucasia.

At the beginning of their article, the authors discuss at length the method of Grimm's "sound laws" and "regular sound changes", with the help of which linguistics has been carrying out the phonetic reconstruction of the words of the supposed "basic languages of a language family" since the middle of the last century. However, it is most surprising in this regard that in this extensive presentation, not a single word is mentioned about the eminently important fact already mentioned above, that in recent decades a whole series of the most renowned representatives of Indo-Germanic linguistics have roundly rejected the hypotheses of a common Indo-Germanic ancestral people and language, and with the help of in-depth methodological analyses have definitively shown that it is impossible to reconstruct a common ancestral language using the traditional "sound law" or "regular sound variation" method of language comparison (pp. 94-101, 407-427 and 549-552 of our book).

In short: Gamkrelidse and Iwanow do not take note of these new, sweeping insights into Indo-Germanism; for them, the genetic language family concept of 19th-century Indo-Germanism still seems to be the only possibility, and even a proven fact. Likewise, they still regard the so-called "linguistic paleontology" (linguistic paleontology) as a solid methodological basis for the geographical localization of the ancestral homeland of the inferred ancestral language, as well as for the determination of the social, economic and cultural relations of this hypothetical ancestral language. In this area, we are reminded that "and Indo-Germanic research has been a major contribution to the inference of the previously assumed common Indo-Germanic basic language." The Proto-Asiatic language Just dissect it, linguist!", where we showed with the help of ample citations from the literature that modern

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In addition to rejecting the idea of the existence of a common ancestor, he also flatly denies the applicability of the linguistic palimstry method to determine the homeland of the hypothetical Indo-Germanic people, or to assess the economic, social and cultural status of the presumed people in question. Then, reading the article, we must quickly note with astonishment that the authors of the article have no idea where the so-called "fertile crescent" lies. This area - as is quite common knowledge, even in less scientific circles - extends from Egypt in a semicircle through the fertile coastal strip of Palestine and Syria (therefore, according to Gamkrelidse and Iwanow, it extends along the coast to the Caucasus). After acknowledging this astonishing geographical ignorance, which is especially serious The "Fertile Crescent" (also known as the "Golden Crescent") stretches across northern Syria, Mesopotamia, and western Iran to the Persian because it occurs in close connection with the very Gulf. In contrast, the "Fertile Crescent" begins in the Balkan Peninsula and extends southeastward and eastward through Anatolia to the southern Black Sea. area that forms the scene of the authors' entire concept, let us also take a look at how the authors of the article imagine the common Indo-Germanic proto-language of eastern Anatolia - northern Mesopotamia that they assume. After placing the Indo-Germanic homeland in the easternmost regions of Anatolia, in Transcaucasia and northern Mesopotamia, they naturally begin their list with the Hittite, Palaian and Luwian languages (since these languages can already be called Indo-Germanic languages to a certain extent and have been known more or less from written records since the middle of the 6th and 2nd millennium BC). According to the authors, the Hittites, Palaians and Luwians were the first to break away from the common Indo-Germanic ancestral people and, migrating west, took possession of the central and western parts of Anatolia.

They further believe that the other Indo-Germanic peoples - that is, the bulk of the ancient people - first migrated east through the Iranian territories south of the Caspian Sea, where they gave rise to the Iranian and Indic branches, as well as the Tocharians, and then the other Indo-Germanic peoples, bypassing the Caspian Sea from the east in a huge semicircle, made their way west through the Ural River and the Volga and gradually populated Europe by expanding westward. As a result of this concept, the Doric migration is started from Western Anatolia - in direct contradiction to all previous ideas in this direction. According to them, the Dorians - and obviously also the Ionian and Aeolian tribes on the western Anatolian coast, i.e. the

Greekness as a whole — would have emerged from the Anatolian Proto-Indo-Germanic group (Hittite, Palae, Luwian), which, however, is in unavoidable contradiction with the division of the linguistic branches of the Indo-Germanic language family tree representation shown in the article, in which the Greeks together with the Indo-Iranian group form the common Indo-Iranian-Greek-Armenian proto-group, and this group is the

941

===== PAGE 936 =====

According to the figure mentioned, it would have split into Greek and Indo-Iranian only much later. In other words, there is a glaring contradiction here between the idea of the branching of the proto-language presented in the text of the article and the relationships between the language branches depicted on the language family tree.

Not to mention the other glaring contradiction that in the territory of the Indo-Germanic homeland assumed by the authors, according to every known historical, archaeological and written data, the Hurrian people group with a conjugative language lived in the second half of the 3rd millennium BC and in the 2nd millennium BC, and before that, this very area formed part of the geographical area that has been in memory since the first half of the 3rd millennium BC. According to the data available to us, these Subarcs or Subirs also spoke a conjugative language, and "Subir-kr"—that is, "Subar land" or "Subir land"—appears in the Sumerian script as "Subar-kr" or , one group of researchers considers them to be a people group closely related to the Hurrians (l. pl. Ungnad, 1936). Moreover, in exactly the same area, approx. Between 3250 and 1750 BC, the so-called "early Transcaucasian civilization" (in Soviet archaeology, the Kura Araxes culture), the bearers of which, according to our current knowledge, were the aforementioned inflected-language Hurrians. (l. Burney-Lang: Die Bergvölker Vorderasiens, p. 100) Then, in the middle of the 2nd millennium BC, in the same area — considered by the two Soviet authors to be the Indo-Germanic homeland — or more precisely in its southern half, the empire called Mitanni spread, whose population spoke the inflected-type Hurrian language. Moreover, in this area, in the first half of the 1st millennium BC, the Hurrian successor state called "Urartu" by the Assyrians existed, whose population used a late version of the Hurrian language of the 3rd-2nd millennium BC, i.e. also an inflected language. In other words, in the area designated by the two Soviet authors as the Indo-Germanic homeland, from the beginning of the 3rd millennium BC to the middle of the 1st millennium BC.

In the period — as proven by contemporary written sources — peoples with a continuous inflectional language inhabited the area. Incidentally, the area in question largely coincides with the territory of later Armenia, where — also proven by written sources — the Indo-Germanic-speaking Armenians appeared only after the fall of the Urartian empire, around 500 BC, but before that no trace of them can be found in the area.

This is how the actual population and linguistic situation of the area — we repeat: it can be proven continuously, without interruption, from the beginning of the 3rd millennium BC to around 500 BC — looks like, where Gamkrelidze and Iwanow believe they have discovered the ancestral homeland of the Indo-Germans. At this point, however, we must definitely mention the fact of eminent importance for us that the authors — although it can be clearly established from what has been said so far that they are followers of that "ahistorical" trend of Indo-Germanic studies, which school continues to adhere to the 19th century Indo-Germanic

948

===== PAGE 937 =====

to the Nychvcsald theory, and the Indo-Germanic languages are postulated to be very ancient according to this point of view (the authors, for example, estimate the age of the Indo-Germanic basic language at 6000 years), despite this, they still write that the Indo-Iranian proto-language branch only separated from the other Indo-Germanic languages around the middle of the 3rd millennium BC. However, this dating is still very

It is interesting for us, because at the same time, Hungarian Iranian studies, led by Professor Harmatta, already speak of independent Indo-Iranian proto-languages that diverged from the Indo-Germanic base language around 5000 BC (see Harmatta, 1968, and quoted in our book, p. 427). Hungarian linguistics is more orthodox in its Indo-Germanic aspect than even the most orthodox Indo-Germanicists - we must undoubtedly include Gamkrelidze and Iwanow among the followers of this most orthodox trend, because as we have seen in the previous sections, they have apparently not heard anything about the new Indo-Germanic insights, the rejection of the hypothesis of an Indo-Germanic primitive people and primitive language, and the principled denial of the possibility of deducing the previously assumed primitive language using the "tone law" method, or even if they have heard about these developments - which we can assume much more about them - they are in any case unwilling to acknowledge them. Moreover, they surpass even the most stalwart representatives of the orthodox "ahistorical" Indo-Germanic trend to date, as is undoubtedly clear from the lines quoted below. The original text of the German article is as follows: "Aus den Keilschrifttafeln von Hattusa geht weiter hervor, dass spätestens in der Mitte des zweiten Jahrtausends aus der indo-iranischen Gruppe die churritische Sprache entstanden war, die im Reich Mitanni an der südöstlichen Grenze Anatoliens gesprochen wurde und sich schon vom Altindischen (häufig Sanskrit genannt) und dem Altrianischen unterschied. Die Sprache kretisch-mykenischer Texte aus derselben Zeit, die in den frühen fünfziger Jahren von den britische Gelehrten Michael GF Ventris und John Chadwick entziffert wurden, entpuppte sich als ein bis dahin unbekannter Dialekt des Griechischen." In Hungarian: "The Hattusian Eklrások Tablets further show that by the middle of the second millennium at the latest, the Hurrian language, spoken in the Mitanni Empire, on the southeastern border of Anatolia, had developed from the Indo-Iranian group and was already distinct from Old Indic (often called Sanskrit) and Iranian. The language of the Cretan-Mycenaean texts from the same period, deciphered in the early 1950s by the British scholars Michael G. Ventris and John Chadwick, turned out to be a previously unknown Greek dialect."

We have already explained in detail the theory of the so-called "Aryan ruling class" of the Hurrian empire called Mitanni in the 2nd millennium BC (pp. 78-79 and 81-83 of our book 1). We could also see on these pages just cited that this hypothesis only stated that

949

===== PAGE 938 =====

that part of the leading stratum of the Mitanni Hurrians was Aryan, but it never occurred to even the most perverted German Indo-Germanist of Hitler's time to claim that the Hurrian language of the population of the Mitanni Empire, which, according to the clear evidence of numerous contemporary written records, was an inflected language that could be proven beyond all doubt, was a branch of the Indo-Iranian languages. This statement by the two Soviet authors therefore testifies to an almost unimaginable level of ignorance that is unparalleled.

But what is closely related to this is also what empire In the previously cited pages of the volume "Towards the Proto-Asian Proto-Language", we also showed our readers that the Mitanni language itself

The thesis of "the Aryan ruling class" was more or less generally held only by Indo-Germanic studies before 1945, while in the last four decades it was only disputed by representatives of the so-called "ahistorical" school, and even these with less and less conviction. However, no one, even among the "ahistorical" scholars, dared to declare the Hurrian language to be an Indo-Iranian language, as it were, "skin-to-skin". The dubious glory of priority in this matter therefore indisputably belongs to Gamkrelidze and Iwanow.

When viewed in the light of this hair-raising oversight, the authors' second attempt at mystification, which appears in the last sentence of the above quote, is virtually dwarfed.

We have seen that the language of the Linear B tablets deciphered by Ventris and Chadwick is simply referred to as Cretan-Mycenaean texts, whereas the texts in question are only relics of the late Mycenaean language and have nothing to do with the language of the Minoan culture previously spoken in Crete. This is a very important difference. Not to mention that, as we have discussed in detail earlier (in our book I, pp. 850-854), the Mycenaean language of the Linear B tablets — despite the decipherment by Ventris and Chadwick — can hardly be called

in the manner of the two Soviet authors, with noble simplicity, "Greek dialect", since, for example, in the text of the same Pylos tablet, the numbers of the ancient Greek language three (tri), the Latin four (quattuor) and the Hungarian language two are freely mixed. (Our book 1, p. 853). This linguistic state rather leads us to conclude that the language of the Linear B tablets dating from around 1200 BC cannot be called simply "Greek dialect", but only represents the earliest stage of the complex process of development of the ancient Greek language, when the lingua franca, the lingua franca used in the Mycenaean cultural circle, — within its framework, in its vocabulary, the words of several languages that later became independent and then developed separately — occurred together. That is, the Late Mycenaean language was quite obviously some kind of pidgin-like mixed language, and around 1200 BC, during the time of the Lincáris B tablets, the crisis and collapse of the Mycenaean cultural complex, the later Mycenaean language was just beginning.

950

===== PAGE 939 =====

the process of language development leading to the formation of the Ancient Greek language (and presumably the Latin language).

In the remainder of the article, the authors discuss in detail the Proto-Indo-Germanic basic language phonemes they have deduced, which largely differ radically from previous Proto-Indo-Germanic phoneme reconstructions. Browsing this section, the reader must involuntarily recall the very apt words of the Reallexikon der Assyriologie, already quoted above in this volume, about the hopeless methodological confusion that occurred in the conservative trend of Indo-Germanic studies after the recognition of the Indo-Germanic nature of the Hittite language: Since the twenties and thirties, the uncertainty caused by the Hittite language, the most important central issue of all inflected languages, with regard to the Indo-Germanic verb system, has in the meantime led to arbitrary doubts and almost arbitrary reconstruction versions regarding the reconstructibility of the Indo-Germanic base language..." (l. Reallexikon der Assyriologie, V. Band 1976-1980, p. 91). Well, we can hope for such an "almost arbitrary" reconstruction version in the case of the Indo-Germanic base language reconstructions presented by the two Soviet authors.

Then the authors unearth one by one the various "linguistic paleontological" inference methods found in the sinkhole of Indo-Germanic studies from 50-60 years ago. There is no lack of attempts to define the ancestral homeland based on reconstructed ancestral language words, as well as an outline of the economic life and cultural state of the ancestral Indo-Germans, of course also relying solely on the basic language words linguistically inferred using the phonetic law method. In other words, this is "linguistic paleontology" at its best, in its most ossified form. Of course, this ensemble - which in every way reflects the now long-outdated methods of Indo-Germanic studies of the 1920s and 1930s - cannot be left out of the previously particularly popular hypothesis of the domestication of horses by the Indo-Germans, nor the theory of Indo-Germanic war chariots, which was repeatedly and extensively re-examined and varied in the most diverse forms over the decades, with the help of which cultural transmissions the ancient Indo-Germans allegedly carried out their enormous migrations, during which they eventually conquered all of Europe and Western Asia.

Then there is another capital blunder in the text of the article, which testifies to the rather incomplete knowledge of the authors, and precisely in the field of archaeological cultures of the area where the Indo-Germanic homeland is believed to be located. They write that the vessels of the Tell Halaf culture have decorative motifs that are similar to those found in the somewhat younger Catal Hüyük culture, belonging to the 7th millennium BC.

951

===== PAGE 940 =====

also occur. To do this, we need to know that the real archaeological situation is exactly the opposite of what the authors write. The Catal Hüyük culture does indeed belong to the 7th (and 6th) millennium, but it is not younger, but rather a good millennium older than the Tell Halaf culture, which for its part existed between approx. 5500 and 4500 BC.

This complete confusion in the dating of Pre-Asian archaeological cultures, as well as other absurd statements of the authors already presented above, such as placing the area of the "fertile crescent" between the Balkan Peninsula and the Caucasus, or classifying the definitively proven inflectional type of Hurrian language among the Indo-Iranian languages

clearly show that Gamkrelidse and Iwanow have such massive educational deficiencies in the field of Proto-Asian archaeology, historical geography there, and even the ancient linguistic relations of the area in question, that they seem completely unqualified to establish any kind of ancient people or ancient language theory with a Middle Eastern relevance, whether in relation to the Indo-Germanic peoples and languages, or any other people and language group.

However, such shocking omissions are far from being exhausted by what has been introduced so far, but appear again and again throughout the article. For example, according to the authors, a hunting legend was recorded on one of the bilingual tablets of the Hattusa cuneiform archive "...in the then extinct Hurrian language, together with a Hittite translation." In Hungarian: "...in the then extinct Hurrian language, together with the Hittite translation." To do this, the following should be noted.

The written records of the Hattusa archive date back to between 1400 and 1200 BC. They cannot be from after 1200, since around 1200 the invasion of the so-called "Sea Peoples" destroyed Hattusa along with the entire Hittite empire, i.e. the archive is proven and indisputably from before 1200. The Hurrian language, on the other hand, was used relatively widely in Near East throughout the 2nd millennium BC, namely in the rather extensive area between the Zagros Mountains and the Mediterranean Sea in the east-west direction, and in the north-south direction from Transcaucasia to Palestine, as is undoubtedly proven by Hurrian texts found in several sites in the aforementioned area, or, for example, by the Sumerian-Hurrian dictionary in Ugarit on the Syrian coast. Moreover, in the first half of the 1st millennium BC, there existed in the Lake Van region approximately in the territory of what later became Armenia. The Hurrian-descendant population of the state called Urartu spoke the Urartian language, a direct continuation of the Hurrian language, even more than 600 years after the destruction of the Hattusa archive. In such circumstances, the above-quoted opinion of Gamkrelidse and Iwanow about the extinction of the Hurrian language before 1200 BC is only one more shining proof of the incredible degree of ignorance of the authors in the field of ancient Proto-Asiatic linguistic history,

952
===== PAGE 941 =====

It can be called downright grotesque that two individuals with such pitiful linguistic background want to propose a new theory on the question of the ancestral homeland of the Indo-Germanic population, which has been bitterly debated for more than a century and a half, but has still not been resolved. But it is also important in this context that the authors of the article themselves stated somewhat earlier in the same work that: "... the Hurrian language developed from the Indo-Iranian group at the latest in the middle of the second millennium..." (1. above). Now, this Hurrian language, which they believe only developed around 1500 BC or slightly earlier, is already being postponed at the time of the creation of one of the tablets in the Hattusa archive, that is, sometime between 1400 and 1200, but in any case definitely before 1200 - that is, barely 2-3 centuries after its development. (Presumably, the authors themselves felt that something was wrong with the classification of the Hurrian language among the Indo-Iranian languages, and therefore they are trying to postpone this Hurrian language, which is extremely disturbing in their circles, as soon as possible.)

The error that our authors made that the Indo-Germanic language Tocharian, once spoken in Eastern Turkmenistan, completely changed its consonant system under the influence of Finno-Ugric languages, also throws a rather sharp light on the level of linguistic knowledge of our authors. — However modest our knowledge of the literature in this field may be, we can state that so far we have not come across any data that would allow us to conclude that Finno-Ugric languages in Eastern Turkmenistan could have ever exerted any linguistic influence on Tocharian or any other language — considering that, according to our current knowledge, Finno-Ugric-speaking peoples have never lived in Eastern Turkmenistan.

As a result, we can state with a clear conscience that neither the theory of C. Renfrew presented above nor the joint article of Gamkrelidse and Iwanow on the Anatolian homeland of the Indo-Germanic peoples and languages can withstand objective criticism. Upon closer, more thorough analysis, both publications proved to be illogical even when viewed in isolation, and moreover, each of them contains several very significant theoretical and methodological contradictions that cannot be dismissed with any objection, which make the idea of a Central Anatolian (Renfrew) or Eastern Anatolian — Transcaucasian — Northern Mesopotamian (Gamkrelidse and Iwanow) homeland of the Indo-Germans not only very unlikely, but also definitively ruled out. — Added to this is the almost unbelievable lack of knowledge of the authors of the second article, Gamkrelidse and Iwanow, which is repeatedly demonstrated in the historical geography of Near East, its archaeological culture, and most importantly its ancient linguistic relations and the history of its language. These shortcomings, taken individually, would be sufficient to undermine the authors' competence in the field of

953
===== PAGE 942 =====

We strongly doubt, at least in the field of ancient linguistic and linguistic history issues discussed in their article; however, considering these hair-raising deficiencies in qualifications listed above as a whole, we can only conclude that the Soviet author pair is definitively and demonstrably completely unqualified to deal with the ancient linguistic relations of the East Asian region in any scientific way.

The Hungarian echo of Gamkrelidse and Iwanow's assessment
After presenting our own opinion on Gamkrelidse and Iwanow's hypothesis following their article published in "Spektrum der Wissenschaft", it will not be uninteresting if we also get acquainted with Márton Istvánovits' review of the original two-volume book by the two Soviet researchers published in 1984, dealing with the same hypothesis of an Indo-Germanic homeland in eastern Anatolia and northern Mesopotamia, which Istvánovits presented in the Ethnographic Research Group of the Hungarian Academy of Sciences - that is, not just any "Ethnographia" published in the 1st issue of 1988, in the forum "New Theory on the Origin of Indo-European Languages and Peoples", and which is a slightly shortened version of the title.

After a brief overview of some of the better-known earlier ancestral homeland hypotheses of Indo-Germanic ancestral research, Istvánovits enthusiastically explains what an epoch-making discovery Gamkrelidse and Iwanow's new Transcaucasian1—Eastern Anatolia1—Northern Mesopotamian Indo-Germanic ancestral homeland hypothesis represents in his opinion. According to Istvánovits, the new theory was necessary because Hrozny had proven that the cuneiform records created in the Hittite kingdom, which existed in the 18th-13th centuries BC in the territory of present-day Turkey, "... are the legacies of an Indo-European-speaking ethnic group." Further research revealed that "... the archives of the former Hittite capital, Hattusas, also preserved cuneiform tablets that are relics of Indo-European languages related to Hittite, but still existing, namely the Luwian and Palaian languages. These languages were therefore widespread in Asia Minor at the turn of the 3rd and 2nd millennium BC," Istvánovits finally concludes with considerable carelessness and "reluctance." "This wording is so distorted, misleading, and even bordering on untruthfulness that it can be called a blurb that has no place in a scientific review. Let's take a closer look, why?

First: In sharp contrast to Istvánovits's opinion, Gamkrelidse and Iwanow's new Indo-Germanic homeland concept could hardly have been necessary - in 1984, when the book by the two Soviet authors was published - because Hrozny "proved" - as is a fairly well-known fact in linguistic circles, as early as 1916 - that the Hittite language is Indo-Germanic in nature.

954
===== PAGE 943 =====

(This is the correct description of the real nature of the Hittite language, about which the prevailing opinion in Indo-Germanic studies is that it represents only the linguistic state of an early stage of development towards Indo-Germanization, but it cannot yet be called simply an "Indo-European language" (1. e.g. pp. 410-411 and 551 of our book).
The Hittite language caused the greatest possible confusion in Indo-Germanic studies, because although it had the oldest linguistic monuments, at the same time, in its phonetic forms and grammar, it differed so greatly from the inferred phonetic structure and grammatical structure of the Proto-Indo-Germanic basic language vocabulary previously reconstructed using the "phonetic law" method, that it was precisely this circumstance that caused the separation of the previously unified concept of Indo-Germanic studies into two diametrically different trends, as we have already learned above in the quote from volume V of the Reallexikon des Assyriologie, page 91. Namely, the recognition of the aforementioned great differences of the Indo-Germanic character of the Hittite language from the previously inferred Indo-Germanic basic language brought about the Second: According to Istvánovits, the Luwian and Palaian languages were "independent Indo-European languages related to Hittite". Now, as we have emphatically pointed out in the previous sections that the Hittite language itself cannot simply be called an "Indo- "ahistorical" Indo-Germanic trend, which we can also read about in the quote just mentioned.
European language", we must now emphasize even more explicitly that so few linguistic remains are known from the Luwian and Palaian languages that, apart from the fact that certain features can be identified in these two languages, which are among the characteristics of the Indo-Germanic languages, they are not yet sufficient to definitively classify Palaian and Luwian into the Indo-Germanic language family.

Thirdly: At the end of the lines quoted above, Istvánovits states with noble simplicity that "... these languages were therefore widespread in Asia Minor in the 3rd-2nd centuries BC." "(emphasis added). After having previously written that the Hittite kingdom existed between the 18th and 13th centuries BC and that the cuneiform texts in question are known from the capital of this country, he now simultaneously claims that the languages of the texts found in the Hattusa archive were widespread in Asia Minor at the turn of the 3rd and 2nd millennia BC. It is obvious to everyone - and no special linguistic training is required for this - that although in a slightly broader interpretation it refers to the middle of the 2nd millennium, it cannot in any case be applied to the turn of the 3rd and "between the 18th and 13th centuries" may mean a time definition 2nd millennium, which term means the period between approximately 2100 and 1900. However, Istvánovits nevertheless chose the phrase "at the turn of the 3rd and 2nd millennia BC", quite obviously in order to thereby

955
===== PAGE 944 =====

could make the above-mentioned languages appear to be more than half a millennium older. It is worth noting that although the Hittite kingdom did indeed exist between the 18th and 13th centuries, the earliest texts in the Hattusa cuneiform archive only date from around 1400, the beginning of the so-called Hittite New Kingdom, because no written records have survived in Hattusa from the earlier, so-called Old Kingdom period.

Fourth: Contrary to Istvánovits's wording — which suggests to the reader that the Hattusa archive would only have contained Hittite, Luwian and Palaian texts — these languages were therefore widespread in Asia Minor be that no less than seven languages were found in the Hattusa archive, of in 1.6. 3-2. "at the turn of the 3rd millennium" (i.e. Hittite, Palaian and are found, since he concludes that , Luwian) — the real situation seems to which only the three listed by Istvánovits alone (Hittite — or rather the so-called "Nes" —, Luwian and Palaian) were languages that could be called Indo-Germanic to some extent (more precisely, more or less Indo-Germanic linguistic features can be found in these three languages, but all three — especially Luwian and Palaian — are still quite far from being called "Indo-European languages"), while three of the other four languages in the archive were inflected languages (the so-called Hattian or Proto-Hatti, which was widespread in Central Anatolia in the 3rd millennium BC, and Hurrian and Sumerian), and the fourth is Semitic Akkadian. In other languages just words, Istvánovits is quite obviously mystifying — consciously or not, let's not ask now, and the Palaian language was widespread in Asia Minor during the rise of the Hittite empire, but the three inflected mentioned, and also the Semitic Akkadian — which is a very, very important difference. Not to mention the fact that if we want to take Istvánovits at his word, we must also point out that in the 3rd-2nd millennium BC --, when he ignores the fact of paramount importance that — contrary to his wording — not only the Hittite, the Luwian

At the turn of the 20th century — during which period he speaks of the spread of the Hittite, Luwian and Palaian languages in Asia Minor — these three languages were still completely unknown there, since according to the generally accepted position of international archaeological research, the Hittites, and with them the Luwians and Palaians, only appeared in Asia Minor after 2000, and their previous places of residence unknown.

On the sidelines of the question of the "Indo-Germanic" nature of the Hittite, Luwian and Palatine languages, Fritz Schachermeyr's assessment in this regard is definitely worth mentioning. On page 262 of his fundamental work, Die minoische Kultur des alten Kreta, he writes about the Luwian language, for example: "In the Luwian language, which we know to some extent from the cuneiform texts found in Bogazköy (the modern Turkish name of Hattusa — our note), an Indo-European component is also evident, especially in the inflection and the pronouns.

==== PAGE 945 =====
ren, mitg the Anatolian substrate appears in the form of a very strong Aegean (Schachermeyr briefly calls the Anatolian-Aegean-Tyrrhenian language community this way, note from us) component. The latter included the majority of the 8/6- treasure and the endings (suffixes, note from us). Aegean were above all the ndr't " and "ss-", "this mixed character of the Luwian language, however, is simply ignored by some researchers. Moreover, they consider it a homogeneous language and what someone . . . mn" endings. The belonged to the Luwian language is also Indo-European in their eyes."

But let us examine Istvánovits' review further. Immediately after the lines quoted above, he writes: "during the existence of the empire, not . . . It was also revealed that in the Mitanni kingdom (at the source of the Euphrates) the Hittite later than the first half of the 1st. 6th. 2nd millennium BC — it was inhabited by a population speaking an Indo-European dialect." — That is, similarly to Gamkrelidze and Ivanov, Istvánovits has apparently not heard anything about the fact that the population of the Mitanni empire (which, by the way, was not limited to the source region of the Euphrates, as Istvánovits believes, but extended from the Zagros Mountains to the Syrian coast) did not speak some kind of "Indo-European dialect" but the inflected Hurrian language. As we have already mentioned above, contemporary written language monuments provide ample evidence that the Hurrian language was an inflected language and has nothing to do with the Indo-Germanic languages. It is downright shocking that such nonsense can be stated in front of a forum of the Hungarian Academy of Sciences without there being anyone to correct these absurdities.

Then Istvánovits rates the theory of the Soviet authors very highly, especially because of the "classical balance" of their method. He considers their work to be "of scientific historical significance." However, we ourselves are much more inclined to think that the "classical imbalances" that we have already encountered, to put it mildly, testify to something completely different than to work of "scientific historical significance."

But the thickest part is yet to come. Istvánovits praises the method of the two Soviet authors to the skies above all because — in the words of Gamkrelidze, We did not look for the specific location of the ancestral homeland of the Indo-Europeans on the map. We . . . were convinced that this ancestral homeland would appear before us by itself if "We are revealing how people lived, acted, and talked about their language, whose language ceased to exist six thousand years ago. That is why we have begun to reconstruct the basic language."

This quote clearly demonstrates the Soviet authors' "historical significance" and he obviously cannot praise them enough. It is . . . "classical balanced" method, which Istvánovits unmistakably clear from Gamkrelidze's words that the public

==== PAGE 946 =====
They evaluate the 19th century hypotheses of an Indo-Germanic ancestral people and language as given facts and it occurs to them that — precisely from a methodological point of view — perhaps they should also think about and take into account that time may have passed over these ideas formulated more than 150 years ago and that today they no longer correspond to the current archaeological, prehistoric and linguistic evidence.

We can also notice from the quote that Gamkrelidze and Iwanow's method is based on the principle of "linguistic paleontology" that has been in use for 30-40 years, i.e. on the assumption that the geographical location of the ancestral homeland of a certain presumed language family, the living conditions of the indigenous people, and their economic, social and cultural status can be practically fully known through the reconstruction of the basic language of that indigenous people's language family using the "phonetic" linguistic method. In this regard, we would like to remind you once again of the clearly negative criticisms, amply supported by original citations from the relevant literature, regarding the method of so-called "linguistic paleontology". Likewise, the theoretical impossibility of the previously generally assumed . . . "The Sun Rises in the East" was repeatedly presented in detail in previous volumes and was discussed by many renowned representatives of Indo-Germanic studies. reconstruction of the words of the common Indo-Germanic language family based on "phonetic laws" has been definitively proven by several internationally well-known Indo-Germanic linguists (see pages 94-101 and 407-428 of our book).

We apologize to our esteemed readers for having to bring up these things — which are already commonplaces for them — again and again, but unfortunately we cannot do anything else, because these new linguistic discoveries of eminent importance are clearly still not sufficiently known, otherwise it would not be possible to represent such profound views before the Hungarian Academy of Sciences without someone immediately curbing the speaker's wild imagination. However, since this did not happen during Istvánovits' lecture, it is obvious that not only have the two Soviet authors not yet become aware of all these methodological discoveries, but the developments in Indo-Germanic studies in this direction of recent decades also seem unknown to the "authorities" of the Hungarian Academy of Sciences.

This ignorance becomes particularly evident when Gamkrelidze, continuing the previous quote, writes the following: "How this happens" — (i.e., the reconstruction of the base language) —,.... I will present a simple example. By comparing the words of all known Romance languages, we can calculate how a word sounded in their common ancestor in Alaatin. But because we know the Latin language, we can also

958
==== PAGE 947 =====
"We have the opportunity to verify the procedure itself, the method of reconstruction. This is exactly how the reconstruction of the common ancestor of all known Indo-European languages takes place on the basis of their

Well, this is precisely the method that Georg Solta, for example, proved in 1952 to be based on a fundamentally erroneous premise (I. Solta, 1952). In this study, Solta explains for the first time that the most certain basis in all of Indo-Germanic studies was initially that the common ancestor of the Romance languages was Latin, or rather the so-called Vulgar Latin. This is true in general, but we have not said much yet. Latin has always developed differently and various factors have played a role in its differentiation. For example, the given linguistic substrate, the time of Roman colonization, the social stratification of the colonizers, the possible superstratum, etc. The reason why linguistic development did not stray too far from the starting point was primarily the restraining force of the Latin literary language, which, moreover, influenced the Romance languages in the Western territories as a Middle Latin ecclesiastical and scientific language until the modern era. Above all, Solta continues, the cultural state of the Roman Empire, with its uniquely centralized, tight military and administrative-legal foundation, cannot be compared in any way with the completely primitive cultural conditions that we have to reckon with among the "Indo-Germanic peoples". Furthermore, we must also take into account that modern Romance studies no longer acknowledges the former existence of a unified vulgar Latin language. It has been shown that Latin was divided into dialects everywhere and only the influence of schools and the administration creates the appearance of unity in written records. Solta then summarizes the conclusions that can be drawn from the foregoing as follows: "If we had only the Romance languages before us, but knew nothing about Latin, we would never be able to correctly conclude Latin based on the comparison of the individual Romance languages. In this situation, however, Indo-German studies are insofar as they want to reconstruct the "Indo-Germanic". Wenn aber diese Rekonstruktion der Ursprache prinzipiell unmöglich ist (das gilt auch für die Annahme, dass die 108. Sprachen in dem Sinne eine idg. Komponenten enthalten, wie die romanische Sprachen eine lateinische Komponente), so würd man sich fragen, was dann der Gegenstand der vergleichenden Sprachwissenschaft sein soll und man kann nur antworten: die Ernährung zwischen den einzelnen Sprachen... (Solta, 1952, in Scherer, A.: Die Urheimat der Indogermanen 1968, p. 326 — emphasis by Solta!.

In Hungarian: "If we only had the Romance languages before us, but knew nothing about Latin, then we would never be able to correctly infer Latin based on a comparison of the individual Romance languages. Eb-959

==== PAGE 948 =====
However, Indo-Germanic studies is in a similar situation, insofar as it wants to reconstruct the "6s-Indo-Germanic". However, if the reconstruction of the Common Language is impossible in principle (this is also valid if we assume that the Indo-Germanic languages also contain an Indo-Germanic component in the same sense as the Romanian languages also have a Latin component), then we must ask the question of what should be the subject of comparative linguistics. We can only answer this: the correspondences between the individual languages. . ."

[In this quote, therefore, the applicability of exactly the same method — namely, the inferences to the Proto-Indo-Germanic base language made on the model of the attempted reconstruction of Latin from the Romance languages — is refuted by Solta in a point-by-point, logical and completely plausible manner, and Istvánovits considers its application by Gamkrelidze and Iwanov in reconstructing the Proto-Indo-Germanic base language to be of "scientific-historical significance." — We could not find a more striking example to prove the - stated - fact that the current academically recognized and thus practically, officially recognized" exclusively Finno-Ugric trend of Hungarian linguistics has irreparably broken away from the standard of international linguistics, consistently ignores the foreign linguistic insights of . . . "The Sun Rises in the East" has been repeatedly mentioned in his previously published volumes recent decades and is the only one to use the "Realexikon der Assyriologie" for Indo-Germanic studies. al- tal in language comparison and language development studies. As we have already shown above, it tries to construct a global prehistoric and linguistic worldview exclusively with the help of these alleged "laws", and also strictly on the basis of "sound laws" it has already gone so far as to declare several proven non-Indo-Germanic languages as Indo-Germanic, and even to turn the Japanese Ainu language into Indo-Germanic (1. Realexikon der Assyriologie, V. Band, p. 95). Similar — also irrefutably outdated — methods . . . The methods of his research school, called the "ahistorical" school, are considered authoritative in of language comparison are praised by egckig Istvánovits from the work of the two Soviet authors when he presents the theoretical foundations "ahistorical" school proclaims the absolute validity of "sound laws", of Gamkrelidze's and Iwanow's reconstruction of the Indo-Germanic basic language with the help of concrete examples. "A word can be considered to belong to the Indo-European root language even if it is present in the most archaic dialects: e.g. Indo-European yuga "yoke", — Hittite yuga , the same" — he writes. Istvánovits apparently chose the word yuga = "yoke" as an example because he was convinced that this word was particularly suitable for illustrating the Gamkrelidze and Iwanow's method of "historical scientific" reconstruction of the root language. Let us not ask now what Istvánovits (or

960
==== PAGE 949 =====

perhaps the Soviet authors -- this cannot be decided on the basis of the text) with the designation "Oindocurpa". This expression is rather ambiguous; on the one hand, it could possibly mean the Indogerman basic language, but in that case, the word "basic language" or even more "Osindoeurpa" should have been used, but it is also conceivable that Istvánovits wants to convey a linguistic stage between "Osindocurpa" and Hittite -- in this context, however, it should also have been explained as something intermediate, namely, what specific later linguistic state the designation "Osindoeurpa" is intended to denote. This, in fact, is unimportant, only affecting the accuracy of the formulation

Instead of analyzing this question, let us look at what G. Solta writes in his study cited above, specifically regarding the word yuga = "yoga". 50118 first states that Paul Kretschmer showed in 1896, in his work "Introduction to the History of the Greek Language", that the only difference between the so-called "primitive" words and the borrowed words is that we know the direction of borrowing in the case of borrowed words, but not in the case of words. But on the other hand, Kretschmer, ref. m. p. 49): "Between 'primitive' words and 'borrowed words' there is therefore an individual language - not a principled one, but a temporal one" - writes Kretschmer (1. Kretschmer, ref. m. p. 23). In Hungarian: , the same difference as "ancestral" between the so-called proto-language and the individual "Cognate" words also migrated from person to person through borrowing and only in this way did they become common Indo-Germanic words. (I. separate languages — that is, there is no difference in The difference between "cognates" and "loanwords" is therefore the same as between the so-called proto-language and the yoke, which is attested in all Indo-Germanic languages and which prove to be an individual language. This is the case with the words "yoke" and "yoga". Solta's arguments in this direction are as follows: "But if between a word like , and Slavic languages, which itself has penetrated into the Old Indo-Germanic, Greek. Mediation (there is no difference in principle), then the whole concept of an original language has become obsolete. The common Indo-Germanic words, considered as loanwords, prove that they are no longer old cultural relations between the Indo-Germanic peoples (1. Solta, 1952, in: Scherer, A. 68. pp. 331-332. — emphasis by Solta). In Hungarian: "However, if a word like "yoke" (Joch), which occurs in all Indo-Germanic languages and which is generally interpreted as a word of the Proto-Indo-Germanic culture, and a word like the Latin word piper has entered all Germanic, Celtic and Slavic languages, then the Proto-Indo-Germanic language, for its part,

"pepper" (Pfeffer) (which is the

961
===== PAGE 950 =====

"(It can be traced back to pipelli, through Greek mediation) there is in principle no difference, but then the whole concept of the proto-language is overturned. The common ancient Indo-Germanic words, interpreted as loanwords, prove no more than that there were old cultural contacts between the Indo-Germanic languages." In other words, through the example of the word yuga (yoke), carefully selected by Gankrelidse and Iwanow (or perhaps by the reviewer, Istvánovits - as we noted above, the wording did not clearly indicate whose opinion is being discussed in this case), and which was obviously intended as very striking evidence, Solta shows what enormous errors the previously generally applied method of proto-linguistic reconstruction in Indo-Germanic studies can lead to, according to which every word that occurs in some older Indo-Germanic linguistic monuments was considered to belong to the vocabulary of the proto-Indo-Germanic linguistic community and - incidentally, it should be noted - the words thus inferred from the assumed proto-linguistic existence were deduced, using the method of "linguistic paleontology", to the geographical location of the Indo-Germanic ancestral homeland, as well as the economic, social and cultural characteristics of the presumed Indo-Germanic ancestral people. their conclusions regarding their relationships.

Then, in the context of the monograph of the two Soviet authors, Istvánovits moves on to praise the work of Iwanow. According to Iwanow: "It has become possible to reconstruct not only isolated words from the Indo-European basic language, but also entire text passages." — This statement once again highlights more clearly than the sun the lamentable obsolescence of the linguistic working method of the Soviet authors. In this issue, international Indo-Germanic research has also been representing a diametrically opposed position for more than 30 years. It will be sufficient to quote the relevant lines of Ernst Pulgram's study "Indoeuropäisch und Indoeuropäer", published in 1958, to show the astonishing lack of method in Iwanow's above statement. Pulgram's following explanations can be found on pages 420-421 of our book. page, but in view of the fundamental, theoretical and methodological importance of the question, we quote it again - and this time in more detail. Pulgram first reconstructed with the "sound-law" method, referring to the basic Indo-Germanic words "stellar"? He points out: "Niemand behauptet oder könnte behaupten, dass die urindoeuropäischen Formen jemals existierten oder so ausgesprochen wurden, wie wir sie schreiben." (Viele unserer Formeln lassen sich in der Tat nicht aussprechen, und dagegen ist nichts einzuwenden.) Dieses besondere Urindoeuropäisch soll ja auch factact keine Sprache darstellen; es ist nur ein Distillat der essential Kennzeichen nach Laut und Form."

(L. Pulgram, 1958; in Scherer: Die Urheimat der Indogermanen, 1968, 461. old.).
962
===== PAGE 951 =====

In Hungarian: "No one claims or can claim that Proto-Indo-European word forms ever existed, or that they were pronounced as we describe them. (Many of our word forms are indeed unpronounceable, and that is no problem.) This special Proto-Indo-European does not claim to be any specific language, but merely a distillation of the essential features in terms of sound and form." Pulgram then also deals in depth with the classic criticism of P. Kretschmer, already cited above, that we have no methodological means at our disposal to distinguish possible genuine Proto-Indo-Germanic words from very old loanwords. Literally he writes: "Wenn nun eine Sternchen-form auf nur drei oder fünf belegten Formen aus den vielen Indo-europäischen Sprachen beruht, so haben wir nicht die geringste Garantie dafür, dass es eine es einträchtis appropriate oder ursprüngliche Form in der Sprache gibt, die wir Urindoeuropäisch nenne. Sie kann sehr früh aus einer anderen Sprachfamilie zu nur drei oder fünf Indo-europäischen Sprachen gekommen sein, aber vielmehr ist sie in allen diesen Sprachen so good Indoeuropäisiert worden, dass sie wirklich ererbt zu sein scheint." (L.

Pulgram, 1958; in Scherer 1968, p. 462) In Hungarian: "Now, if a star-shaped word (the word marked with a star is not 2031012816, but only the basic word forms of a language family linguistically reconstructed by the "phonetic law" method; our rule) is based on the given word forms of only three or five languages out of the many Indo-European languages, then we cannot have the slightest guarantee that a truly corresponding or original word form exists in the language we call Proto-Indo-European. The word form in question could have entered the three or five Indo-European languages very early from another language family, but perhaps it was so perfectly Indo-Europeanized in all these languages that it really seems like an inherited word." Moreover, Pulgram continues his explanations: "Auch wenn die Parallelformen eines Wortes in allen indoeuropäischen Sprachen belegt sind, so ist die Möglichkeit immer noch offen, dass es nicht aus dem Ur-indoeuropäischen stammt, sondern nur ein Lehnwort ist, das sich aus irgendeinem Grunde über den ganzen indoeuropäischen Bereich breitet hat, Ebenso kann ein gemeinromanisches Wort, das überall in der Romania erscheint, nicht-lateinischen Ursprungs sein, und jede ursprüngliche "tateinische" Form, die man nach solchen romanischen Parallelformen würde würde rekonstruieren, wäre falsch. Das bedeutet, dass eine beliebige Anzahl von Sternchenformeln nicht nur nicht unbedingt wirkliche urindoeuropäische Formen darstellen, sondern auch dass, selbst wenn dies der Fall ist, die Rekonstruktionen nicht das gleiche Alter haben. Daher muss der Versuch, etwas auf Urindoeuropäisch zu schreiben — und er ist factact gemacht worden -, zu einem Durcheinander von Sprachformen führen, die sich im Grad ihrer Echtheit und ihrer Entstehungszeit

963
===== PAGE 952 ===== beträchtlich voneinander unterschieden. Ähnlich sähe das Werk eines Linguisten der Zukunft aus, der ohne die Hilfe überlieferter Aufzeichnungen einen Satz zusammenbrauen würde, bestehen aus, nehmen wir an, Mittellatein, Altfranzösisch und modernem Italienisch, samt einer Anzahl von Formen, die niemals wirklich ausgesprochen wurden, um sein Erzeugnis dann "Uromanisch" zu nennen. (1. Pulgram, 1958; in Scherer 1968, pp. 462-463) In Hungarian: "But even if the parallel forms of a word can be detected in every Indo-European language, in this case the possibility is still open that it does not come from the Proto-Indo-European language, but is only a loanword that for some reason spread throughout the entire Indo-European region. It is also possible that a common Romanian word that occurs everywhere in the Romance languages is of non-Latin origin, and that everything would be original. This means that not only do a certain proportion of the asterisked word forms not necessarily denote real Proto-Indo-European word forms, but also that, even if this former circumstance were true, the reconstructed words are not of the same age. For this "Latin" word form, which we would reconstruct based on such Romanian language parallels, is erroneous reason, attempts to reconstruct a text in a Proto-Indo-European language describe—and indeed they have attempted to do so—they differ substantially from one another in the degree of their real value and in the time of their origin. The work of a future linguist who, without the written records handed down to us, would cobble together a sentence, say from Middle Latin, Old French, and Modern Italian, to which he would add a certain number of forms that were never actually used in speech, and who would then call this creation "Oösromän -, necessarily lead to such confusions of linguistic forms that language."

Finally, Pulgram summarizes the results of his research in this direction: "It is a fundamental mistake to equate common Indo-European words with primitive Indo-European words and to build conclusions about the ancestral people or the ancestral homeland of the primitive Indo-Europeans on it. But that is exactly what has been done, if it seems to be the preferred research method." (L. Pulgram 1958; in Scherer 1968, p. 463) In Hungarian: "We make a fundamental mistake when we regard common Indo-European words as Eastern Indo-European words and draw conclusions about the ancestral people or the ancestral homeland of the primitive Indo-Europeans from them. However, this is precisely what is often done, and it even seems to be the most preferred research method."

From the above, it follows indisputably that Pulgram, on a theoretical-methodological level, with the help of an in-depth analysis of concrete, datable and provable linguistic facts, irrefutably demonstrates that even in the pre-964
===== PAGE 953 =====
The attempted reconstruction of the presumed 6Sindo-Germanic language family based on "phonetic laws" is also theoretically impossible, not to mention the fact that - as Iwanow believes - it would even be possible to reconstruct, or rather, to use Pulgram's expression, to cobble together, Proto-Indo-Germanic language texts. Istvánovits then quotes the following from Iwanowet: "The well-known linguist-philologist R. Jacobson has proven that the rhythm of certain genres of Slavic folklore poetry (bilinas, laments) is a direct descendant of common Indo-European metrics; those that have been reconstructed on the basis of a comparison of the hymns of the ancient Indian Rigveda and the oldest forms of ancient Greek (from which the Homeric hexameter originates)."

However, these ideas must also include the fact that, as regards the characteristic of ancient Greek metrical poetry, the hexameter, and its parallels, e.g. in the hymns of the Rigveda, these rhythmic forms of poetry, based on the knowledge of the pre-Asian cuneiform monuments currently available to us, have not been some kind of "Early Indo-European" characteristic at all, but have ultimately been of Sumerian origin. This fact was proven very nicely and convincingly by Géza Komoróczy in his extremely valuable study entitled "The Theory of Sumerian Poetry" (see Komoróczy: A Sumer irodalmi gyálmógy, 1979, pp. 637-676). Komoróczy first emphasizes that in the examination of the issues of Sumerian-Akkadian poetry, metrics, and emphasis, all previous attempts at explanation have been based, without exception, on the Indo-European languages.

They did not start from the meter of the poem, nor did they try to read one of the systems into the Sumerian and Akkadian texts, "... without having previously examined the applicability of the method." (I. Komoróczy 1979, p. 640) "So naturally the explanations were not successful either," adds Komoróczy.

For us, such "methods" are no longer new in the ranks of Indo-Germanic studies. Until recently, Indo-Germanic research considered . We have seen countless times in the volumes of "The Sun Rises in the East" that the Indo-Germanic languages, the civilizations carried by the Indo-Germanic peoples, and first and foremost the so-called "Hellasian miracle" (i.e., ancient Greek culture) as the absolute measure of all things, and other peoples and civilizations appeared only as reflections of these in its prehistoric and cultural-historical approach and hypotheses.

"miracle" pale

In his aforementioned study, Komoróczy further demonstrates point by point that Sumerian poetry was based on rhythms.

The existence of metrics is first and foremost proven by the frequent reference in the closing formula of incantations to the need to "count" the text. This was denoted by the Sumerian word Sid (Akkadian manu).

965

===== PAGE 954 =====

We would like to note here that the Sumerian word Sid, which occurs in both the phonetic forms sit and Sita (I. Borger 1978, p. 129) and which means "to count, , sieve, sieve Friday..." begins with the ancient Hungarian

"to count, to read rhythmically", can very easily be related to the destructive reading verse szit" and even the word "rezitieren rytkesen" i.e.

counted — Sid, sit Another -, demons in a figurative sense through "scolding"-6 spells, perhaps also with our words "scold", "scold".

proof of metricalism — Komoróczy continues — is that on the more sophisticated tablets the poems were consistently divided into lines. The lines of verse usually correspond to a single sentence. If the sentence is longer, an independent, closed part is still included in the line of verse, and the second line — and possibly the third — was written somewhat further in from the edge of the line. Within the lines of verse, space was left between the parts of the sentence that were connected in some sense, that is, the syntagmas were highlighted. Since we do not encounter such a thing in prose, but in verse it is a fairly common phenomenon, we can rightly assume that the omissions are signs of some kind of metrical division.

Komoróczy then tells how, while translating Sumerian poems into Hungarian, he realized that the essence of Sumerian poetry is repetition, and that these repetitions form a chain: a new member is added to the repeated member, and then during further repetitions the old member is always omitted. The poem progresses through the new and omitted members of the repetitions." (I. Komoróczy 1979, p. 648)

Then — after having methodically proven over twenty pages with abundant quotations that the essence of Sumerian poetry is indisputably a theme that progresses gradually through repetitions and omissions — he also mentions that this peculiarity "... is not unknown to the Hungarian reader." He encountered it in our older folk songs, he encountered it in the poetry of Endre Ady -- and he encountered it in the ancient Hebrew literature well known to every European reader: the Bible" (I. Komoróczy 1979, p. 668). Moreover, we learn much more from Komoróczy than this. He tells us that in addition to the phenomenon of "parallelismus membrorum" (parallelism of versts) found in the Bible, this repetitive form of verse of Sumerian origin had a very strong influence on the poetry of almost the entire Western Eurasia: it occurs in the ancient Indus Vedas, in the Old Turkic, Mongolian poetry, and even among the Finno-Karelians (Kalevala) and the Ob-Ugric peoples (cf. Komoróczy 1979, p. 669, with note no. 69, on page 689).

This characteristic feature of Sumerian poetry, that is, the real nature of the theme gradually advancing through repetitions and omissions, is most simply explained by János Arany 966

We can convey this to our readers by quoting the opening verses of his ballad "Bor vitéz":

===== PAGE 955 =====

The sun's rays are lost in the mist,
A blind darkness sits in the Bérczen valley. A brave man receives wine for his good horse:
"God bless you, my sweet lady!"

A blind darkness sits in the Bérczen valley, A cold wind rustles the bare trees. "God be with you, my sweet lady!" The brave Bor is already galloping far away.
A cold wind rustled the bare trees, A little lark was walking on the road. The brave Bor was already galloping far away, The lark cried out her beautiful eyes.
A little lark is on the way:

Where are you going? Where are you going? The beautiful girl cried her eyes out: Her father said: I will marry you!
and so on, through fourteen verses.

It is already clear from what has been said so far that almost the entire metrical poetry of Western Eurasia can ultimately be traced back to the ancient Sumerian rhythmic verse method introduced by Komoróczy. This should not be a surprise to us, since it can be proven archaeologically that almost the entire Western Eurasia was repeatedly colonized by pre-Asian — Mesopotamian agricultural settlers in the millennia BC, and these pre-Asian colonists introduced agriculture, animal husbandry, metallurgy and other elements of higher culture throughout the region. It is therefore not surprising that the Sumerian rhythmic verse method also reached these settlers from the ancient Indies through the Turks and Mongols to the Finns and Obi-Ugric peoples.

However, Komoróczy goes on to reveal even more detailed connections through meticulous, detailed investigations. He points out that the effort to write poems of the Sumerian type can also be clearly observed in Akkadian literature. However, this could not be fully achieved due to the different structure of the Semitic Akkadian language, namely that the continuous Sumerian sentence part carrying jyyyyj was fragmented into words in Akkadian, and the divisive function was taken over by the word stress. The Sumerian poetic form then

it was generally widespread throughout the ancient Near East (I. Komoróczy 1979, pp. 673-675).

967

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Finally, Komoróczy also addresses the much-debated question of the origin of the ancient Greek hexameter. He emphasizes that no less a figure than Aristotle himself called the hexameter a poetic meter alien to the spirit of the Greek language. In 1958, S. Segert raised the idea that the hexameter may have been of Ugaritic origin. "It is needless to say that it did not find a response among Greek poetry theorists," Komoróczy notes with a palpable mockery, and then continues, "..... The idea, "...But the idea is very attractive. We would put it this way: the Greek hexameter line is nothing more than an imitation of the Ugaritic epic verse. The rhythmic effect of the characteristic articulation-stressing Ugaritic verse could only be achieved in Greek by other means: the role of the Ugaritic word stress was taken over by the Greek syllable length, the rhythmic formula became more bounded and rigid than it originally was, the boundary of the rhythmic units and the word boundary no longer coincided." (I. Komoróczy 1979, pp. 675-676.)

Accordingly, the spread of metered verse in Western Eurasia can be roughly described as follows: the clear semantic structure of the original Sumerian metrical verses, in which the rhythms always formed an organic whole with the text, closely aligned with the smaller sentence parts (syntagms) that formed independent units in terms of meaning, gradually became more and more fragmented during multiple adoptions. The Semitic-speaking Akkadians, Canaanites (Ugarit), and Hebrews were only able to bring the rhythm into harmony with the specific features of their languages by breaking down the emphatic structure of the syntagms into individual words, while the Greeks (and later the Romans) — again due to the compelling effect of their different language structure — were forced to break up even the words in order to create emphatic rhythms: in Greek (and Latin) hexameter, rhythm is independent of word boundaries.

This, in turn, means that in the "classical" Greco-Latin metrical poetry, the beat as a "spiritus rector" completely subsumes the meaning of the sentence, and thus not only the connected parts of the sentence, but also the individual words, are torn apart in accordance with the requirements of the rhythm, the scandalized sentence becomes a jumble, sometimes almost meaningless. This represents a much more primitive stage of development, both philologically and from a poetic and aesthetic point of view, than the meaningful, syntagmatic rhythmic division of Sumerian poetry more than two millennia earlier, and clearly proves that Aristotle was right when, knowing nothing about the Sumerian and Akkadian-Ugaritic antecedents, he nevertheless recognized purely by intuition that the hexameter is a verse form alien to the spirit of the Greek language, and therefore cannot be of Greek origin.

In the knowledge of all these connections of closer and more distant pre-Asian origin, there can be no doubt that R. Jacobson recognized

===== PAGE 957 =====

nor that there are connections between the rhythms of certain Slavic poetic genres and hymns and the metrics of the oldest forms of ancient Greek poetry, can hardly be explained in the way that Iwanow does. These parallels are, in fact, quite obviously not the direct descendants of some nebulous common Indo-European metrics, as Iwanow believes, in the world of pre-Mesopotamian written records, but rather, they are probably not the direct descendants of some nebulous common Indo-European metrics, as Iwanow believes, probably through Semitic transmission, both to the Rigveda, to the ancient Greeks, and to Slavic —, but they undoubtedly came from ancient Sumerian poetry—all folklore. In his review, Istvánovits then presents the Indo-European basic language dictionary, linguistically reconstructed from Gamkrelidse and Iwanow's "starry" words and texts, in which "... three basic

The authors discuss a conceptual field: the natural environment, material and intellectual culture, and social organization of Indo-European society," he writes. In other words, the two Soviet authors are pursuing "linguistic paleontology" from yava, in its usual form 40-50 years ago. So Gamkrelidse and Iwanow are practicing linguistic paleontology, which was indeed a generally applied method in Indo-Germanic studies in the 19th century and the first half of the 20th century, but which, however, proved to be a fundamentally flawed endeavor for theoretical and methodological reasons after 1945. As we have already seen from the Pulgram quote above: "We make a fundamental mistake if we consider common Indo-European words as Old Indo-European words and draw conclusions from them about the ancient people or the homeland of the Old Indo-Europeans." After these lines, Pulgram continues his discussion of "linguistic paleontology" as follows: "Since, for example, a number of Indo-European languages contain related words for "birch" and "beech", the Urindo-European, it was said, must have been at home in a region where birches and beeches thrived.

If one plots the boundary of the area within which these tree species occur on a map, one obtains, as we are told, the boundaries of Proto-Indo-European. This conclusion may be linguistically correct, but it need not be, for, as we have seen, the asterisked reconstructions of the words for birch and beech, like all the others, may not even belong to the particular Proto-Indo-European language at a given time for which we are seeking a homeland. But even if one could at least overlook this objection, because the terms in question are indeed Proto-Indo-European—albeit undatable—words, two compelling reasons remain for abandoning this method of linguistic paleontology, as it has been called. First, the existence of related or even identical words in different languages and regions does not prove that they everywhere refer to Proto-Indo-European.

969

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The same things were related. For example, the robin in England is by no means the same bird as the "robin" in America. English immigrants, unable to find this bird, to which they felt an emotional attachment, in the New World, transferred its name to a bird that looked somewhat similar to the robin because it happened to have a red breast. Thus, the name passed from a small songbird to a thrush species almost twice its size. Future linguistic paleontologists searching for the origin of Proto-English might arrive at astonishing results if they equate the occurrence of the word and the thing "robin." Even if we accept as a given that Proto-Indo-European contained the precursors of the words for birch and beech, we still don't know whether these referred to the trees we now call by those names.

Secondly, even if the first objection could somehow be refuted (which I cannot imagine due to the lack of linguistic evidence), it would still be impossible to agree on the extent of Proto-Indo-European because we do not yet know how widespread the test subject was during the Proto-Indo-European period compared to the present day—especially since we cannot even date such a period. Indeed, what we know of the changes and cycles of climate outlined in an earlier chapter must undoubtedly lead us to believe that the distribution of all components of the prehistoric flora several millennia ago was not the same as it is today.

But the passionate proponents of linguistic paleontology went even further. From the existence of certain vocabulary words in all or the majority of existing Indo-European languages, they inferred—without the slightest regard for the pitfalls just pointed out—even the social organization, religion, customs, and race of the Proto-Indo-Europeans, whom they now called "Indo-Europeans." This name can only have meaning in the sense of "Indo-European speakers"; but since it was mostly used, explicitly or implicitly, to designate an ethnic, cultural, or racial group, it was bound to remain meaningless in reality, although it served its purpose in the pseudoscientific endeavors of idealists, propagandists, and enthusiasts. (L. Pulgram 1958, in Scherer, A.; 1968, 463–465. old.)

In Hungarian; "Since, for example, a whole series of Indo-European languages contain related words for "birch" and "beech tree", therefore it was claimed that the ancient Indo-European language must have been native to an environment where birch and beech trees grew. So if that area 970
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we draw the boundary of ŷy4 within which these tree species occur on the map, we get, as they say, « the boundaries of the Proto-Indo-European language. This conclusion may be linguistically correct, but it does not necessarily have to be correct, because - as we have seen > the star-marked constructions for naming birch and beech + as well as all the other reconstructed words - perhaps do not even belong to the specific Proto-Indo-European linguistic stage bound to a given time, which we are looking for the homeland of. But even if we could ignore this objection, because the words in question were indeed Proto-Indo-European - although of course undatable - words, even in this case there would still remain two valid reasons for which we should abandon this method called linguistic paleontology.

First, the existence of related or even identical words in different languages and regions does not prove that they refer to the same thing everywhere. For example, a robin in England is not at all the same bird as a robin in America. English immigrants, who did not find this bird in the New World, to which they had special emotional ties, transferred its name to another bird that somewhat resembled the robin because it happened to have a red breast. Thus the name of a small songbird was transferred to a species of thrush almost twice its size. Future linguistic paleontologists searching for the homeland of the Old English language would come to astonishing results if they equated the word "robin" with the occurrence of the thing. So even if we were to take it as a given that the words "birch" and "beech" existed in the Old Indo-European language, we would still know nothing about whether these words referred to the birch trees we call them today.

Secondly, even if the first objection could somehow be overcome (which I cannot imagine, for lack of linguistic evidence), it would still be impossible to arrive at a common denominator regarding the distribution of the Proto-Indo-European language, because we still know nothing about the extent to which the tree species in question were widespread in the Proto-Indo-European period compared with the present day—especially since we cannot even date such a period (emphasis added). However, what we know about climate changes and climate cycles, outlined in a previous chapter, undoubtedly leads us to the conviction that the distribution of all elements of the prehistoric flora was not the same a few millennia ago as it is today.

The passionate defenders of linguistic paleontology, however, went even further. They argued that the existence of certain words in all or most of the Indo-European languages that exist today—the example just presented—was a result of the fact that

===== PAGE 960 =====
without giving them the slightest attention — conclusions were drawn, moreover, concerning the social organization, religion, moral relations and race of the East Indo-Europeans, who were simply called "Indo-Europeans". This name can only have meaning in the sense of "speakers of the Indo-European language", but since it was most often used, explicitly or implicitly, to designate groups of certain ethnic, cultural or anthropological types, it had to remain meaningless in reality, although it achieved its goal in the field of pseudo-scientific aspirations of idealists, propagandists and dreamers.

This would be the opinion of the internationally recognized, renowned Indo-Germanist Ernst Pulgram, thoroughly supported and proven from all sides, on the theoretical impossibility of attempting to reconstruct the ancient Indo-Germanic language using "tone laws", and on the unusability of the method of so-called "linguistic paleontology".
In short: the attempt to deduce the Indo-Germanic language family's basic language using the linguistic, "phonetic" method fails mainly due to the fact that we do not know of any scientifically based method that would allow us to select early loanwords and inherited words from the words that occur together in the Indo-Germanic languages. This linguistic situation also prevents us from being able to determine the ancestral homeland of the ancient Indo-Germanic people using the so-called "biogeographical method" (using the common names of certain plants and animals in the Indo-Germanic languages). This latter endeavor belongs to the field of so-called "linguistic paleontology", which, in addition to the above linguistic exclusionary reasons, is made absurd by two additional circumstances. First: the changes in the meaning of words over time, which in the vast majority of cases — especially in ancient times — we cannot trace due to the lack of data. Second: the multiple climate changes and climate fluctuations of the past millennia, which we still know only very incompletely, and during which the distribution areas of individual plant and animal species were repeatedly subjected to substantial changes. Last but not least, we also learn from Pulgram that Indo-Germanic research cannot even establish the time period in which the supposed Indo-Germanic proto-language would have existed.

Then Istvánovits enthusiastically lists all the words, expressions, and concepts that the Proto-Indo-European language already contained — that is, according to the linguistic reconstructions of Gamkrelidse and Iwanow. This list becomes especially absurd when the Soviet authors record the names of barley, wheat, flax, apple, dogwood, strawberry, 52016 as original Proto-Indo-European words, and according to them, for example, the word meaning wine was taken over by the Semitic languages from the Proto-Indo-European base language. Likewise, the Proto-Indo-972

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The enemy
According to Soviet authors, the names of most domestic animals, such as horses, donkeys, bulls, cows, sheep, goats, dogs, pigs, etc., are genuine additions to the European language. In other words, in Hungarian terms, it is a question of the fact that, in Gamkrelidze and Ivanov's view, the ancient Indo-Europeans carried out the so-called "Neolithic revolution" in Central Anatolia, Transcaucasia and Northern Mesopotamia, i.e. they were the inventors of agriculture and animal husbandry. However, all these lofty ideas are quite obviously - and most importantly, demonstrably - nothing more than mere figments of the imagination.
We have a multitude of archaeological finds and written sources from Eastern Anatolia, Transcaucasia, and Northern Mesopotamia, all of which attest that in these regions in the last centuries of the 4th millennium BC, ca. Since 3250 BC, that is, since the beginning of the so-called "early Transcaucasian civilization" (this culture is known in Soviet archaeology as the Kura-Araxes civilization), the same population has inhabited it, which then appears in history in the second half of the 3rd millennium BC with cuneiform monuments written in its own language. This population was the Hurrian people, who spoke an agglutinative language beyond all doubt, and the Hurrian language has nothing in common with the Indo-Germanic languages. The entire international linguistic and prehistoric research agrees on this issue without dissent.
Now we can also see why the two Soviet authors had to make the Hurrian language more or less Slavic at all costs. (Remember, they claimed that the Hurrian language was a branch of the ancient Indo-Iranian language.) They absolutely needed this mystification because otherwise the horse's foot would have hung out too long under their hypothesis of an Indo-Germanic homeland in Eastern Anatolia — Transcaucasia — Northern Mesopotamia: they obviously could not have placed the homeland of the Indo-Germans in an area where — as we have shown above — a population with a contiguous language had already inhabited it since the 4th millennium BC. Therefore — obviously in accordance with their "classically balanced" and "historically significant" method — they simply "Indo-Germanized" the inflected language of the Hurrians, exactly in the same way as their methodological role models, the representatives of the so-called "ahistorical" school of Indo-Germanic studies, "Indo-Germanized" the language of the Japanese Ainu, also strictly using the method of "tone-law" linguistics' reconstructions of the proto-language, with which Gamkrelidse and Iwanow also work.

In connection with the words listed above, which Soviet authors claim to be Indo-European, we recall pages 175-177 of our book, where we demonstrated multiple serial correspondences between the Sumerian language, the Semitic and Indo-Germanic languages, and the Hungarian language.

===== PAGE 962 =====
between the names of zidllat. These close proto-linguistic parallels spanning several language families prove that the házállat names of the Indo-Germanic languages are not original Indo-Germanic words, but loanwords originating from the spoken language of the Proto-Asiatic-Neolithic-Early Metal Age linguistic koine, which, however, reached not only the Indo-Germanic languages, but also the Semitic languages and Hungarian. However, with this, we can only support and prove the epoch-making recognition of Indo-Germanic studies after 1945, which was discussed in detail above, from a completely different, independent perspective, according to which even if the parallels of a word can be found in all known Indo-Germanic languages, the word in question may still be an early loanword. The same situation is of course also with the word bor, which is supposedly of Indo-Germanic origin; Contrary to Gamkrelidse and Iwanow's opinion, the word meaning wine is not an Indo-Germanic loanword in Semitic languages, but rather the other way around. While the Proto-Semitic word for wine mar appears in written records of the Akkadian language before 2000 BC, the earliest recorded Indo-Germanic word for wine is

Its occurrence dates back to the middle of the 1st millennium BC, i.e. a good 1500 years later. So who took the name of wine from whom? With this distribution of linguistic monuments, there can be practically no doubt that the word meaning wine was transferred from one of the Semitic languages to the Indo-Germanic languages as a loanword.

Of course, the word meaning cart or chariot cannot be left out of the Proto-Indo-European vocabulary reconstructed by Soviet authors, along with other terms referring to chariot parts (wheel, axle, yoke, draft animal, tool, cart pole). Since these words were inferred from the time of the Proto-Indo-Germanic language using the "sound law" method of reconstruction of the proto-Indo-Germanic language — which, as we saw above, is a completely unreliable procedure due to theoretical and methodological reasons — they directly say that the existence of these words in the proto-Indo-Germanic language provides decisive evidence for the localization of the Indo-Germanic homeland. And since, based on the excavations of O. Vjaparidze, A. Mnatsakyan and other Soviet archaeologists — continues Istvánovits — the Englishman S. Pigot came to the conclusion that the chariot was invented in a remarkably narrowly defined area in the 4th millennium BC, namely in the region between Transcaucasia and Lake Van and Lake Urmia, according to the two Soviet authors it is clearer than day that the Indo-Germanic homeland must have been there too. See, it is as simple as that. The solution to the entire Indo-Germanic homeland question, which has been bitterly debated for a hundred and fifty years: all that needs to be done is to presuppose the 19th-century idea of Indo-Germanic studies, according to which riding and harnessing horses to carts were invented by the "ancient Indo-Germans", and then to place the Indo-Germanic homeland where the results of archaeological research localize the invention of the cart.

974
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In other words, from the previous line of thought of the Soviet pair of authors, we can reconstruct point by point the more detailed circumstances of the origin of their Indo-Germanic ancestral homeland hypothesis. The very first step - as we have already indicated in the previous brief summary - was obviously based on the idea, which has been one of the most popular hypotheses in the last 100 years, that the domestication of the horse, harnessing it to a cart and the invention of riding were cultural achievements of the ancient Indo-Germans. Well, Gamkrelidze and Iwanow took this popular assumption as an a priori fact, and from then on they simply

It was "natural" that they "managed to reconstruct, in a practical way?" a whole vocabulary suitable for carting, referring

to the basic vocabulary of the ancient Indo-Germanic languages. — Of course, without even paying attention to the multi-level pitfalls presented above, even though any one of them is sufficient in itself to completely nullify the credibility of any such reconstruction. — In the end, however, the two Soviet authors were literally "forced to take action", because after the Proto-Indo-Germanic peoples and their reconstructed basic language vocabulary had already been inseparably linked with the alleged Proto-Indo-Germanic origin of carting, they had no choice but to simply place the ancestral homeland of the Proto-Indo-Germanic peoples in the place from which, according to the unequivocal evidence of archaeological research, the earliest cart finds are known — that is, in Transcaucasia, in the region between Lake Van and Lake Urmia.

However, since in this region only approx. Around 1800-1700 BC, the earliest Indo-Germanic languages, which were already partly beginning to be Indo-Germanized, but were by no means fully developed, appeared, namely Hittite (Nesi), Palaian and Luwian, which is why the two Soviet authors had to necessarily mention the inflected Hurrian language, which can be dated from the earliest written records in this area — from the second half of the 3rd millennium BC — and which could not be ignored in any way — the "Indo-Iranian" — which, on the other hand, made their nicely contrived and, in their opinion, carefully balanced hypothesis absurd. Istvánovits goes on to explain that one of the main pillars of the Soviet authors' evidentiary procedure is the fact that the making of the Indo-European chariot required a very hard mountain wood species, which could only be harvested with a bronze axe (this is stated by Sz.

Semyonov proved it by the method of experimental archaeology); and it was precisely the Indo-Europeans who possessed a developed bronze culture, bronze art, which is undoubtedly confirmed by the corresponding special linguistic material. In other words, according to the evidence of the line of thought just presented, the Soviet authors have so completely abandoned the idea when drawing the level of education of the ancient Indo-Europeans that, after having previously attributed the invention of agriculture, animal husbandry, and carting to the ancient Indo-Europeans, they now want to vindicate the glory of the discovery of bronze art to the ancient Indo-Europeans.

975
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In connection with all these emerging hypotheses, however, it is absolutely necessary to emphatically point out the fact that such infantile ideas have disappeared from serious Indo-Germanic research - undertaken with the demands of science - for more than 100 years. In our volume "Towards the Proto-Asian Proto-Language" we showed that O.

Schrader, the world-famous Indo-Germanist linguist, had already in 1883 categorically refuted and roundly rejected previous views according to which the ancient Indo-Germans had a developed culture and agriculture. (1. Schrader: Sprachvergleichung und Urgeschichte, 1883.) Schrader also showed in the same work that -- also in sharp contrast to previous ideas -- the ancient Indo-Germans were practically unfamiliar with metals. They could only roughly work copper -- and even then only with a hammer -- but no trace of metal casting can be found among them, Schrader states. (Our 1st book, p. 57) The dubious glory that these, one might say, "childhood diseases", which had been discarded from serious scientific Indo-Germanic studies for more than a hundred years, were revived at the end of the 20th century undoubtedly belongs to the two Soviet authors.

On the sidelines of these absurdities, we would just like to note that this Gamkrelid and Iwanow-like ancient Indo-European population must have been a strange group in the southern foothills of the Caucasus and in Northern Mesopotamia. If agriculture, animal husbandry and the settled lifestyle, i.e. the entire "Neolithic revolution" had started with them - as Gamkrelidze and Iwanow claim -, a process that took place in the area in question as early as the 9th-8th millennium BC, and they had also invented bronze art, the earliest data on which (including real tin bronze) come from around 3200 BC, then all this would mean that the Gamkrelidze - Iwanow's Proto-Indo-European core population, in the view of the two authors, existed from the 9th millennium BC until the beginning of the 3rd millennium BC (cf. above: according to the authors, the unity of the Proto-Indo-European basic language would have broken down about 6000 years ago) in the same relatively small area - Transcaucasia, Eastern Anatolia, Northern Mesopotamia -- would have been inhabited, initially under Neolithic, then Chalcolithic, and finally Bronze Age cultural and economic conditions, without any significant population growth occurring during these 6,000 years and without at least some ethnic groups being forced to migrate. This is practically impossible, because the introduction of rudimentary Neolithic agriculture alone increases the number of the previous fishing-hunting population by about 100-150 times in a relatively short time. Despite this, Soviet authors only speak of the fact that the population growth forced the ancient Indo-Europeans to migrate from their ancestral homeland in the first half of the 13th millennium BC.

976
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I were forced, In stark contrast to this idea, it can be objectively proven with a mass of archaeological | finds that in the same period | In the 9th and 3rd millennium BC, the agricultural (and later the nomadic) population of the pre-4/5181 areas increased to such an enormous extent that starting around 6000 BC, the surplus population of these pre-Asian ethnic groups populated almost all of Europe and West Asia. These massive colonizations originating from East Asia, which occurred in waves and regularly repeated every 500-600 years, can be proven beyond doubt by the close connections and clear parallels of archaeological finds in the vast areas of Europe and West Asia, from the Atlantic Ocean to the Altai Mountains and India. It is therefore unimaginable, or even impossible, that from this enormous demographic explosion, which agricultural and animal husbandry way of life, and later it was a necessary consequence of metalworking and urban culture, only the Gamkrelidze-Iwanow Transcaucasian-Northern Mesopotamian-Eastern Anatolian Indo-Europeans would have remained, if they had really lived in the said region in the period in question, as the Soviet authors assume,

Moreover, the two Soviet authors later find themselves in even more blatant contradiction with the well-known archaeological and prehistoric facts. At least according to Istvánov's review.

Istvánovits literally writes: "According to the evidence of the vocabulary, Indo-European society had a developed urban culture, intensive trade and a significant material economy." On the other hand, in the areas where Gamkrelidze operated" and Iwanow place the Indo-European Oshazá, we know of the earliest urban settlements only from the second half of the 3rd millennium BC, when - according to the assumption of the Soviet authors - the ethnic and linguistic unity of the Indo-European people had already disintegrated. Therefore, even if we accept the argument of Gamkrelidze and Iwanow (of course only conditionally), and based on it, the still unified Indo-European society could not have had

"with a developed urban culture", which fact, however, only proves once again that "phonetic" reconstructions of the ancient language are not worth a penny, since the authors claim the former existence of a "developed urban culture" among their Transcaucasian ancient Indo-Germans on the basis that words that testify to "developed urban culture" were reconstructed for the time of the Indo-Germanic basic language, while in sharp contrast to this, during archaeological research in the area in question, no urban settlements from the time in question have been found so far. As if by the way, we should also point out that the previous Istvánovits 6, in which Istvánovits speaks of "Old Indo-European, Indo-European society", also clearly confirms Pulgram's observation quoted above that the "linguistic paleontologist" simply 977 in relation to the era

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The proponents of the method of the 19th century, even in the supposed Proto-Indo-European period, speak simply of "Indo-Europeans", and by these "Indo-Europeans" they mean certain ethnic, cultural or anthropological groups, although the term "Indo-European" has a concrete meaning only if we mean "speakers of an Indo-European language", and interpreting any other meaning into this term remains meaningless, although - as Pulgram notes - it has achieved its goal in the field of pseudo-scientific endeavors of idealists, propagandists and dreamers. (I. above.)

These last-mentioned fantasies (the appropriation of the introduction of agriculture, animal husbandry and the discovery of bronze art by the ancient Indo-Europeans, as well as the claim of "developed urban culture" in ancient Indo-European society) are so far unique in the research history of Indo-Germanic studies — which is certainly not poor in fantastic hypotheses — except for the earliest times in the middle of the 19th century, when — as we have already mentioned above — attempts were made to reconstruct the vocabulary of the basic population of the ancient Indo-Germanic language family using the methods of A. Pictet (see our book, p. 57). Incidentally, the aforementioned A. Pictet also searched for the ancestral homeland of Indo-Germanic with the help of "linguistic paleontology" and tried to reconstruct the economic, cultural and social relations of the ancient Indo-Germanics on this basis.

The secret of Gamkrelidze and Iwanow's method therefore apparently lies in the fact that it is a priori expedient, they reconstruct the vocabulary of the basic East Indo-European language in such a way that, in addition to agriculture and animal husbandry, it also reflects "developed urban culture" and bronze art. In this reconstruction, of course, they work only on the basis of the procedure generally applied in the so-called "ahistorical" school of Indo-Germanic studies, which takes into account only the "sound laws", that is, according to the method by which this ahistorical trend has already succeeded in "Indo-Germanizing" the language of the Ainu. We should not be surprised that these reconstructions, at least on a formal linguistic level, almost always "succeed", since we are already familiar with the statement of the "Reallexikon der Assyriologie" that the classification of the Hittite language among the Indo-Germanic languages ... led to almost arbitrary reconstruction variants..." (I. Reallexikon der Archeologie, V. Band, p. 91). Then this After completing the reconstruction operation, our authors draw the conclusion from the imaginary Proto-Indo-European basic language vocabulary thus obtained that the ancestral homeland of the Proto-Indo-Europeans could only have been in an area in which the multitude of achievements in the field of material and intellectual culture, the existence of which is proven by the basic language vocabulary reconstructed in the above way among the Proto-Indo-Europeans, according to them, was already found in ancient times. That is, in Hungarian,

978
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The cat bit its own tail: the basic language vocabulary was reconstructed with the ulterior motive of reflecting a whole range of cultural preferences, and then these reconstructed basic language words serve as evidence that the cultural achievements in question were indeed found among the ancient Indo-Europeans.

Nothing proves the correctness of our line of thought more eloquently than the argumentation with which the Soviet authors finally exclude the Balkan Peninsula, whose territory, in their opinion, could be seriously considered in the definition of the Indo-European homeland, along with the regions of Transcaucasia, Eastern Anatolia and Northern Mesopotamia, based on many other considerations, from the possibilities of the ancestral homeland they assume. Istvánovits writes in this regard: „ Summarizing what has been said so far, we can say that the conclusions drawn separately from each thematic group of the reviewed dictionary, although they coincide, nevertheless still target a fairly extensive geographical region in the localization of the ancestral homeland, which also includes the Balkan Peninsula. However, we must note that at the time in question, the whole range of achievements in the field of material culture, which were at the same time very important for the Indo-Europeans, were not yet known in the Balkans. Therefore, we must exclude the Balkans from the area of the Indo-European ancestral homeland to be recorded. Gamkrelidse-Iwanow therefore concludes: the Indo-European ancestral homeland could only have been in Western Asia (Asia Minor, Northern Mesopotamia).”

Bring on the scientific method. What else is this than the “sound law” word reconstruction, a typical example of a “circulus vitiosus”. In the first stage, it has been completely defined as illusory, even a theoretical impossibility, in which they naturally include a whole series of expressions claiming method” they infer a Proto-Indo-European basic language vocabulary that “advanced bronze art”, “advanced urban culture” and many other cultural achievements, so that then in the second stage, from these completely unmethodically deduced, therefore unrefuted, reconstructed phantom words, they automatically conclude that before the Indo-European homeland (the beginnings of which are dated by Soviet authors in their discussions to the end of the 4th millennium BC), advanced bronze art and advanced urban culture can already be dated, i.e. only in Western Asia”. However, it is particularly could only have been in an area where the breakdown of the Indo-European basic language occurred in the ancient times in question. characteristic of the prehistorical knowledge of the two Soviet authors that neither of them seems to be aware that, according to our current knowledge, at the end of the 4th millennium BC - when they believe the Indo-European basic language began to break down - 979

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they say — “developed urban culture” only in the Sumerian-inhabited areas of southern Mesopotamia 16162606, while in Transcaucasia, eastern Anatolia and northern Mesopotamia, where they claim the Indo-European homeland was located, we only have data on urban civilizations from the middle and end of the 3rd millennium. -So we can see again that in the case of Gamkrelidse and Iwanow's hypothesis, they use a typical “idem per idem” argument, using archaic language reconstructions whose actual validity cannot be verified, and It is a vicious circle. The completely unreliable, even proven methodologically flawed, to “prove” a highly developed and rather early Proto-Indo-European culture, and then, taking this alleged Proto-Indo-European civilization as a given fact, they declare that the Indo-European homeland could only have been there “where we can demonstrate the sufficiently high level of civilization required by the reconstructed imaginary Proto-Indo-European basic language vocabulary already in ancient times through archaeological research.

In this regard — as we have seen before — it is not a problem if prehistoric research knows of peoples with adverbial languages in the area thus designated in the period in question (in our case primarily Hurrians, but in the same area and in the immediate vicinity we also know of the Hattian, Subarian and Gutian peoples who also had an adverbial language from the 3rd and 2nd millennia BC). In such a case, it is enough to simply state, according to the method of the Soviet authors, “of scientific historical significance”, that the Hurrians spoke an ancient Indo-Iranian dialect, and in their opinion the question is settled. According to them, yes, but according to the current state of international research, no. — This is therefore the secret of the hypothesis “of scientific historical significance” apostrophized by Gamkrelidse and Iwanow Istvánovits.

After the previous lines of thought in the review clearly led to the conclusion that Gamkrelidse and Iwanow date the unity of the Indo-European core population to the 5th and 4th millennia BC, we can later read that the Soviet authors showed that the Semitic, Kartvel and Indo-European core languages were of the Sprachbund type, and thus the ethnic map of East Asia roughly follows the pattern of 1.6. “in the 3rd millennium” — writes after the 3rd millennium - otherwise the Indo-European Istvánovits. According to these, therefore, the Gamkre-(language alliance type) connection. “,... lidse and Iwanow place the disintegration of the Indo-European basic language basic language could not have formed a language alliance with the Semitic and Kartvel basic languages in the 3rd millennium. On the other hand, this opinion is impossible for the sole reason that the Semitic basic language — if such a common y-y- Semitic language ever existed at all — had long since split into individual languages by the 3rd millennium BC, of which Akkadian from Mesopotamia and the Eblaite Semitic language from Syria are already known from written records from the 3rd millennium,

980
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It also becomes clear from what follows that the disintegration of the Indo-European basic language was caused by the demographic explosion that necessarily accompanied the food-producing economic form, but in a most miraculous way, this explosive population growth not only occurred from the 9th to the 3rd millennium BC -- as if “frozen” -- but also, for the first time, did not force the population of the Indo-European ancestral homeland to migrate, although according to Soviet authors, the ancient Indo-Europeans lived in the area of one of the most ancient foci of the food-producing economic form, where - e.g. at the Zavi Chmri settlement - the domestication of goats and sheep can be dated as early as 8900 BC, and as early as 8000-7500 BC -- e.g. at Cayönü - developed agriculture, animal husbandry and architecture. archaeologists have discovered, meaning that the explosive population growth should have occurred first among them. No, according to Soviet authors, this Neolithic population growth first occurred among the Hurrians living in Ciscaucasia, who made their way south, encountered the ancient Indo-Europeans, and displaced them.

“In any case, we know that this is what they did to the Indo-European community

that once occupied the Mitanni territory — the Indo-Iranians,” Istvánovits writes in his review. We can see that a unique prehistoric concept unfolds before the wondering eyes of the bona fide reader in the above lines. According to the authors, in Ciscaucasia, i.e. north of the Caucasus, in present-day Southern Russia, the Hurrians, to which area, according to the clear evidence of archaeological finds, the agricultural-animal husbandry economic form only reached millennia later (we only have data on agriculture and animal husbandry from the immediate northern foreland of the Caucasus from the turn of the 4th/3rd millennium BC, and even later in Istvánovits's review itself we can read that the oldest food-producing archaeological culture in the area bordered by the Urals, Volga and Dnieper, which adjoins Ciscaucasia from the north, is the so-called pit grave or kurgan culture belonging to the 3rd-2nd millennium BC), than according to the assumption of the Soviet authors, the ancient Indo-Europeans living in Eastern Anatolia and Northern Mesopotamia, in whose places of residence — as we already know — already existed The transition to agriculture and animal husbandry began in the 9th millennium BC, but despite this, the population growth that necessarily accompanies a food-producing economic form would have occurred earlier among the Hurrians than among the ancient Indo-Europeans, and they were only forced to move out of their alleged homeland in Transcaucasia and northern Mesopotamia under pressure from the Hurrians.

That is, according to the view of Soviet authors, the ancient Indo-Europeans were agriculturalists and animal breeders during the period of about 6,000 years, from the 9th to the 3rd millennium BC. Despite their education, and later their bronze art and their developed urban culture, there would have been no demographic explosion. The authors remain indebted for explaining this absurdity.

981
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However, we then encounter another irresolvable contradiction regarding the hypothesis of the two Soviet authors. After we had previously registered with astonishment that Gamkrelidse and Iwanow, in their study published in the June 1990 issue of “Spektrum der Wissenschaft” (Die Frühgeschichte der indoeuropäischen Sprachen), simply named the proven inflected Hurrian language as a branch of the Indo-Iranian language group, now in the review Istvánovits (who, as he writes, describes the nature and significance of Gamkrelidse and Iwanow's discovery partly based on the praise of Roman Jacobson) writes the following: „ At the same time, a very large number of loanwords were transferred to the Indo-European basic language (and from there to other languages of the region) from other languages of the region in question: from Hattü, Sumerian (these can be considered extinct by the end of the 3rd millennium BC), Elamite, Hurrian-Urartian, etc. — which were neither Semitic nor Kartvelian nor Indo-European languages.” That is, here Istvánovits — or more precisely Roman Jacobson — in stark contrast to the two Soviet authors clearly and unequivocally states that the Hurrian language — and moreover all the other languages of the region in question listed above — did not belong to the Indo-European languages. It is extremely instructive and absolutely worth mentioning in this connection that Istvánovits apparently does not even notice that with these lines he is quoting Jacobson, since — as we have already mentioned — he first claimed, following Gamkrelidse and Iwanow, that the Hurrian language was an “Indo-European dialect.” (1. above.)

Finally, we would like to emphasize once again that the multiple glaring contradictions described above, which appear between the Soviet authors' hypothesis of a Transcaucasian-Eastern Anatolia1-Northern Mesopotamian Indo-European ancestral homeland and the numerous well-known archaeological complexes from the Neolithic, Chalcolithic and Early Bronze Age of the region in question, simply qualify Gamkrelidse and Iwanow's ideas in this regard as absurd. Moreover, we already have written sources from the 3rd millennium BC, which all testify that the area in question — as far as we can look back into the past with the help of written records and the finds of archaeological civilizations of the Neolithic and Early Metal Age — was inhabited by the Sub-Aryans, Hattians and Hurrians, who spoke the Akkadian language, since the most ancient times, and that until the first half of the 2nd millennium BC — that is, until the appearance of the Hittites — no trace of an Indo-European-speaking population can be found in this region,

In this respect, however, the most striking evidence against Gamkrelidse and Iwanow's East Anatolian -- Transcaucasian -- North Mesopotamian Indo-European ancestral homeland thesis is the fact that a proven non-Indo-Germanic, so-called Anatolian Aegean Tyrrhenian language community was actually East 982

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It left the clearest linguistic signs of its former existence in Anatolia: the ancient, pre-Indo-Germanic Anatolian-Aegean-Tyrrhenian geographical and toponyms, spread from Celtic Anatolia through the Aegean region to the Carpathian Basin, as well as Italy and Iberia, occur most frequently precisely in Celtic Anatolia (see Schachermer 1979, pp. 230-234, maps in Figs. 122, 123, 124, 125 and 126). Our readers can find a detailed presentation of our linguistic and prehistoric knowledge of the Anatolian-Aegean-Tyrrhenian language community, as well as a discussion of the close Sumerian linguistic connections of this ancient language community, in our book, pp. 845-880. can be found on the pages of the journal, therefore it is not necessary to discuss this complex of issues again this time, but we would simply like to point out again — and with great emphasis — that according to the currently generally accepted conclusion of international linguistic and prehistoric research, this Anatolian-Aegean-Tyrrhenian language community belongs to the Indo-Germanic languages-

This fact, taken alone, and without taking into account all the other contradictory data listed above, represents the clearest and most unambiguous refutation of the ideas of Gamkrelidse and Iwanow.

983
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Chapter 3
THE LATEST RESULTS OF HUNGARIAN HISTORICAL RESEARCH

On May 12, 1989, the "Department of Linguistics and Literary Studies" of the Hungarian Academy of Sciences held a public class meeting entitled "Half a Century of Hungarian Historical Research". The lectures given there were published in the 1990 (March) issue of "Magyar Tudomány". This overview was necessary mainly because the last prehistory symposium took place sixteen years earlier, in 1973 (organized by the Kőrösi Csoma Society), and its material was published in 1977 under the title "Hungarian Prehistoric Studies", edited by Antal Bartha, Károly Czegledy and András Róna-Tas. This volume of studies should be quite familiar to our readers, because the published articles are used from several aspects: On the one hand, more than once to support our own perspective, on the other hand, many times to present the logical and other contradictions of the prehistory ideas contained in them, and not least, to provide archaeological, prehistoric, linguistic or historical source data that were mostly consistently silenced until recently by the trend of Gyula László, which preaches our exclusively Finno-Ugric origin. to explore and describe it in more detail — to the "another idea is possible" is the motto of the extent that the studies published in the volume proved to be useful for this purpose.

Given this background, there can be no doubt that we must also present the latest views of the school of prehistory and linguistics, which is based exclusively on our Finno-Ugric origin, "officialized" by the Hungarian academic trend, in a series of studies that considers the comprehensive criticism of this exclusively Finno-Ugric Hungarian trend as one of its most important objectives. Therefore, below we will take the presentations given at the above-mentioned class meeting one by one and examine whether the academically generally accepted Hungarian prehistory has changed at all since 1973.

The first study comes from János Harmatta, entitled; "The Ancient History of the Hungarians". Generally looking at Harmatta's article, we can already state in advance that the main direction of the entire thought process of the paper is that the Ancient Hungarians, and even the Finno-Ugric peoples, from the time of the formation of the so-called "Finno-Ugric basic language", i.e.

984
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According to Finno-Ugric linguistics, starting from about 4000-161 BC, for many millennia they were raised on the cultural heritage of the Iranian peoples, who were at an incomparably higher level of culture, and practically their entire material and spiritual culture, economic and social development could be attributed to the cultural influences originating from the Urals. The Iranologist Harmatta therefore does not deny himself - to be more precise, he clearly cannot get rid of the narrow perspective of his own special field, not to mention his "tube vision" - and, like an unadulterated over-specialized specialist, he looks at everything through "Iranian" glasses. We can consider Harmatta's opinion that the language of the Xiongnu people - that is, the Asian Huns - was an East Iranian language of the Saka type (more on this later in 1.).

In his study, Harmatta also deals with the issues of language families and the basic languages of language families. In this area, he makes some very thought-provoking statements, although — as we will show below — he once again shows the incomprehensible contradiction that we could already register in his article (Iranians and Finno-Ugrians, Iranians and Hungarians) published in the volume "Studies of Hungarian Prehistory"; when on one page he wrote that modern Indo-Germanic studies had already given up the thesis of the Indo-Germanic language family tree, and that the term "Indo-Germanic" was merely a linguistic concept, so that, for example, we cannot speak of an Indo-Iranian ancestral people, and on the next page he flatly stated that the Indo-Iranian tribes — i.e., according to his own previous words, a non-existent ancestral people — were around They separated from the other Indo-Germanic peoples around 5000 BC (L. Harmatta: Hungarian Prehistoric Studies, 1977, pp. 168 and 169; — more detailed information in our book, pp. 426-427).

In this study, Harmatta first reviews the changes in perspective that occurred after the Second World War in the fields of prehistory and linguistics. He once again emphasizes that the contradictions between the language family tree theory and the assumption of a single basic language have become increasingly obvious. According to Harmatta, the fundamental starting point of the new perspective is a clear definition of the concept of genetic language kinship and its defining features. According to this, only languages can be considered genetically related if it can be proven that they once formed a common linguistic communication system. In parallel, instead of the neo-grammatical idea that explains the formation of a basic language as the linear development of the language of a single language community, the perspective of integration that gradually emerges from the continuous contact of several different language communities, but only reaches the communication threshold, is increasingly coming to the fore ~ writes Harmatta.

985
===== PAGE 974 =====

However, this means in Hungarian — although Harmatta's wording (apparently not by chance) carefully avoids the terms "areal" and "language equalization" — that Harmatta acknowledges — at least in principle — that the so-called "language family base languages" of language families previously thought to consist of languages that are genetically related to each other, i.e. languages related to each other by descent, are, according to the new linguistic approach, merely linguistic alliances that have developed as a result of areal language equalization. were formations, i.e., through the long coexistence and constant intensive contacts of several, originally different language groups, their languages became more or less unified over time, i.e., more or less common linguistic features — so-called "isoglosses" — developed between them, and thus this linguistic equalization could even reach the threshold of mutual intelligibility. Then, as the contacts became looser (e.g. due to smaller or larger migrations), the languages in question further developed separately in groups, towards individual languages.

Much more significant than this theoretical position, however, is Harmatta's statement that Hungarian Uralistics has also revealed that "... the development of the Finno-Ugric basic language also took place in the process of areal relations, because the distribution of typological features clearly points to four or five different groups, and the opposing peripheral areas do not have preserved identical ancient features."

So, this latest theory on the formation of the so-called Uralic-Finno-Ugric language family, espoused by Hungarian Uralistics, exactly matches the areal language leveling approach, which was presented on pp. 329-333, 498-511 and 650-653. We have already represented ourselves in the volumes of "The Sun Rises in the East" published so far a decade ago. (cf. e.g. our book

In our thousand-year train of thought, pages 498-511 of the current edition of our book entitled "Dissect only the tongue!" We started from the linguistic-statistical tables of Gyula László's fundamental work entitled "The Earliest Sections of Our Prehistoric History" (L. László 1961, pp. 36—39) and Gyula László's explanations to outline the relationships between the Ural-Finno-Ugric languages in a more realistic way than the genetic language family theory. From these tables of Gyula László — in which he presented the Samoyed and Lapp word cognates found in other Ural-Finno-Ugric languages (and also the ancient Altaic and Indo-Germanic word cognates of the Ural-Finno-Ugric languages, which are not important for us in this context) — it is clearly evident that the Finno-Ugric peoples (Lapps, Baltic-Finns, Ostyaks) who still live further north near the Samoyeds have much more Samoyed word cognates (211 in Lapp, 10 in Baltic Finnish). 248, in the Ostyaks 217) than in the southern ones

986
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(161 in Chereemis, 177 in Zirjen, 178 in Vogul), however, the number of word matches decreases significantly again among those living in the southernmost band (143 in Mordvin, 140 in Votyak), while in Hungarian it is even 14 times less (124). The distribution of the Finno-Ugric vocabulary shared with the Lapp language shows the same picture: most of it is found in the Samoyeds, who still live in the eastern neighborhood of the Lapps today (142 words), much less in the Baltic Finns (109 words), again much less in the languages of the Ob-Ugrians, Volga-Finns and Permians, Ossetian 81, Vogul 77, Chereemis 75, Mordvin 75, Zur Jen 67, Votjak 62 words), and in the Hungarians — compared to the other languages — almost insignificant (38) words. Thus, the number of words shared with the Lapps, just like the number of Samoyed word cognates, decreases regularly in direct proportion to the increase in the geographical distance of the individual Finno-Ugric peoples from the Lapps or Samoyeds.

From these regular word-matching distributions, Gyula László concludes that in ancient times the geographical location and spatial relationship of the individual Uralic peoples may have been similar to today, but not as widely dispersed as we find them today. He then writes: yÿ From what has been said, we can conclude that in the Uralic period we cannot imagine complete linguistic and territorial unity either, but that the Finno-Ugric genera and genera groups may have lived in a fairly significant territorial division - roughly corresponding to the later separation. It seems that we can only speak of a Uralic basic language with a completely uniform vocabulary in theory, and even then only in the most elementary expressions. In the times still accessible to us, we may have been talking about close-knit genera and sister languages, which, however, already had their own vocabulary, albeit to a lesser extent. Among the linguistic concepts, the language chain theory is closest to this idea. (l. László 1961, p. 38) — We can see from the quote that already in 1961, Gyula László, although very cautiously, but still firmly raised the idea that other possibilities for explaining the relationships between the Ural-Finno-Ugric languages are conceivable, not only the academically "officialized" rigid Ural-Finno-Ugric language family tree theory.

If we now interpret the above-mentioned distribution of Samoyed and Lapp word cognates not based on the genetic language family concept, but from the perspective of the theory of areal language equalization, then already at first glance we can clearly distinguish the Lapp and Samoyed ethnic groups as those that originally formed two separate linguistic components in ancient times in the later areal language equalization processes of the Uralic languages, and some of whose words were incorporated into other Finno-Ugric languages in greater or lesser numbers depending on their geographical proximity, i.e. the intensity of neighboring contacts.

===== PAGE 976 =====

- This view is in full accordance with the thesis generally held by Finno-Ugric studies, according to which the Lapps did not originally belong to the Finno-Ugric language family, but only relatively late adopted their present-day Finno-Ugric language from their Finnish neighbors. On the other hand, our above line of thought is also in agreement with the theory, which has been advocated for some time by several renowned Finno-Ugric linguists, that not only the Lapps, but also the Samoyeds, Voguls and Ostyaks were not originally Uralic or Finno-Ugric peoples, but spoke some Paleosiberian or Arctic dialects, and also only in late times adopted the languages of the Finno-Ugric peoples living near them and partly among them. Consequently, the Uralic, i.e. Finno-Ugric-Samoyed languages, taken in the classical sense, can be rightly divided into a northern and a southern group. — We are not the first to raise this question. Completely identical views are held by, for example, Gyula László (l. László 1961, p. 68, note 49),

and G. Ránk (l. Ránk 1968, pp. 5-6). The relevant literature can also be found in both cited places. Similarly, according to Ravila, Nielsen, Lagercrantz, etc., the Lapps and the Ob-Ugric peoples are not only anthropologically different from other Finno-Ugric peoples, but their languages also have common features that are absent in other Finno-Ugric languages, but are present in Samoyed (l. Juhász 1981, p. 9). All this therefore fully corresponds not only to the language chain theory of Gyula László, but also to the beliefs of many other renowned researchers. Projecting these ideas onto an archaeological level, we can look for the ancestors of the Lapps among the fishing-hunting ethnic groups of the Paleolithic-Mesolithic Kunda archaeological culture excavated in the Baltic and the Upper Volga region, and the ancestors of the Samoyeds among the ethnic groups of the Sigr culture in the Ural region, also living on a fishing-hunting plateau (l. László 1961, p. 193). At the same time, Gyula László looks for the ancient Uralians in the narrower sense among the bearers of the late Svidrian archaeological culture extending from Poland to the Oka. These Swedes, who were also Paleolithic-Mesolithic fisher-hunters, migrated north and northeast in the middle of the 3rd millennium BC, as a result of the northern expansion of the southern agricultural cultures (Tripoli culture, late pit-grave culture), and penetrated the Eastern European forest belt (there is concrete archaeological evidence of these migrations), and their western lineages — the ancestors of the later Finnish groups — mixed with the proto-Lappid Cundaeans, and their eastern lineages — the ancestors of the Ob-Ugrians — mixed with the proto-Samoyed Sigris (l. László 1961, pp. 194-195).

We could add to this argument, which is indeed very convincing and supported by archaeological data, that we can hardly see in the Swedes proto-Uralians in the sense in which this ethnic concept -- and their proto-language -- is defined by Finno-Ugric linguistics. The original, 988
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In fact, to the Sigr tribes that have not yet mixed with the Swedes = taking into account the clear, irrefutable results of anthropological research -- in addition to the pre-Samoyeds, we must indisputably include the distant ancestors of the Ob-Ugric peoples: the anthropological character of the Voguls and Ostyaks shows virtually no commonality with the groups of Finns, but they are close to the Samoyeds in anthropological terms, and their languages also contain much more Palesiberian-Arctic elements found only in the Samoyed languages than in the Finnish languages proper. Therefore, if neither the Samoyeds nor the ancestors of the Ob-Ugric peoples were among the Swedes — and this can be assumed with a probability approaching certainty from the above — then in their case we cannot speak of a "Uralic", or even a "Finno-Ugric" pre-ethnic group, at most of a "Proto-Finnic" one. Going further, under such circumstances, we cannot consider the language, or rather the languages, of the Swedes to be some kind of "Finno-Ugric", perhaps in a vast area where Swedish culture was spread, and taking into account the very low population density that necessarily follows from the fishing-hunting lifestyle of the Swedes, we cannot in any case speak of the possibility of the development of some kind of "basic language" - or more precisely, a communicative "Uralic" basic language family, but only a Proto-Finnic language chain. We spoke about the languages of the Swedes earlier because the area from the Vistula to the Oka language - even if it is relatively uniform.

Thus — continuing our previous line of thought — with the migration of the Sweder clan groups located south of the pre-Lapp, pre-Obi-Ugric and pre-Samoyed Kunda-Sigirian ethnic groups to the north and northeast, the mutual influences between the dialects of the Kunda-Sigirian — originally in all probability Arctic — ethnic groups and the Sweder proto-Finnic clans gradually became more intense due to the geographical proximity, and the earliest stage of the long process of language equalization began. We can also date these migrations and, with them, the earliest beginnings of the development of the pre-Uralic or pre-Finno-Ugric areal language chain in an archaeologically satisfactory manner: all this can obviously be considered simultaneous with the appearance of comb-pitted pottery in the Baltic, Upper Volga and Oka-Kama, i.e. around the middle of the 3rd millennium BC. However, we emphasize again that this date can only be referred to the very first stage of the language leveling processes of today's Ural-Finno-Ugric languages, because with such intense areal linguistic contacts between each other, which led to the leveling of the national-tribal languages of the entire Eastern European forest belt to such an extent that a common, understandable communicative language could only have developed after 2000 BC, i.e., the spread of the agricultural economic form in these areas and the associated

989
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could be imagined after a concomitant huge population increase, but never under fishing-hunting conditions. — A very good proof of this is the result of paleoanthropological studies, which showed that the Lapp race dominates in the anthropological material of the northern finds of comb-pitted pottery (l. Lipták 1977). These northern pottery finds are therefore not yet the relics of the later Finno-Ugric peoples, at least in earlier times, but come from the Kunda pre-Lapps who did not yet or only very slightly mix with the Swedes who migrated north, which proves that although the Kunda people learned the art of making pottery very early on from the Swedes who migrated north, the contacts between them were not yet so intense as to lead to anthropological mixing and, consequently, to linguistic equalization.

The migration of the Swedes to the north, however, pushed some of the Kunda-Sigr fishing-hunting clans even further north than their previous residences. The consequences of this secondary migration are also evident in archaeological finds: towards the end of the 3rd millennium BC, the finds show the migrations of the ancestors of the Lapps in the west to the north and the ancestors of the later Samoyeds and Ob-Ugric peoples to the east in a northerly or north-easterly direction (l. László 1961, pp. 193-194).
Going further, in the northernmost groups of the Swedes, who lived closest to the Kunda-Sigirian pre-Lapps, pre-Samoyeds and pre-Obi-Ugrians, were the first to penetrate their territories during the northward movement, and over time they mixed the most with the Kunda-Sigirian tribes that remained in place. We can observe the ancestors of the later ethnic and linguistic divisions of the Finno-Ugric peoples, from which those national and linguistic groups were then formed, in which the most Lapp and Samoyedic basic word similarities are found. These would be the Finno-Ugric peoples and languages forming the northern band mentioned above, primarily the Baltic-Finnic group, and further east the northern wing of the Volga and Permian, the Cheremis, and the Zürjén, with their 248 (in the Baltic languages), 161 (in Cheremis) and 177 (in Zürjén) Samoyedic cognates, and 109 (in the Baltic languages), 75 (in Cheremis) and 67 (in Zürjén) Lapp cognates.

In the course of these discussions, therefore, in contrast to the east-west division of the Finno-Ugric peoples and languages by the traditional Finno-Ugric language family theory, a combined, more north-south, but also to a certain extent east-west, distribution also unfolds before the eyes of the attentive reader.
Let us now take a look at the more or less distinguishable ethnic and linguistic groups that originally seemed separate, whose languages, according to the latest findings of Uralistics, contributed to the Ural-Finno-Ugric basic language.

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language in the process of language equalization, As we saw above in Harmatta's study, Uralistics assumes four or five such originally linguistically distinct ethnic groups. Let us see, therefore, to what extent this new Uralistic recognition is compatible with everything we have discussed above. We ourselves - completely independently of the new theory of Hungarian Uralistics mentioned by Harmatta -- have mainly used in our previous discussions the book "The Earliest Stages of Our Prehistoric History" published by Gyula László in 1961.
We relied on his work, as well as on our series of studies entitled "The Sun Rises in the East", especially on the volume "Boncold csak nyelvész!", which was published in 1982. So no one can say that we have taken anything from Harmatta's new Uralistic theory. -- Well, let the facts speak for themselves, that is, the considerations regarding the areal approach to the relationship of the Uralic-Finno-Ugric languages, which we mentioned earlier,

We have succeeded in distinguishing the following originally undoubtedly separate folk and linguistic groups:
1/ The ethnic groups of the Kunda archaeological culture -- most likely the Proto-Lapps.
2/ The bearers of the Sigr culture archaeological culture — more than likely the proto-Samoyeds and the proto-Obi-Ugrians.
3/ The groups of the population of the late Swedish archaeological culture that migrated north into the forest belt — in all likelihood the Proto-Finns.

3/a. The northernmost genera of these Swede groups are most likely the distant ancestors of today's Baltic Finns, Cheremis and Zurs, who can be called the northern wing of the Finns.

3/b. The southern branches of the Swedes who migrated north - most likely the early ancestors of the modern Mordvins and Votyaks, who form the southern wing of the Finns,
4(To these we would add the agricultural, livestock and bronze-working colonists who arrived en masse from the south in several waves between 2000 and 1500 BC in the Volga, Southern Urals and Ob regions (more on these |. below), who formed the populations of the Andronovo, Abasevo and Balanovo archaeological cultures, and among whom we should in all probability also look for the Proto-Hungarians,

However, Harmatta then says that the beginnings of the early Neolithic cultures of the area later inhabited by Finno-Ugric people can be dated to the 5th millennium BC based on calibrated C" data, and this suggests that each of the populations of these cultures may have participated in the areal development of the Finno-Ugric basic language. In connection with this idea, we must point out two very important facts that support Harmatta's above position:

991
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are clearly contradictory. One such fact is that the Neolithic, or New Stone Age, archaeological civilizations of the "later Finno-Ugric inhabited area", i.e. the agricultural-animal husbandry economic stage, can be dated by traditional archaeological dating only to 2000 BC, and based on calibrated C" measurement results, only to 2500 at most, and not to the 5th millennium BC, as Harmatta writes - not to mention the fact that we have already shown above in this book that C" datings in their current form cannot be considered a scientific method, because the carbon atoms that have entered the plants from the carbonates of the soil are not taken into account in the measurements, and therefore there are often millennial differences between the ages determined by C" measurements and the dates obtained by traditional archaeological methods, in each case in such a way that the C" method always makes the archaeological finds in question appear to be much older, than their real age. But even apart from that, Harmatta is quite obviously using the same fundamentally flawed and extremely misleading archaeological terminology that István Fodor used in his book "Verecke's famous path..." and whose unmethodicality we have already pointed out twice in the volumes of "Keleten kél a nap" published so far. (see pages 309-310 and 666 of our book)

In short, the point here is that there is considerable confusion in the naming of certain paleoarchaeological periods. One such inaccuracy is the misleading and outdated use of the term Neolithic. For over 40 years, modern archaeology has been referring to cultures that were exclusively food-producing, i.e. farming and/or animal husbandry, as Neolithic. In contrast, in the past, the Neolithic was generally associated with the appearance of polished stone tools or ceramics.

concept. However, it has been proven many times that none of these criteria can be considered a defining characteristic. On the one hand, food production can be associated with chipped stone tools without any further ado (e.g. in areas where there is no easily polished stone type), and on the other hand, in several sites it has been known for a long time that Since the 10th century, agriculture and animal husbandry have been evident, but ceramics only appeared centuries, sometimes even millennia later. This variation is called the Neolithic without ceramics or before ceramics (aceramic or pre-ceramic). However, the opposite also occurs: quite often we come across archaeological cultures of fishermen and hunters who did not yet know how to produce food, but which used ceramics. Such cultures learned the art of making clay from their more advanced neighbors who lived in the Neolithic. In these cases, it is obvious that we cannot speak of Neolithic (Neolithic) cultures according to modern archaeological terminology, since the archaeological cultures did not know food production.

992
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We are faced with exactly this latter phenomenon in the archaeological cultures that Harmatta calls early Neolithic civilizations and, based on calibrated C¹⁴ measurement data, he believes can be dated to the 5th millennium BC. Incidentally, it is practically the same finds that are being discussed in István Fodor's book "On the famous path of Verecke..." also on pages 40-47 of his book, where he consistently calls the archaeological culture of the Kelt-Ural region, which was mainly excavated by VI Chernyvetsov and actually dates back to 2500-2000 BC, and which had already adopted ceramics from its southern neighbors but had not yet practiced agriculture, a Neolithic culture and, based on Chernyvetsov's original dating attempts, which were made about 20 years before the publication of Fodor's book (1975), but which have since become irretrievably outdated, he dates it to the 4th-3rd millennium BC. Now, Harmatta, despite all the chronological corrections that have occurred in the meantime, still calls this Chernyvetsov-like Mesolithic fishing-hunting archaeological culture and its neighboring Kama region, also fishing-hunting, Neolithic, i.e. New Stone Age, using calibrated (04) data from the second half of the 3rd millennium BC, even in 1989.

We have already seen that since no trace of food production was found in the culture in question — as Fodor himself explicitly acknowledges in "The Famous Way of Verecke..." — this culture can in no way be called Neolithic, even if its bearers were familiar with ceramics. On the other hand, in our comments to Fodor's book, we also showed that it is impossible to date the archaeological culture in question in the Eastern Urals to the very beginning of the 4th millennium BC, as Fodor does, because the clear, undeniable serial parallels with the Early Middle Ages in the finds from the very beginning make it evident that this Ural culture was organically connected with the Early Middle Ages culture, and according to all available data it was created through the northward expansion of the Early Middle Ages culture. The oldest site of the Eastern Minarian culture, however — settlement layer No. 4 of Dzambasz Kala — dates from the first half of the 3rd millennium BC, around 2700. (1. László 1961, p. 105) It is therefore obvious that the fishing-hunting culture in question in the Eastern Ural region, in which small remnants and traces of the Eastern Minarian culture are found everywhere, cannot itself be earlier than the settlement of Dzambasz Kala 4. According to this, these fishing-hunting settlements in the Ural region can be dated to between 2500-2000 BC at most, but in no case earlier.

We have already explained all this in more detail (pp. 309-312, 312-315 of our book), but due to the eminent importance of the question, and especially since Harmatta still makes the same mistakes in this regard in 1989 as Fodor did in 1975, it is considered absolutely necessary to

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We must once again emphatically point out the untenability of these views. — It seems beyond doubt that the Finno-Ugric school of thought insists convulsively on stylizing these fishing-hunting settlements in the Ural and Kama regions as Neolithic, and at the same time on aging the remains, in order to present at least some archaeological support for the Finno-Ugric and Uralic basic languages and basic populations inferred by linguistics to be around 4000 and 6000 BC, respectively. The reason why this archaeological "support" has succeeded in the way we have shown above is to be found solely in the fact that the hypothesis of the Ural-Finno-Ugric language family has been a set of absurdities concocted at a desk from beginning to end throughout its existence.

However, Harmatta then takes the same position on a very important issue as we do. He writes verbatim: "It is true that the western limit of the distribution of the Siberian pine (Pinus Sibirica) does not even reach the western limit of the Kama-Ural Neolithic culture, but the name of the tree can only be found in the Permian, Ob-Ugric and Samoyedic languages, i.e. in the actual distribution area of the tree, and there is no evidence that it existed in other Finno-Ugric languages."

In other words, with these lines, not just any dilettante, but the qualified linguist Harmatta, flatly rejects Péter Hajdú's (and, following Hajdú, István Fodor's) assumption about the Uralic people's homeland in Western Siberia along the Lower Ob, which was based on Hajdú's hypothesis that the name of the stone pine and some other taiga tree names would once have been found in the Uralic base language, but that the Finno-Ugric peoples further west would have forgotten these tree names over time, and that is why we no longer find the names of the trees of the taiga forest among them today.

We would like to remind you here that Gyula László had already rejected Hajdú's hypothesis in 1964, for the simple reason that in the area designated by Péter Hajdú, there was no trace of human life in the period he mentioned. And where there are no humans, there could not have been an ancestral homeland, he adds at the end (1. László, 1964.). Furthermore, in our volume entitled "In Double Measure ...", in 1981 we ourselves sharply opposed this completely improbable idea of Hajdú, which Fodor enthusiastically promoted in his book "The Famous Way of Verecke ..." and even further developed, insofar as he believed that the effects of the East-Minar culture, which began around 2700 BC, were already noticeable around 4000 BC in the above-mentioned Chernyvetsov-like Eastern Ural fishing and hunting settlements, of which, according to him, 16% of the population was made up of the still unified Uralic native population.

We also pointed out there that the assumption of forgetting tree names is extremely unlikely, but it is completely inconceivable that the European deciduous forest and the Siberian taiga forest could have met each other in the northern part of the Ural Mountains earlier than in the Southern Urals — as 994

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This was claimed by Péter Hajdú against Gyula László at the time (all this 1, pp. 305-315 of our book). Then Harmatta fgy fr: "In the 5th millennium BC, south of the archaeological cultures associated with the Urals, the Dnieper-Donets culture, which can be associated with the Baltics, and the pit grave culture of Indo-Iranian ethnicity were located. Their populations may have played a certain role in the integration process of the Uralic basic language." - In connection with this statement, we must definitely note that although we readily acknowledge the important role of the populations of the aforementioned archaeological cultures in the areal language equalization processes of the development of the so-called "Uralic basic language", unfortunately, international archaeological research dates the Dnieper-Donets culture to 3000 BC, but at most to the end of the 4th millennium, the beginnings around 3000 are much more probable. It can be proven that the Dnieper-Donets culture appeared during the B/I stage of the Tripolje culture, which stage, in turn, belongs between 3000 and 2800 (1. e.g. Müller-Karpe, 1968, p. 169 and Müller-Karpe 1968, "Tafelband", table no. 4).

It is also necessary to know that the relationship between the Dnieper-Donetsk culture and the pit-grave culture is not as simple a question as the unsuspecting reader might think from Harmatta's text quoted above, since Harmatta's line of thought would actually imply that the Dnieper-Donetsk culture and the pit-grave culture were two simultaneous and adjacent archaeological cultures in the Eastern European region south of the Finno-Ugric settlement areas. The real situation, however, seems to be that the Neolithic and Early Metallurgical cultures of southern Russia

"There has been considerable uncertainty in the archaeological literature for decades regarding the names of cultures of the period. For example, the names of the Dnieper-Donetsk culture are following:

- 1./ Dnieper-Donetsk culture;
- 2./ North Pontic culture;
- 3./ Mariupol culture;
- 4. early ochre grave culture;
- 5./ early pit-grave culture;
- 6./ Kurgán I. Culture (Gimbutas)

As we have already mentioned, this culture began around 3000 BC and lasted until 2500. As its name suggests, it was spread between the Dnieper and the Donets. Around 2500, the so-called Late Pit-Grave or Late Ochre Grave or Kurgan culture (Gimbutas Kurgan II culture) appeared in the areas of the Dnieper-Donets culture, but extending eastwards to the Volga. However, this cannot be derived from the earlier Dnieper-Donets culture, because around 2500 the population of the area in question was practically completely replaced within a relatively short period of time; short-lived peoples arrived from the south.

===== PAGE 984 =====

A large population filled the entire area of the culture, while the population of the Dnieper-Donets culture consisted of long-headed Paleo-Europeans (more on this in our book 1, pp. 801-803).

Under such circumstances, it is therefore quite difficult to imagine that the term Dnieper-Donetsk culture should mean Baltics, while the partly synonymous term Pit Grave culture would at the same time cover Indo-Iranians. However, since it is not clear from Harmatta's previously quoted lines on this subject exactly what he means actually under the names of the Dnieper-Donetsk culture on the one hand and the pit grave culture on the other, so we will leave the solution of this dilemma to Professor Harmatta. Therefore, we would like to note only this regarding this issue that these "Balts" and "Indo-Iranians" probably became Baltics and Indo-Iranians in Harmatta's idea precisely because this is imperatively required by the linguistic hypothesis inherited from the 19th century, according to which the Uralic base language contained Proto-Indo-Germanic loanwords, and the Finno-Ugric base language contains a series of Indo-Iranian or Proto-Iranian loanwords. Therefore, we should not be surprised that after Harmatta explained that the Baltic and Indo-Iranian populations of the Dnieper-Donetsk culture and the Pit Grave culture, in his opinion, played a role in the integration process of the Uralic base language, he immediately enthusiastically adds: "Linguistic research has long noticed that the Uralic base language contains Indo-European and the Finno-Ugric one contains Indo-Iranian or "Old Iranian linguistic elements can be detected." — He obviously did not think carefully about the fact that with this "prelude" he himself revealed the "purposefulness" of his 616001 assumption, i.e. that the population of the Dnieper-Donets culture had to become "Baltic" (i.e., actually, "Proto-Indo-European" — it should be noted that Indo-Germanic studies consider the Baltic languages to be extremely conservative, which still preserve numerous Proto-Indo-Germanic features today), and the population of the pit-grave culture had to become "Indo-Iranian" in order to give the idea, which was born in the 19th century as a result of purely linguistic speculations, and which — as Harmatta writes, "linguistic research had long since noticed" — at least the appearance of archaeological supportability through a retrospectively invented archaeological hypothesis. — Such "vicious circles" are also very typical of Hungarian prehistory research with a Finno-Ugric perspective. It is no wonder, since

This case is an excellent example of the "methods" used by representatives of academic Hungarian scholarship when they strive to find a way to study the language that was once

===== PAGE 985 =====

The generally known fact that the so-called "Uralic" language family has practically no older linguistic monuments makes the Proto-Uralic-Proto-Sumerian language comparison methodologically unfeasible (we can only trace the monuments of the Hungarian language back to 950 AD, with the help of Hungarian slang words in the work "On the Governance of the Empire" by Emperor Constantine the Great, while the earliest monuments of the Finnish language practically only date from the 16th century, and the oldest known word forms of the other smaller Finno-Ugric languages date from the 18th-19th centuries). In Hungarian, therefore, there is no question of our being able to learn the so-called "sound history" of the Uralic languages with a scientifically demanding methodology, seven full millennia before the earliest linguistic monuments (Uralistics dates the formation of the so-called "basic Uralic language" to around 6000 BC). Despite this, this gaping gap of seven millennia in the field of linguistic monuments of the Uralic languages does not obviously constitute any methodological obstacle for Harmatta in reconstructing the "sound history" of the Uralic languages. Rather, he believes that it prevents the Proto-Uralic—

===== PAGE 986 =====

Although this question is completely unimportant from the Hurarian prehistory point of view and has at most the significance of curiosity, we still considered it necessary to mention it, because it once again sheds a very characteristic light on the completely unfounded methods with which Harmatta works in setting up his various hypotheses, and whose closer and more distant circumstances and connections he examines very superficially. First of all, as regards Harmatta's identification of a part of the star-named "kasva-people with the Kassu ethnic group that occupied Mesopotamia in the first half of the 2nd millennium BC, it is absolutely necessary to point out the fact that the name Kassu was the Akkadian-Babylonian Semitic name of the people in question, while the Kassu called themselves "gal-zu" or "gal-du" (L. Balkan: Kassitenstudien I), c.f. with the second part of the Kassu royal name "Kuri-gal-za". However, if we want to compare people's names linguistically, then it is evident that in the case of the Kassuks, the name "kasva" – a question cannot be compared with the Akkadian-Babylonian word ka3su, but with the people's own name, i.e., the word "gal-zu" or "gal-du". The word form "gal-zu" or "gal-du" cannot be derived from the name "kdsva". Secondly, as for the tribes of the Western Turkic Empire called Ko-sha in Chinese sources, it is methodologically obvious that they were not, first of all, supposedly living in the Turkmen region 3,000 years before the emergence of the Turkic Empire, but can be dated to 998

It is not possible to compare the name of the tribes called Ko sha with the hypothetical people's name *Kasva, which is only linguistically inferred, but we must first take into account that the name of the leading tribe of the Ku šn empire, which existed in the 1st-3rd centuries AD, that is, barely 300 years before the formation of the Kökük empire, sounded like yī shi or Kui-shang in Chinese sources (1. e.g. Hauszig, 1953), which I especially the Kū-shī variant — is almost identical in its phonetic form to the name Ko sha. In other words, the Ko-sha tribal names of the Western Turks could clearly have denoted the surviving fragments of the Kushans from barely 300 years earlier, rather than the remnants of an alleged (let's not forget: this is only a star-shaped, i.e. linguistically inferred, not a truly existing, people) people from the 3rd millennium BC.

1 / Osirani: *arba-> Finnish orpo , orphan, Hungarian orphan"; Sumerian: ur-ba = "eins, einzeln, einzig" (one, individually, single, sole). Note: only figurative meaning compliance.

2 / Oirāni: araya->*alya->mordvinE al" a , man, , man"; Sumerian: uru = " , Mann" (man).

3 / Oirāni: arayan->*alyam->mordvin lomasEger"; Sumerian: ur = " , Mensch, feindlich" (man, hostile). Note: uncertain, partial correspondence

„nőtestvér“; Sumerian: Ses = „Bruder, Schwester, Geschwister“ (brother, sister, siblings). Ancient Iranian: sarda- östjäkE sir , genus“, Hungarian s.; Sumerian: Sar, = „viel sein, Gesamtheit“ (to be many, multitude); and Ser, = „binden, Band, Bann“ (to bind, bond, commanding or prohibiting power; cf. s.).

19/ 𐎠𐎼𐎷𐎡𐎴: vadu-> cheremisz fate, wife, woman" Sumerian: no correspondence,
0.1 𐎠𐎼𐎷𐎡𐎴: vantd->vuguk Bodnt, wife"; Sumerian: banda= "Stütze" (support). Note: metaphorical correspondence.
21./ Old Iranian: yyyyy (stem vantr-) LappK vintim, suitor, groom"; Sumerian: banda Note: "Stütze" (support)
metaphorical correspondence.

92.4 Old Iranian: varta-, vartā-> MordvinE vardo "(slave)servant, (slave)servant- woman", divided into Ort , servant"; Sumerian: arad, arda= "Sklave" (slave); cf. also Akkadian: Warduz "Sklave" (slave).

23. Old Iranian: varya-> Finnish orja , slave", MordvinE ure , slave, wage earner"; Sumerian: arad, arda = "Sklave" (slave).
24/ Old Iranian: vasta, vasta -> mordvinE vasta , husband, wife"; Sumerian: mas = "Zwilling, Hälfte, der Anderer" (twin, half, the 2 , wife"; hill).
25./ Osirani: vasa-, vasa-> mordvinM vase mäšik). Sumerian: mas = "Zwilling, Hälfte, der Anderer" (twin, half, the

26.1 Öiräni: "vdra > votjāk var "(slave)", ziiřjēn ver "(slave)"; Sumerian: arad, arda=„Sklave" (slave).
27.1 Öirāni : "vidavadi -> Hungarian widow; Sumerian: no correspondence
28./ Öirāni: zana-> zürjēn zon "young man, child, boy"; Sumerian: no correspondence.
Of the 28 Old Iranian/Öirāni — Finno-Ugric (Hungarian) word correspondences listed by Harmatta, which Harmatta naturally records as loanwords of Iranian origin in the Finno-Ugric languages and in Hungarian without exception, 16 words, i.e. 57.1% of the words, have completely regular phonetic and semantic correspondences in Sumerian linguistic monuments, another 5 words (17.9%) either have only partial phonetic parallels in Sumerian, or the correspondence in meaning can only be demonstrated in a figurative sense, while the remaining 7 words (25%) have no parallels in the Sumerian vocabulary known today.
In other words, in Hungarian, 57.1% of Harmatta's 28 ösirāni/öirāni -Finno-Ugric (Hungarian) word parallels are quite obviously Sumerian loanwords (we could also call them "migratory words") in both the Iranian and Finno-Ugric (Hungarian) languages, while in the case of a further 17.9% of the words, the Sumerian origin is uncertain, but the geographical area in question is 1001

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Given the historical and linguistic relations of the Sumerian language, which can be deduced from the archaeological findings available to us today, and the pre-Asian written records, the Sumerian derivation of these words is very likely. We must also take into account the important circumstance that we know only 6-7000 words from the Sumerian language (or we knew them at the time of the publication of A. Deimel's dictionary, from which work we took the Sumerian parallels for the above words, since no more complete Sumerian dictionary has been published since then), and despite this, the ratio of regular and uncertain correspondences is still extremely high, 57.1+17.9%, i.e. 75% in total.
We have found similar correspondence rates before, when we showed obvious Sumerian correspondences in 39 words (73.6%) out of 53 ancient Iranian Finno-Ugric (Hungarian) word parallels in Harmatta's study "Iranians and Finno-Ugric, Iranians and Hungarians" (pp. 553-558 of our book).
Since these extremely high percentages appear for the second time between words with parallel phonetic and meaning in the ancient Iranian (6Iranian) and Finno-Ugric (Hungarian) languages, as well as in the Sumerian language, and practically in both cases they show fairly constant values (73.6 90 and 7590, respectively), we are convinced that these correspondences cannot in any way be classified as simply Iranian loanwords in the Finno-Ugric languages and in Hungarian, as in Hungarian Finno-Ugric studies (and Iranian studies), but absolutely require some other, much more plausible historical or linguistic development explanation. It seems most likely that we evaluate these word correspondences in both the Iranian, Hungarian and Finno-Ugric languages as word borrowings of Sumerian origin, and that they are the result of an areal language equalization process, more specifically a former West Asian (and to a lesser extent East European) We consider it to be a linguistic remnant of a "lingua franca" in both Iranian, Hungarian and Finno-Ugric languages.

The applicability of this approach is acknowledged by Harmatta himself in his present study, when he writes that after the Second World War, the contradictions between the language family tree theory and the assumption of a single basic language became increasingly obvious, and the idea of areal language equalization came to the fore instead of the assumption of common descent,

Let us therefore examine more closely whether it is probable that such a former, areally balanced, spoken language existed in Western Asia (and Eastern Europe) and, if so, at what period and, more precisely, in which region. If we add to the ancient linguistic connections outlined above the archaeological evidence and anthropological data currently available to us, we should soon notice that

1002
===== PAGE 991 =====
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In Western Asia and the easternmost part of Europe, there once existed an era and a geographical area when and where the most important prerequisites for the emergence of a relatively homogeneous communicative language were provided. According to all the archaeological, prehistoric and anthropological data known and available today, this period and area was the era and area of the Andronovo archaeological culture and the subsequent Karasuk culture, between approximately 1800/1700 and 1200 BC (Andronov6), and between 1200 and 700 (Karasuk), and territorially the geographical area extending from the Altai to the Ural Mountains and the Volga, and from the southern border of the forest belt to the present-day Iranian-Soviet border region. The Andronovo archaeological culture was at that time the leading civilization of the northern half of Western Asia, with an exceptionally high level of culture, with agriculture, animal husbandry, and advanced bronze art of artistic quality, which was already produced for export. At the same time, the Andronovo civilization also had far-reaching long-distance trade connections. Moreover, the population was largely anthropologically uniform: throughout the entire area of the culture we find the same so-called "Andronovo human type", which K. Jettmar briefly described as follows: "a plump Europid with a broad face, a low forehead and a rather short head" (1. Jettmar et al. 1966). If we now recall the archaeologically well-traceable process of the emergence of the Andronovo culture, which we have already described (pp. 31-32 and 317-318 of our book), then it becomes quite clear that the area of the Andronovo culture was home to many of the first-rate major centers of the West Asian colonization of the Chalcolithic and Early Bronze Age, which originated from East Asia in the 2nd millennium BC. It was in this area that those who, in the 5th millennium BC, Since the 10th millennium, Proto-Asian peoples migrated north in several waves, first establishing the South Turkmen civilizations (Dzeitun, Anau, Namazga Tepe) and the early Transcaucasian culture (Kura-Araxes culture), then later migrating even further north, they formed the Kuban-Maykop culture, the late pit-grave culture and the Early Bronze Age cultures of the Del-Ural region (Balanovo, Abasevo, etc.) in Eastern Europe, and the Afanasievo culture in Asia. Finally, through the gradual expansion and continuous integration of the archaeological cultures just listed, and with the help of the colonial waves that continued to arrive from the south, especially from southern Turkmenistan, at the same time, the Andronovo culture was created.

Accordingly, from the circumstances of the formation of the Andronovo culture outlined above, it can be concluded with a very high degree of probability that in the area of this cultural circle, a whole series of successive waves of Pre-Asian settlers of several millennia met again, after independent, separate development lasting for a longer or shorter period of time. This was contributed by

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===== PAGE 992 =====
Moreover, in the Andronovo culture we must very likely count on the development of huge political organizations and large-scale tribal alliances, which — similar to the North American Iroquois or the African alliance of the Bantu — encompassed huge territories, in this case perhaps the entire steppe region from the Urals to the Yenisei - writes, for example, M. Grjasnow on page 94 of his work entitled "South Siberia". Thus, within the framework of the Andronovo culture, all the prerequisites for a convergent areal language leveling process working towards linguistic unification were indeed found in the northern half of Western Asia and the easternmost parts of Europe.
In summary: according to our approach, an areal language alliance-like structure developed in the areas of the Andronovo archaeological culture between approximately 1800/1700 and 1200 BC. formation, or more precisely, a common communicative language -- generally used in the region from Altai to the Volga. This "lingua franca" also existed during the first centuries of the Karasuk culture that followed the Andronovo civilization, since, according to archaeological observations, the Karasuk culture (between about 1200 and 700 BC) was an organic continuation of the Andronovo civilization, and did not represent the emergence of a new population or the spread of a new culture. The relatively rapid spread of the new cultural form over large areas of the steppe can be explained by economic changes and the accompanying changes in lifestyle: during this period, the transition from a settled, combined agricultural and animal husbandry economic form to a semi-nomadic, periodic migration-related economic system occurred (see above).
Grjasnow: Südsibirien 1970, p. 104) This unity lasted until the beginning of the 1st millennium BC. In the last centuries of the 2nd millennium BC, the settlements of the tribes with the Karasuk type of culture covered vast distances, at least from the Yenisei to the Tien Shan and the Aral Sea. However, at the beginning of the 1st millennium, a culture — coming from the Volga and Don regions — spread across the steppes of Kazakhstan and Central Asia (all this I. Grjasnow: Südsibirien 1970, p. 132).
The above-mentioned lingua franca thus existed, according to archaeological data, until the beginning of the 1st millennium BC, since the prerequisites outlined earlier were also present up to that time. Then, between about 1000 and 900, starting with the disintegration of the unity of the Karasuk culture, the loosening of relations and the aforementioned migrations from west to east led to the individual groups of the cultural and linguistic alliance in question going their separate ways again, linguistically speaking, i.e. their languages continued to develop in different directions.
In this way, according to our view, one of these earlier ethnic groups of the Andronovo or Karasuk culture could have participated in the later processes of the formation of the Indic and Iranian languages, while another group, by all indications, played a very important role in the consolidation of the Turkic languages.

===== PAGE 993 =====
In the 10th century, the further linguistic development of a third group led to the formation of the Proto-Naga language and the linguistic equalization of the Finno-Ugric languages.
We would like to emphasize here that the above scheme naturally only provides a very sketchy picture of the processes of language equalization and does not take into account such undoubtedly very important factors as, for example, that in the development of the Indus and Iranian languages, it was obviously not only the aforementioned Andronovian group that played a role, but also the Dravidian languages, the Elamite language, and finally - but not least - the Semitic dialects of the Assyro-Babylonians and then the Arameans, which in all probability very strongly influenced the final consolidation of the Indus and Iranian languages. Let us just think that, for example, the state administrative language of the Old Persian Empire was Aramaic. Of course, this complexity, which we have just glimpsed by briefly outlining the processes of development of the Indic and Iranian languages, applies equally to the consolidation processes of the other languages that we mentioned earlier as successors to the Andronovo "lingua franca".
If we consistently consider this theory of areal language equalization, then those inscriptions from the 2nd and 1st millennium BC in Eastern Europe, which Harmatta writes were written in Old Iranian and Old Iranian, become much more understandable. In the case of these inscriptions, there is obviously a language alliance
We are faced with a mixed language (or languages) in which (or in which) Iranian elements are also found, and which elements have been identified by Iranologists - including Harmatta - in the previous

In accordance with Indo-Germanic practice, they naturally absolutize and speak with noble simplicity of "Iranian" inscriptions. Just as, for example, Gamkrelidse and Iwanow simply and consistently spoke of "Indo-European" languages in the case of Hittite, Luwian and Palaic. However, as can be seen from Harmatta's own example in this regard 15 — he gives the reading of an inscription on a side piece of a bridle from the 6th century BC, found near Orenburg, on which, according to him, the word cyrezný can be read

—, These readings are also fabricated using the method originating from the 19th century, which consists in first completely arbitrarily and purposefully cutting the word to be interpreted into syllables that have equivalents somewhere in the such astonishing etymological monsters arise (such as Avesta text, and then identifying the syllables thus obtained - or, more precisely, syllables - with the meaning fragments of syllables with parallel phonetic forms taken from the Avesta. This is how "month with combined horses"), of which our readers are already familiar with a bunch (e.g., pp. 225-227 of our book).

1005
===== PAGE 994 =====

In our study, Harmatta explains the aforementioned cyrezný inscription according to this method as the possessive case of the Ōrāni Cīra-zana-personal name Cīra-zana-„fejeményénc”, in the sense of, "Cīra-zanée". So here, even with the greatest effort, Harmatta could not find Avestan parallels for the syllables of the inscription in question, so he resorted to the other well-known Indo-Germanic evasion and simply qualified the inscription as a personal name, and then even corrected the word part cyra to the phonetic form yyyyy, "and only after the manipulations should it be properly "adjusted" so as not to "shake the inscription in question in the pants".

"succeeded" in finding an Iranian etymology for the inscription. We can see that with such "methods" everything in the world can be "read" in Iranian, only previously

Harmatta then reports that it has recently been discovered that the Xiongnu Empire was preceded by a Yueh-chih Empire of similar size and power by nearly three centuries, extending far west to the Irtysh region. It has also been discovered, he writes, that the Yueh-chih language was an Eastern Iranian language similar to Sakha. The language of the Xiongnu also proved to be a Saka-type Eastern Iranian language. Then, following these new discoveries, Harmatta searches for the origin of the Turkish (and Ugric) cavalry terminology in the Yüeh-Chih-k language or in another Saka-type Eastern Iranian language. To support this assumption, he lists seven Turkic-Iranian-Ugric-Hungarian word parallels. However, it is very characteristic that of these seven words, which according to Harmatta's idea are intended to prove the Eastern Iranian linguistic origin of the Turkish (and Ugric-Hungarian) cavalry terminology, he is unable to present a truly existing old Eastern Iranian word correspondence that can be found in linguistic monuments, but in each case he only lists "starry" — i.e. linguistically reconstructed, deduced from modern Iranian languages on a "phonetic" basis — word forms as "proof" of the Iranian origin.

This fact, however, very likely indicates that something is wrong with the interpretation of the Yüeh-Chih language — and even more so the Xiongnu language — as an East Iranian language of the Sakas type. Our suspicion is especially strengthened by the fact that, in the decades following the Second World War, more and more researchers have been strongly questioning the 19th-century assumption that the Iranian language of various steppe and other West Asian peoples (Scythians, Sarmatians, Sakas, Medes, Parthians, Kushans, etc.) originated in Iran. We have already presented some of the relevant literature data to our readers in our previous volumes (1, for example, our book, pp. 57-59, 82-85, 224-236 and 262-265).

Particularly noteworthy in this regard is the position of K. Jettmar and his co-authors in the essential handbook "Geschichte Mitteleuropas" (Handbuch der Orientalistik, 1966), according to which the thesis of the alleged Iranian languages of the Persian fringes has very shaky foundations.

===== PAGE 995 =====
kon 811. On the one hand, the written record interpreted as some of the Iranian languages (yyl yl, in connection with what was said above in connection with Harmatta's interpretation of the Tyrozný inscription as Iranian) is not yet an ethnic or linguistic bi

zoryntek, as e.g. the Latin language of the 12th century European chronicles is not either. On the other hand, Arab sources repeatedly call the Khorezmian, Sogdian, Saka, Bactrian and Fergana languages Turkic languages (i.e. also the Saka language, one of the dialects of which was spoken by the Yüeh-chih and the Hsiung-nu, according to Harmatta). Likewise, the Iranian-ness of the Scythians and Sarmatians is certain, Jettmar continues. Almost the only "evidence" that is constantly referred to is the Europid skulls in the grave finds. In connection with this latter remark, however, we must absolutely state that the anthropological category "Europid" cannot be identified at all with the peoples speaking "Indo-European" languages, because anthropologically, the Caucasians, as well as the majority of the Turks and Finno-Ugric peoples, but also the East Asian and North African races (Semites, Hamites). It is therefore a false argument to tacitly equate the anthropological concept of "Europid" with the Indo-European peoples, as we can register this step by step in earlier Indo-Germanic studies.

It is also very important in this context that the oldest texts of the Avesta, the so-called "Gathas", are practically not understood by Iranologists even today, because in the words of F. Altheim: "Satzbau und Ausdrucksweise, Wortstellung und Wortwahl, eine hohe Altertümlichkeit der Sprache und des Denkens setzen dem Verständnis Schranken." In Hungarian: "The sentence structure and the way of expression, the word order and the choice of words, the high antiquity of the language and the way of thinking place barriers to understanding." (1. Altheim-Stiehl 1970. p. 63) If we add to this the fact — which is also clearly stated in Altheim-Stiehl's work just quoted — that primarily the Middle Iranian language, but also partly the Avesta itself, form an almost inextricable linguistic web with the ancient Turks, then the

In its entirety, we have before us the deplorable methodological state of Iranology, contrary to all the loud propaganda to the contrary. But in addition, our above-mentioned arealistic idea, according to which the ancestors of the Iranian and Turkic languages more than likely once belonged to the same areal language alliance within the framework of the Andronovo "lingua franca", also receives new support from a completely different side.

If we take all these accompanying circumstances into account, then we can only take the position that the Iranian-ness of the Yüeh-chih language, but even more so of the Hsiung-nu language, was probably established by the same methods with which the so-called "ahistorical" school of Indo-Germanic studies already established the language of the Japanese Ainu.

succeeded in "Indo-Germanizing" it.

===== PAGE 996 =====
As a result of this situation, we ourselves take a cautious wait-and-see stance on this issue until more reliable data are available for research to definitively determine the linguistic affiliation of the Yüeh-chih and the Hsiung-nu, rather than the opinion of some Iranologists that the languages of these two peoples are East Iranian languages. For now, it only seems likely that these languages may have been in some way related to the successor languages of the areally equalized Communicative Language of the Andronovo and Karasuk civilizations that developed in various directions (1. above), and in this case it is also conceivable that they may originally have been some kind of Turkic-Iranian mixed language.

Harmatta further claims that Turkic ethnic elements first appeared in Europe as part of the Hunnic migration in the second half of the 4th century AD, and the next Turkic ethnic wave reached Europe around 463 with the migration of the Saragur, Ogur and Onogur. However, we cannot agree with any of these data, because they do not correspond to the known sources of the time. On the one hand, Ptolemy, the world-famous Alexandrian geographer, mentions Huns north of the Caucasus as early as the middle of the 2nd century AD, and he also calls the Ural River by its Turkish name, Dzayz (Yajk). Our readers are already familiar with this data (book 1, p. 260). On the other hand, F. Altheim states in volume I of his work "Geschichte der Hunnen" that Huns already lived among the mountains of the Caucasus in the 2nd century AD (1. Altheim 1959-1962. Vol. I, p. 9). They invaded Persia at the beginning of the Sassanid period, during the reign of the Armenian king Chosrov (222-238), who was from the Parthian Arsacid dynasty. Agathangelos reports on this invasion (1. Altheim 1958-1962. Vol. I, p. 12). We know of another very early Persian Hun invasion from Priscus, who writes that the Huns invaded Persia long before Attila, at a time when Rome was still the capital of the empire (see Altheim 1958-1962. Vol. I, p. 12-13). In other words, according to Priscus, this invasion took place before the founding of Constantinople, i.e. at least 50 years earlier than the well-known conquest of Eastern Europe by the Huns around 375. In addition, the fact that the Huns, after crossing the Don around 375, encountered five closely related tribes of Hun origin west of the Don also very likely indicates a presence of the Huns in Europe much earlier than 375 (see Altheim 1958-1962. Vol. I, pp. 8-9).

As for the alleged immigration of the Saragurs, Ogurs and Onogurs to Europe around 463, this hypothesis, which has been repeated over and over again in Hungarian historical research with a zeal worthy of a better cause for many decades, is based on a misunderstood or misinterpreted — whether consciously or unconsciously, let us not ask now — data from Priscus. Priscus reports that around 463 AD the Saragurs were conquered by the Urogs.

1008
===== PAGE 997 =====
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(probably Ugrians, Ogurs) were expelled from their homes, the Ugrians were pressed by the Onogurs, who were supported by the Sabirs, while the latter were attacked by the Avars, and the Avars were driven from their former homeland by the peoples living in the ocean region: From this notice of Priscus, Bálint Hóman and Gyula Moravcsik drew the conclusion that the peoples living in the ocean region obviously meant some Mongol-Tunguz ethnic groups, while the Avars, according to the unanimous opinion of the research at that time, were identified with the Zhuang-Zhuangs of Inner Asia. According to the understanding at that time, these Zhuang-Zhuang Avars would have lived in the Altai region and to the east of it. Based on this definition, the Sabirs were then placed west of the Zhuang-Zhuangs, in the Ob-Irtysh region in Western Siberia, while the Onogurs, Urogs (Ugrians) and Saragurs were again localized somewhat further west, roughly in the region of the Ural Mountains and the Ural River. Thus, according to the hypothesis of Hóman and Moravcsik, the entire migration chain would have started from Inner Asia and led through Western Siberia and the southern part of the Ural Mountains to Europe, to the Kuban-Caucasus region. This assumption is undoubtedly based on the idea of Finno-Ugric linguistics that the Hungarians are descended not only linguistically but also ethnically from the Finno-Ugric peoples, more specifically from the Voguls and the Ostyaks - the so-called Ob-Ugrians - i.e., as a result, the ancestral homeland of the Hungarians could only have been in the regions inhabited by the Finno-Ugrians, somewhere in the Ural Mountains. However, since the earliest sources mentioning the Hungarians, or most likely referring to the Hungarians, place our ancestors without exception in Southern Russia, in the Kuban, Don and Black Sea regions, the sharp contradiction between the generally accepted assumption of Finno-Ugric linguistics and the geographical data of historical sources had to be explained in some way. The hypothesis of Hóman and Moravcsik, however, was excellently suited to eliminate this contradiction, since with this interpretation of Priscus's statement, accompanied by the Onogurs or Ugrians, or even possibly hidden under the name of one of these two peoples (let's think: our national name used abroad comes from the name Ugrian or Onogur), around 463 the ancestors of the Hungarians could be conveniently, almost self-evidently, relocated from their ancestral homeland in the Urals, as required by the Finno-Ugric theory, to the Kuban-Don-Pontus region, where the Hungarians first appeared in contemporary written sources. All this would have been fine up to this point, if we had not already had the data at our disposal when the hypothesis was first developed.

There are also contemporary and nearly contemporary sources that report that this entire people movement described by Priscus: Galom started from the eastern Iranian region, the Sogdia-Bactria region, from the Hephthalite or White Hunnic empire there, and during the migrations the Avars

===== PAGE 998 =====

From eastern Iran they migrated west along the southern coast of the Caspian Sea, approximately to the area of present-day Azerbaijan, while the Sabirs, who, according to several ancient sources (e.g. Ammianus Marcellinus) had already lived in the area of Dagestan before the start of the great Hunnic migration — that is, before 375 AD — under pressure from the Avars, migrated further north along the western coast of the Caspian Sea, pushing the Onogurs ahead of them, who eventually settled in the Kuban region, and to the north of them, approximately in the region of the Maeotis and the Donals6 section, the Urogs (Ogurs) and the Saragurs found new habitation.

This Sabir-Onogur-Urog (Ogur)-Saragur migration, which we have just outlined, from south to north, can be proven by numerous contemporary sources (1. Haussig: Theophylakt's Exkurs über die skythischen Völker, described in more detail on pp. 256-259 of our book).

This southern migration route is also proven by Armenian sources (e.g. Moses of Chorene), which already in the Parthian period - before 226 AD - repeatedly mention the Ogurs and even the Bulgars in the northern foreland of IsaKa- | ukázus, in the neighborhood of Armenia. Ammianus Marcellinus also reports in the middle of the 4th century AD - as we have already mentioned - that the Sabirs were already settled in the vicinity of Dagestan before the migration of the Huns, which began around 375. Jordanes, in the 6th century, writes that in Attila's empire the Onogurs (or Hun-Ogurs) lived west of the Sea of Azov, as well as in Dacia, Moesia and Thrace (i.e. in the eastern parts of the Balkan Peninsula; Dacia at this time meant the Roman province called "Dacia ripensis", which lay on the southern bank of the Danube, in the northern part of present-day Bulgaria), and after the collapse of the Hunnic empire they moved further east again.

So we can take any of the ethnic names: 5280116, Ogurt, or Onogurt, or even Bolgárt; all of them can be proven by contemporary sources to have lived in Europe well before 463. =

In addition to all these source data, there is also a very significant realization that speaks against the hypothesis of Hóman and Moravcsik. In the meantime, it has become clear that the European Avars cannot be identified with the Zhuangzhuang of Inner Asia.

«jýýýýj] — as previous research suggested, they —, but around the Var and Chun tribes of the Hephthalite-Hun empire in the eastern Iranian-Turkmenistan-Bactria-Sogdia region as the leader were composed of ethnic groups clustered around tribes. (1. Haussig 1953, described in our book, pp. 256-259.)

Then, Harmatta uses the term "Hepta geneal" (seven tribes) in the Byzantine Theophanes Asparuch's report on the Bulgarian conquest of the Danube, which, as can be clearly established from the context, refers to a group of seven tribes of the Eastern Balkan Slavs, with the help of rather convoluted and wordy "textual criticism" and arbitrary textual additions, to refer to the "seven Hungarians" tribal confederation.

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The text in question - even in Harmatta's own translation is the following: "...having brought under their rule the Hepta geneai ("Seven Tribe") of the surrounding Slavic tribes, the Severians, from the beginning of the Beregaba Strait to the east, 656 parts, and to the south and west 1016 6586 (parts) up to Abaria, they settled the rest, the Hepta geneai, who were in contractual relations (with them)." - That is, we repeat - it is clear from the text that we are talking about Slavic tribes, not Hungarians. In the following, Harmatta - obviously to support his "seven Hungarian" hypothesis - shows on a needle holder found in the Late Avar cemetery in Szarvas, dating from around 750 "Hepta geneai"

interprets a part of the runic text in Hungarian as follows: "I weave a rug. Father Karvaly made it." According to Harmatta, this Hungarian text passage would prove the presence of the "Hepta geneai" Hungarians in the Szarvas area at the end of the Bulgar-Avar period in the 8th century.

We are not yet in a position to decide whether Harmatta's Hungarian reading of a part of the inscription on the Szarvas needle holder is correct or not. We can only note that, as far as we know, carpets were not often woven with needles. In other words, Harmatta's interpretation resulted in a rather meaningless text. However, a much more important thing in this context is the fact that Harmatta is clearly coming very close to accepting Gyula László's thesis of "double conquest", while the vast majority of representatives of academic Hungarian scholarship have — in sharp contrast to this — roundly rejected Gyula László's theory in recent years. Harmatta's reconciliation of "Hepta geneai" = "seven Hungarians" is in fact nothing other than the most important, one might say decisive, theory of "double conquest".

recognition of the moment that, roughly simultaneously with the conquest of the Danubian Bulgarians by Asparuch, around 680, the so-called "Griffes-Indás" archaeological group, a Hungarian-speaking population also settled in the Carpathian Basin,

Harmatt's explanations of the antecedents and course of the conquest testify to a very significant change in perspective. 15 According to him, the Bulgars and Alans of the Donets region, i.e. the population of the Mayak culture in the Donets region, were the so-called "Kabars", who rebelled against the Khazars around 815 and joined the Hungarians.

This greatly increased the power of the Hungarian tribal alliance, extending its power in the northeast to the Volga, becoming independent from the Danubian Bulgars (cf. above with the agreement "Hepta geneai" = "Heimagyar") and soon after » 862 - it also launched its western campaigns. After that, according to Arab-Persian sources, the Hungarian tribal alliance was independent until around 880. (According to the Byzantine emperor Leo the Wise, not only until 880, but also in 895 - our note), and only after this period did it enter 1011

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political relations with the Khazars — probably due to the advance of the Pechenegs. However, around 889 the Pechenegs crossed the Volga somewhere near Saratov and wedged themselves into the territory of the Hungarian tribal alliance. At this time, according to Harmatta, parts of some tribes (Magyar, Jeny, Gyarmat) broke away from the bulk of the Hungarians and remained near the Volga.

Harmatta then explains that the idea of conquering the Carpathian Basin may have matured among the Hungarian chieftains in the early 890s. He then writes: "The right moment came in 895, when war broke out between the Bulgarians and Byzantium and the Byzantines asked the Hungarians for military assistance.

The Hungarian operational plan developed for the occupation of the Carpathian Basin was based on accurate knowledge of the political and military situation and the balance of power. No more than 1/5 of the Hungarian army — the Kabar troops, about 3-4,000 cavalry warriors — under the command of Árpád's son, Liüntiká, crossed the Lower Danube with the help of the Byzantine fleet and attacked the center of the Bulgarian state. However, this was only a pretended attack, and served only the strategic purpose of preventing the Bulgarians from defending their territories in the Carpathian Basin east of the Danube. At the same time, the Hungarian main forces — an army of 16-17,000 cavalry — under the leadership of Prince Árpád, with the entire population and livestock of the tribes, penetrated the Carpathian Basin via the Verecke Strait, along the route previously used by the Hungarian troops on their western campaigns, and occupied it in several operations.

After three victorious battles, Liüntika withdrew from Bulgarian territory and began to invade Transylvania with his troops. At that time, Simeon, the Bulgarian ruler, who needed some military success to strengthen his negotiating position against the Byzantines, with the help of the Pechenegs, attacked the remaining Kabar people and the rearguard protecting their camps, and ravaged the Kabar camps, which had been largely emptied, and then hastily retreated with the Pechenegs. This Bulgarian-Pecheneg raid, the success of which Simeon greatly exaggerated during the negotiations with the Byzantines for understandable reasons, had no strategic significance from the point of view of the Hungarian conquest." — So far Harmatta on the circumstances and course of the conquest.

We must note with considerable astonishment that Harmatta's description quoted above corresponds in every way to the picture of the conquest that can be logically deduced based on the data of the available contemporary sources, which we ourselves explained in detail eleven years ago, while discussing the views of István Fodor and, more precisely, György Györffy (pp. 352-361 of our first book). It is almost unbelievable, but it is a fact that Harmatta finally got off the "Pechersenian run" that he had been chasing for more than a century, and he is even willing to admit that the conquering Hungarians understood something about it.

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for military leadership and they did not attack Bulgaria's central Balkan territories (according to Györffy and Fodor in 894) and Carpathian Basin provinces (according to Györffy and Fodor in 895) separately, but the two campaigns took place simultaneously, in 895.

Finally, Harmatta explains the much-discussed but still unresolved question of the origin of the Székelys, referring to the evidence of the runic inscription on the Szarvas needle holder, which he interpreted partly as Hungarian, by saying that the partly Hungarian and partly Turkish text is evidence that an ethnic group consisting of Hungarian and Turkish elements had settled in the Szarvas area sometime between the Bulgarian conquest (679-681) and the time of the needle holder (around 750). The Turkish element among them, according to Harmatta, was represented by a fragment that broke away from the western Turkic ethnic group called Eskil (or Esikil). This idea is also significantly supported by the fact that, according to Chinese sources, the ethnonym Eskil also occurred in the Sikil variant around 660 AD. The ethnonym Székely derives from the name of this Turkic ethnic group, writes Harmatta.

This Székely origin hypothesis also represents a very significant advance against the opinion that has been academically "sanctified" in recent decades, according to which the Székelys' stay in the Carpathian Basin before the conquest is only a fabulous report of Hungarian chronicles, which has no historical basis whatsoever.

linguistics The study "Should Finnish Prehistory be Rewritten?", which is the next paper in the 1990/3 issue of "Magyar Tudomány", reports on the latest, most important developments and debates in Finland regarding Finno-Ugric and prehistoric research, more precisely the Ural-Finno-Ugric people and language family hypothesis. We learn that in Finland, fundamentally new prehistoric and linguistic theories have emerged in the last ten years, according to which the Finns have lived in their present-day places for 9,000 years - some say only about 5,200 years. Research into Finno-Germanic linguistic relations has also led to astonishing results. Given the large number of loanwords of Germanic origin in the Finnish language, the question inevitably arises: "...are the Finns Germanized Finno-Ugric or Finno-Ugric Germans?" writes Enikő Szij, and then explains that the above chronological problem can only be solved in two ways: 1./ By moving the traditional language family chronology 2000 years earlier, which, however, seems impossible for several reasons. (We must fully agree with Enikő Szij in this, since the current language family chronology is already extremely overstretched and has no basis in linguistic history.) 2./ Or we must revive the old, so-called great ancestral homeland theory, which would have extended from the Urals to Scandinavia. — This idea, however, is extremely outdated in the context of the fishing-hunting economic stage.

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It fails on the grounds of the extremely low population density, which cannot be ignored with any excuse, which makes even the hypothesis of a Ural-Finno-Ugric ancestral homeland in the Oka-Kama-Byclajia region, localized to a much smaller area, impossible, let alone the idea of an ancestral homeland extending from the Urals to Scandinavia. — However, Enikő Szij prefers not to talk about this exclusionary reason.

The attentive reader can therefore draw the only conceivable conclusion: under such circumstances, only the areal language equalization theory of the relationships between Finno-Ugric languages can claim to be seriously considered as a realistic explanation of the nature of the linguistic affinities and connections between Finno-Ugric languages.

Then Enikő Szij reports that there are currently several different Finnish prehistory theories in circulation, which all refer to the same archaeological works. She sees the reason for this as: "...the practitioners of certain fields do not formulate their arguments clearly, precisely, in a way that is understandable and unambiguous for specialists in other sciences." (Emphasis by Enikő Szij). However, this explanation is obviously nothing more than a cheap excuse. Our attentive readers can probably recognize the real reason for the phenomenon outlined by Enikő Szij earlier. The simultaneous existence of several different prehistory theories referring to and relying on the same archaeological results

Its existence can hardly be explained as if archaeologists were unable to formulate it accurately, but rather can be traced back to the fact that the results of archaeology — as we have repeatedly shown in our previous volumes — both individually and as a whole clearly speak against the idea of the genetic, i.e., lineage-related relationship of the Ural-Finno-Ugric language family. It is therefore evident that Finno-Ugric linguists and paleo-historians who nevertheless wish to produce a genetically interpreted Finno-Ugric prehistory from these archaeological data — which without exception refute the genetic Finno-Ugric language relationship — are forced to interpret the archaeological finds in question completely arbitrarily, in a way that somehow corresponds to their own ideas of language development and prehistory.

In other words, each person, from a different perspective, collects excavation observations that, in their opinion, can somehow be used to support the former existence of the Ural-Finno-Ugric language family, which is interpreted as being genetically related. We have had occasion to point out this "method" — or rather, lack of method — in several cases, not in relation to Finnish, but also in relation to Hungarian Finno-Ugric studies, in the course of our studies so far.

At the end of her article, Enikő Szij again emphatically emphasizes that whether it is a question of 9,000 or 5,200 years of independent Finnish history, the Finno-Ugric
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In this case, kinship can only be explained by returning to the so-called great dshaza theory. This stubborn adherence to the theory of a genetically related Ural-Finno-Ugric people and language is a direct result of the results of archaeological research over the last half century that have clearly refuted even the possibility of the former existence of a genetically related Ural-Finno-Ugric people and language.

After the above, Enikő Szij concludes by emphasizing for the third time that the main goal of future research should be to review and rethink the great ancestral homeland theory that offers a solution in the light of the latest knowledge.

After this, it seems to the unbiased observer that the prehistoric horizon of the Hungarian Őstörténetkutatás, working in the wake of Finno-Ugric linguistics, has shrunk to the narrow cross-section of its so-called "tube vision" field of vision, and its approach has become so rigid that, apart from the old 19th century, long-outdated linguistic beliefs, it is completely incapable of utilizing or incorporating any new archaeological, prehistoric, linguistic or linguistic-historical data or ideas into a new, more modern Finno-Ugric linguistic-historical theory.

A The following article from the 1990/3 issue of "Magyar Tudomány" was written by Lóránd Benkő. Its title is: "Our common history and Hungarian linguistics." Since Benkő is in his thesis, he mainly focuses on the article "Hungarian linguistic history — Hungarian ancestral history" published in the volume "Hungarian 65- historical studies" that the Hungarian people developed many independent linguistic characteristics, no longer shared with other related languages, in their so-called "separate life" of about 1500-2000 years up to the conquest of the country, and that for this reason, the historical events of this period should be approached and explained from a linguistic perspective — if at all possible — only from the perspective of the Hungarian language.

We have already discussed this 1977 study by Benkő in more detail on pages 524-532 of our book. We have shown there that although Benkő correctly assesses a methodological issue that is not very worth considering in theory — but only in theory — he disregards all methodically in the practical application of these theoretical requirements that he himself has laid down, and even thinks that he can etymologize Hungarian words from Finno-Ugric languages that have no parallel in any Finno-Ugric language. — For this reason, it is not necessary to discuss this latest article by Benkő in more detail, and so we will only mention one example, which he refers to in connection with the fact that the unacceptable and unmethodical habit has become very prevalent in related disciplines, that Hungarian linguistics has already

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His obsolete, no longer valid word or name derivations are very often used to support historical hypotheses. He literally writes: "The ethnonym Eszegel cannot be the predecessor of the Hungarian Székely." — Well, just a few pages earlier we presented János Harmatta's hypothesis on the origin of the Székelys, in which he precisely states that the name Székely derives from the name eszegel, or rather eskil or eskil, which he refers to as etymological support that the Székely phonetic version of the name eskil can be found in Chinese sources as early as around 660 AD. However, Professor Harmatta is not a practitioner of one of the "társtudományok", but is in the order and manner of a professionally trained linguist — although not explicitly a Finno-Ugricist, but an Iranologist, whose competence in Finno-Ugric and Hungarian linguistic issues is unreservedly recognized by Hungarian academic scholarship. This is eloquently proven by the fact that the most important keynote speech, summarizing the entire topic, at the public Hungarian prehistory meeting of the Academy's Department of Linguistics and Literary Studies on May 12, 1989 was delivered by János Harmatta (1. above).

We do not wish to take a definitive position on this particularly unique linguistic issue, but we dare to note that it is quite difficult to see what linguistic obstacle there would be to identifying the written name Szekler in Chinese sources with the Hungarian Székely ethnonym. Thus, this linguistic hair-splitting by Benkő is eerily reminiscent of another Hungarian linguistic absurdity, according to which the names of Hungarians used abroad, Ungar, Hungar, Ugri, Vengri, etc., are derived from the ethnonym Onogor, according to our linguists, but at the same time they allegedly have nothing to do with the name of the Huns, although contemporary sources often use the ethnonym Onogor in both the Ungur and Hun-Ugur phonetic forms to name the same people, and even explicitly add several times that in the case of this people we are talking about a Hun-descendant ethnic group (more on this in our 1st book, p. 273).

Lajos Vargyas: This His lecture entitled "The Pre-Conquest Strata of Hungarian Folk Poetry and Folk Music" is included in the following article in the 1990/3 issue of Magyar Tudomány. paper is particularly characteristic of the current deplorable state of Hungarian studies. In his presentation, Vargyas flatly states that our most ancient musical traditions are shared with the Ob-Ugric people; these are the two-, or possibly three- or four-tone lamentations. - In sharp contrast to this, in the volume "Magyar Őstörténeti tanulmányok" published in 1977, which contains the lectures delivered at the prehistoric symposium held in November 1973, in the study "László Vikár and Finno-Ugric Relations", the questionable Vargyas-like Ob-Ugric-Hungarian lamentation-pair-1016 Hungarian folk music is Turkish

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He showed categorically in relation to the huzams that they cannot be used to prove any kind of relationship. Literally: "We also know melodic motifs moving on three, four or five adjacent notes from many other parts of the world; they could have been created independently of each other, because they are simple and natural expressions of the human soul. They cannot be appropriated for any culture or language family." - These lines are completely clear and irrefutable evidence that it is methodologically impermissible, and even downright unscientific, to conclude from the parallels of the very primitive Obi-Ugric and Hungarian lament melodies moving on only two, three or four adjacent notes that these correspondences testify to the folk kinship, perhaps even a common origin, of the Obi-Ugric and Hungarians.

However, what is most shocking in this context is that the authorities of the Hungarian Academy of Sciences selected as the lecturer on ethnography and folk music at the 1989 prehistoric meeting — despite Vikár's completely convincing and indisputable refutation presented earlier — precisely the same Lajos Vargyas, who is well known to have been tinkering with these Obi-Ugric-Hungarian lamentation parallels for more than three decades, which he believes prove a common origin. With the inclusion of László Vikár at the 1973 prehistoric symposium, it was obvious that "a mistake had slipped into the arrangement", and apparently to eliminate this afterwards, Lajos Vargyas was dragged forward to give the lecture on ethnography and folk music at the 1989 prehistoric session, who then lived up to expectations: he beautifully recited the lesson on the lament melody couplets that, in his opinion, proved the Vogul-Hungarian folk kinship and whatnot.

István Fodor gave a lecture entitled "On the Half-century History of Archaeological Research" at the academic class meeting on May 12, 1989. His arguments are worth briefly discussing only because they are textbook examples of how Hungarian prehistorians and archaeologists, educated on the foundations of Finno-Ugric linguistics, are apparently simply unable to break out of the magical circle of 19th-century hypotheses of Finno-Ugric studies. In his lecture, Fodor says nothing new, but simply repeats the same highly improbable, and in more than one case even downright absurd, ideas that we have already described in more detail in Chapter 3 of our book "In Double Measure..." during the discussion of his book "On the Famous Path of Verecke...". We have repeatedly shown there that Fodor's views are generally not at all compatible with the actual results of archaeological research.

For example, Fodor still in 1989 consistently supports Péter Hajdú's hypothesis of a Uralic ancestral homeland along the Lower Ob, which, however, has no archaeological support whatsoever, but is merely Hajdú's evidence.

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It is based on an impossible, purely linguistic speculation, according to which: a/ the Uralic root language would have had the names of the characteristic trees of the Siberian taiga forest; b/ the Uralic root language would also have had the names of the trees of the European deciduous forest; c/ over time, the Western Finno-Ugric peoples would have forgotten the names of the trees of the taiga forest; d/ at the same time, the Eastern Finno-Ugric peoples would have forgotten the names of the trees of the European deciduous forest.
Well, Fodor — evidently feeling himself the very high improbability of this hypothesis pyramid built on "sound-law" linguistic reconstructions, which is moving in a purely empty space (since the Uralic-Finno-Ugric languages have practically no linguistic monuments, so the linguistic deductions attempted by linguistics to the age of the Uralic base language, i.e., about 7000-8000 years, have no tangible, concrete linguistic monument basis), — attempted to find an archaeological foundation for Hajdú's hypothesis of a Uralic ancestral homeland along the Lower Ob in his book "On the Famous Path of Verecke...". In this effort — as we have already explained in Chapter 3 of "With Double Standard..." — he did not hesitate to date the beginnings of the Paleolithic-Mesolithic fishing-hunting settlements in the Eastern Urals excavated by Chernyvetsov — in the finds of which strong Keteminiarian cultural influences can be detected — to around 4000 BC, although according to the chronological definition of international archaeology, the earliest site of the Keteminiarian culture — settlement No. 4 of Dzambas Kala — dates back to ca.

It dates from 2700 BC (1. László 1961. p. 105). In other words, the Early Middle Ages culture began 1300 years later than the earliest Early Middle Ages cultural radiations in the Eastern Ural region, dated by Fodor to around 4000 BC. In other words, these Eastern Ural settlements, together with their Early Middle Ages parallels, did not begin around 4000 BC, but belong to ca. 2500-2000 (more on this question |. our book, pp. 310-313 and 314-315). However, in this entire problem area of Hajdú-Fodor's Lower Ob tundra Uralic ancestral homeland hypothesis, the most characteristic thing is that János Harmatta, an Iranologist linguist with decades of research experience in Finno-Ugric studies, wrote in his above-mentioned lecture, which was delivered at the same academic prehistory class meeting on May 12, 1989, as Fodor's lecture just discussed, that the name of the stone pine can only be found in the Permian, Ob-Ugric and Samoyedic languages, i.e. in the actual distribution area of the tree, and there is no evidence that it existed in other Finno-Ugric languages.

We do not need to deal with the works of Pál Lipták: "Anthropological Problems of the Prehistory of the Hungarians", Ivan Balassa: "Ethnographic Possibilities of Hungarian Prehistory ... "Eastern Yield" — Research", and Attila Paládi Kovács: or Zootechnics in the Old Hungarian Era", because
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There is nothing essentially new in them that our readers would not already know from our previous poems.
However, we must definitely refer to György Györffy's study entitled "On the Issues of the Hungarian Conquest", which forms the next article in the 1990/3 issue of "Magyar Tudomány", for similar reasons as those for which we dealt with István Fodor's paper 18, since Györffy - also in direct contradiction to the position of János Harmatta's paper described above - consistently claims that Levente's Bulgarian campaign took place in 894 and was independent of Árpád's campaign of conquest in 895. He also continues to hold the view that the Pecheneg attack was catastrophic in scale, and he essentially evaluates the conquest as a desperate escape from the Pechenegs.

We have already spoken at length about this question, also in Chapter 3 of our volume entitled "In Double Measure...", where we reviewed Fodor's book "On the Famous Road of Vereckc..." — and that is because in this book Fodor fully embraced Györffy's ideas about the conquest. Thus, the criticism of Fodor's view of the conquest actually also means a criticism of Györffy's completely identical views on the conquest.
István Erdélyi's lecture entitled "Research on the Archaeological Monuments of the Pre-Conquest Hungarians in the Soviet Union" concludes the series of articles in the 1990/3 issue of "Magyar Tudomány" describing the Academy's prehistory meeting of May 12, 1989. Erdélyi only deals with the finds of the so-called "Magna Hungaria", i.e. the region of the Volga-Bjelaja-Ufa rivers, which are most likely related to Hungarians, in his study, and he is more inclined towards the view that the archaeological finds of the "Magna Hungaria" region that can be associated with the Hungarians, i.e. the archaeological remains of the Hungarians found along the Volga by Brother Julianus in the first half of the 13th century, come from a Hungarian group that separated from the majority of the Hungarians in the 7th-8th centuries AD, rather than from a section of the Hungarian people that remained in their ancestral homeland. — We can agree with István Erdélyi's view in everything.

Summarizing what was said at the academic prehistory class meeting on May 12, 1989, we can state that the most significant, and one could even say epoch-making, novelty was Enikő Szijs's presentation on the enormous changes in perspective that have occurred in Finland in the research of the prehistory of the Finnish people in the last decade, the essence of which could perhaps be most characteristically summarized as the Finns' withdrawal from the Ural-Finno-Ugric homeland.
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In addition, János Harmatta's presentation also contained one or two very important new aspects and insights, among which his statement that Uralistics has recently come to the conclusion that the formation of the Finno-Ugric basic language "took place in the process of Aredlian contacts, because the distribution of typological features clearly points to four or five separate groups, 5 the opposing peripheral areas do not have preserved identical ancient features."
Of course, we cannot know — at least not yet — whether these lines of Professor Harmatta do not merely constitute the same "alibi statement" as, for example, We could find it in his book "Introduction to Uralic Linguistics", where Hajdú talked about how the Uralic-Finno-Ugric published as early as 1966, "...developed near the centers of development of several old Eurasian languages and language families" and it is probable "...the assumption that, in addition to the archaeological ones, linguistic traces also point to the southern connections of the Pre-Uralians." (1. Hajdú 1966. pp. 9-10.) Later he stated that "...It is highly probable that the Indo-Germanic, Semitic, Hamitic, Ural-Altaic, Chukchi and Kartvel language families have a very old genetic connection" (1. Hajdú 1966. p. 92.). — However, in the practice of Hungarian linguistic research over the past quarter of a century since 1960, there is no sign in the world that anyone has seriously addressed the issues of the southern origin and southern linguistic connections of the Ural-Finno-Ugric languages. Péter Hajdú himself did nothing in this area, and has even repeatedly spoken out vehemently against the investigation of any southern linguistic connection of the Finno-Ugric languages. For example, he called even the mere raising of the question of the possible Sumerian connections of the Hungarian language "emigration psychosis", "sensationalism", "pseudo-scientific fiction" and the like (1. e.g. Hajdú: "They found a new homeland, they are looking for their ancestral homeland" 1969; Hajdú: , The Sumerian myth and reality 1972; Hajdú: , The Sumerian question and reality, 1977.).

However, we absolutely must deal with a remarkable idea raised by Péter Hajdú. First "Our Sumerian Connections" (Our Sweet Mother Tongue, Vol. III, No. 2, 1981.), because there are some very of all, he sharply distinguishes himself from the assumption of Sumero-Hungarian or Sumero-Uralic linguistic kinship from its exaggerated manifestations, more precisely from its extension to the ideological, racial, and even religious-historical space. — We would like to emphasize right away that we ourselves agree with this distinction. — In response to the refutation of the hypothesis of the kinship of Hungarian and Sumerian, or of the Uralic languages and the Sumerian language, Hajdú only notes that "...this belief is outdated in linguistics; it is not current, so it tries to maintain ideas that have lost their credibility." Although this tone is already dead-1020

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It is much more reasonable than Hajdú's aforementioned statements of 1969, 1972 and 1977, and even approaches objectivity to a certain extent, and although the discussions of Sumerian-Hungarian linguistic relations so far have almost without question come to the fore in favor of the genetic relationship between Sumerian and Hungarian, we must nevertheless point out with emphasis that - with regard to our own areal approach to language equalization - a very essential circumstance escaped Péter Hajdú's attention. Namely, if any "belief" in linguistics is truly "outdated; not current", then it is precisely the 19th-century "belief" of genetics, i.e. thinking in languages considered to be related by descent, and in close connection with this, the also genetic, language family tree-like relationship of the Uralic-Finno-Ugric languages.

idea, with their successive unified ancestral peoples, ancestral homelands and basic languages, i.e. the so-called "Uralic", "Finno-Ugric" and "Ugric" ancestral peoples, ancestral homelands and basic language levels. The complete obsolescence of these views in international linguistics, and even their methodological impossibility, have been clearly shown in our book (pp. 406-428), supported by ample citations from the literature.

As a result of this linguistic situation, the problem in the field of possible Sumerian-Hungarian linguistic connections is not at all whether there is a genetic linguistic relationship between Sumerian and Hungarian — as Hajdú believes, and which version of kinship he tries to refute — but rather the question revolves around the nature and intensity of those linguistic connections — which, according to all available data, were like areal language equalization — as a result of which many hundreds of Sumerian words and numerous grammatical features have regular and obvious phonetic, semantic and grammatical parallels in Hungarian.

Even more characteristic are Hajdú's further explanations, in which he once again rejects the possibility of Sumerian-Hungarian linguistic relations by saying that Finno-Ugric linguistics has succeeded in compiling an inferred (!) and assumed (!) linguistic system from which "...the more notable forms of the vocabulary and grammar of the related languages can be derived." However, according to Hajdú, we cannot speak of a "basic language" that would include both Uralic (Hungarian) and Sumerian.

Hajdú's convulsive efforts, with which he clings to the 19th-century idea of "language families" and "basic languages" that are considered genetically related, and presents the issue as if language relatedness - as such - were only conceivable on a genetic, i.e. descent basis, are striking. At the same time, he does not even hint at the fact that, for example, Indo-Germanic research has become much, much more convincing in 150 years, in essence-1021

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developed a much more comprehensive and far broader "inferred, assumed linguistic system" than Finno-Ugric studies could ever manage to do (since this language family has practically no linguistic monuments), and from which Indo-Germanic "linguistic system" incomparably "more notable forms" of the vocabulary and grammar of Indo-Germanic languages could be derived than those produced by Finno-Ugric studies, and despite everything, modern international Indo-Germanic linguistics has in recent decades conclusively proven that the attempted linguistic derivation of the previously assumed common ancient Indo-Germanic base language is theoretically impossible, even with the knowledge and help of Indo-Germanic linguistic monuments, which are many thousands of times more numerous than the monuments of the Finno-Ugric languages and which predate them by several millennia, and the designation "Indo-Germanic" is nothing more than a mere linguistic — purely structural — — concept. It has been clearly shown that the Indo-Germanic languages did not have a common basic language, much less a common ancestor, but only developed relatively late, starting roughly at the beginning of the 2nd millennium BC, gradually and in different periods, through multiple language mixing and superstratifications — that is, areal language equalizations. (Our 1st book, pp. 407-427 and 549-552.)

The situation of the school that claims the genetic affinity of the Ural-Finno-Ugric language family is also greatly aggravated by the fact of paramount importance that the Indo-Germanic languages are much closer to each other than the languages of the group called the Ural-Finno-Ugric language family. This circumstance, that is, the much looser, much more distant relationship of the Ural-Finno-Ugric languages than the Indo-Germanic languages, is also acknowledged by Finno-Ugric linguists themselves. Thus, for example, according to József Szinnyi, the members of the Finno-Ugric language family (!) relate to each other in approximately the same way as the individual Indo-Germanic language groups (!). (1. Szinnyi: „Magyar nyelvhasznítás 1927. p. 4: — quoted by Endrey 1982. p. 49, — emphasis added by us).

According to Géza Bárczi, the degree of kinship between the individual Finno-Ugric languages does not even come close to the similarity between the different members of the Latin or Germanic group, and can best be compared to the kinship that exists between the different language groups of the Indo-European language family, e.g. the Germanic and the Slavic groups. (1. Bárczi: Hungarian language 1943. . The p. 272 — quoted by Endrey 1982. p. 49; emphasis added).

From all this it follows that the idea of the genetic, descent-related relationship of the Finno-Ugric — and even more so of the Uralic — language family was originally built on extremely shaky foundations, and in the sense of the norms and methodological guidelines valid in international linguistics today, it can in no way be maintained. So in fact, the genetic meaning of the relationship of the Ural-Finno-Ugric language family is precisely that of the

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The field is the "belief" which, in the words of Péter Hajdú, "is outdated in linguistics; it tries to maintain ideas that are not current, therefore have lost their credibility."
Furthermore, as is clearly evident from the above quotes by Szinnyei and Bárczi, Uralic Finno-Ugric linguistics even grossly violated the methodological requirements of the 19th century language family approach - one might say disciplined them - when it classified distant languages into the same language family, interpreted as being genetically related, whose linguistic connections do not even approach the intensity of the parallels between other language families, e.g. the Indo-Germanic languages. Antal Endrey, for example, very correctly states that "...our linguists worked with a false category when they grouped the Finno-Ugric languages, which were only very distantly related to each other, into a common language family." (1. Endrey 1982, p. 50 - emphasis by Endrey.)

It is worth observing what a fundamental, methodological contradiction the continuation of the outdated idea of the genetic Ural-Finno-Ugric language family leads to, on the one hand, and the panic-like fear of researching the Sumerian connections of the Hungarian language, on the other hand, even in Péter Hajdú himself. We have already mentioned that in his book "Introduction to Ural Linguistics", Hajdú holds fundamentally different views on the origin of the Uralic peoples and their ancient linguistic relations. He states that according to archaeological data, the language of the Urals ...developed near the centers of development of several old Eurasian languages and language families", and it is likely that "...in addition to the archaeological ones, linguistic traces also point to the southern connections of the Pre-Urals" (1. Hajdú 1966. pp. 9-10). Moreover, he also recognizes that the methods of comparative linguistics to date are insufficient to solve the problems arising in the study of the more distant relations of the Uralic languages, and that in the future paleolinguistics will have to approach the linguistic events of this distant era with new methods. In this regard, he draws attention to the importance of applying probability theory in the exploration of ancient kinship-like relations between different language families, since the possibility of a coincidental coincidence of two words in a comparison of two languages is greater than in the case of a comparison of many languages.
He then notes that researchers have so far discussed the similarities of the Uralic-Indo-Germanic, Uralic-Altaic, Indo-Germanic-Semitic, Indo-Germanic-Sumerian, etc. language families separately. However, a more reliable result is expected if we compare the languages of all Eurasian language families together (1. Hajdú 1966, p. 91). He then points out that the very old genetic relationship of the Indo-Germanic, Semitic, Hamitic, Ural-Altaic, Chukchi and Kartvelian languages "...can be established with great probability" (1. Hajdú 1966, p. 92). Thus, Péter Hajdú clearly saw the ancient connections between the West Eurasian language families as early as 1966, and even one of the future research

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He stated as an important objective the complex examination and exploration of these ancient relationships using new methods. It is quite astonishing, however, that while on page 91 of his book cited above he explicitly referred to the similarities between Indo-Germanic languages and Sumerian (1. above), in his final summary, on page 92, he does not mention Sumerian, although he states that the genetic relationships of almost all Eurasian, North and East African language families, from the Hamitic languages to Chukchi, are "highly probable".

Yet the archaeological traces that he also believes point to the southern connections of the Urals have also clearly shown — much more specifically since 1966 — that it was precisely the Proto-Asian-Mesopotamian time, for millennia. In other words, according to Péter primitive civilizations that exerted decisive cultural, linguistic and ethnic influences over almost all of Western Eurasia, as far as could be recognized at that Hajdú, a "highly probable" genetic relationship between the West Eurasian and Northeast African language families - which we do not evaluate as a genetic language relationship, but rather as the result of a Neolithic and Early Metal Age West Eurasian areal language equalization process, and which we could most probably call a "West Eurasian proto-linguistic community" - was indisputably formed, according to the mass of archaeological finds, as a result of the effects of the huge migrations and colonizations originating from East Asia and repeated in waves for more than four full millennia, between approximately 6000 and 2000 BC. Since the Sumerian language was demonstrably the dominant commercial, state, literary, cultic and scientific language in all respects in the original homeland of these settlers who were farmers and later metalworkers scattered throughout Western Eurasia, in East Asia, for about 2500-3000 years, the position that, for example, the Chukchi language should be included according to Hajdú, while Sumerian should be excluded from broader paleolinguistic research examining the ancient relationships of individual language families, as suggested by the aforementioned list by Péter Hajdú, can be called absurd. On the contrary, Hajdú's above-mentioned paleolinguistic findings of fundamental theoretical importance directly demand that we not only include the Sumerian language in comparative linguistic studies that go beyond the boundaries of language families, but that we should also primarily start from Sumerian linguistic monuments in such research. This process is clear from the archaeological material available to us.

This is a compelling methodological requirement that cannot be ignored with any excuse. Hajdú's procedure, which, in direct contrast to all the

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because it has archaeological data, but also its own principled conclusion
In this context, he categorically rejects the inclusion of Sumerian in the research of the more distant relations of the Uralic languages, which relations and connections - and we must emphasize this very emphatically again - according to all the available local data, were not of a genetic, i.e. descent-related nature, but rather acted in the direction of the areal linguistic alliance-like equalization of the Uralic, 3114[1 and, with great probability, other languages.
At this point, there would also be an opportunity to reconcile to a certain extent the seemingly diametrically opposed positions of the "officialized" academic school of thought that the Hungarian people and language are of Uralic-Finno-Ugric origin, and the trend that considers this rigid theory unprovable, improbable and obsolete and seeks the antecedents and early connections of the development of our people and language in much more southern areas. We should only agree in advance that for the time being we should not debate such - in our opinion - secondary problems, such as the question of whether Hungarian is a Finno-Ugric language or a Finno-Ugric language, but rather that we should first consider paleolinguistic questions globally and examine thoroughly, without any "genetic" language family prejudice, the more distant ancient connections of Uralic-Finno-Ugric and other West Eurasian language groups, namely precisely on the basis of the theoretical and methodological findings of Péter Hajdú introduced earlier, i.e. several Eurasian so-called

"language family". However, we must of course include Sumerian in this broad comparison of proto-languages,
In fact, we must start directly from Sumerian in these paleolinguistic investigations, because on the one hand, Sumerian was the first and earliest written cultural language of our land, which was the dominant cultural language in all respects in East Asia for about 2500-3000 years, and on the other hand, according to the clear evidence of archaeological finds, the vast agricultural and metalworking colonizations that once started from East Asia exerted linguistic influences of paramount importance on the languages of the primitive, sparsely populated fisher-hunter natives of the aforementioned regions in the southern areas of Europe and Western Asia, all the way to the northern forest belts. In other words, taking the Sumerian language into account is an indispensable primary methodological requirement for any global paleolinguistic comparison.

Here, we believe, the statement of János Harmatta's presentation described above, according to which Uralistics has shown that the formation of the Uralic-Finno-Ugric basic language took place in the process of areal contacts, could be connected to these wide-ranging, complex linguistic studies, because the distribution of typological features clearly points to four or five separate groups.

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In our opinion, it would be very fruitful if this line of thought were seriously considered by the experts of Finno-Ugric linguistics in Hungary. No one disputes — least of all ourselves — that they have a very thorough formal linguistic background, and thus they would be the ones primarily called upon to explore the nature of these extremely complex, multifaceted paleolinguistic processes. What we disagree about can actually be reduced to the only difference in perspective, that our trained Finno-Ugric linguists focus all their knowledge solely on supporting the 19th-century dogma of the genetic relationship of the Uralic-Finno-Ugric languages, and that other ancient linguistic connections — despite the above-mentioned theoretical position of Péter Hajdú — are not even taken into account in practice. We do not doubt for a moment that if Hungarian linguists did not cling rigidly to the genetic language family approach of the 19th century, but examined the other connections of the Hungarian language beyond the Ural-Finno-Ugric languages with the same thoroughness and intensity as they have so far researched the relations between the Ural-Finno-Ugric languages and Hungarian, then within a foreseeable time they would be able to resolve those eminently important, global paleolinguistic questions that have been raised by the archaeological research in Pre-Asia of the past 100 years, and which — we emphasize this again emphatically — have led to the rejection of the hypothesis of an Indo-Germanic ancestral people and basic language by the entire line of the most authoritative scholars of international linguistics for several decades.

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Chapter 4
THE EARLY NEW CENTURY FEMISTIC PROTO-ASIAN LANGUAGE KOINE
In our series of studies entitled "The Sun Rises in the East", we have repeatedly pointed out that the current results of archaeological research indicate with great probability that in Neolithic, Chalcolithic and Early Bronze Age Near East, a common, widely used, spoken language (so-called lingua franca) existed, which we call "Neolithic-Early Metal Age Egeo-Jian Nyelian Koine", which, according to all indications, had several, temporally successive linguistic stages. We emphasize that this Near East can be evaluated as the result of language equalization processes. This view is supported — with very serious arguments — by the complex interactions, expansions, mixings, and superstratifications of the numerous, archaeologically distinct pre-Asian Neolithic and Chalcolithic cultures that can be revealed from the finds. Within the framework of these extremely complicated cultural connections and unification processes, and as a parallel result of these, we must view the areal as a linguistic projection of the pre-Asian cultural koine.

also the processes of language leveling, i.e. the development of the Proto-Asian linguistic koine, which, according to the archaeological findings of successive cultural circles that expanded over time to larger and larger areas and became increasingly unified, had several linguistic stages that followed one another in time (for more details, see our Book I, pp. 707-712 and 878-880). Due to the eminent importance of the question, we will briefly summarize what was said there below.

In Chapter 1 of our book "The Sumerian Question", we have shown in detail that the impulses determining the development of Pre-Asian culture have been increasingly concentrated on the central areas of Pre-Asian culture since the beginning of the 6th millennium BC - Mesopotamia, Syria and the westernmost strip of Iran. The earliest, relatively uniform cultural circle, the Hassuna culture of northern Mesopotamia, which appeared around 6000, developed in this region, and then as an organic continuation of this, around 5600 - also in northern Mesopotamia - the Samarra culture appeared, which, however, already extended to much larger areas than the preceding Hassuna culture. Roughly simultaneously with the Samarra culture, also

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In the middle of the 6th millennium, a little further west, in the region of Syria, the Tell Halaf culture emerged, also with clearly distinguishable and largely uniform finds. Then, through the mutual interactions of the Samarra and Halaf cultural circles, and finally through the dominant expansion of the Tell Halaf culture into the Samarra areas, the unified Samarra-Halaf cultural complex emerged around 5000, which already extended from the Mediterranean Sea to the Zagros Mountains. In other words, starting with the consolidation of this cultural circle, from about the turn of the 6th/5th millennium, we can rightly speak of considerable cultural uniformity in the region of Syria and Northern Mesopotamia.

This cultural homogeneity, as well as the find complexes testifying to the relatively developed cultural conditions: the significant processing of copper, lead, gold, and silver at numerous sites, the settlements that were already distinctly urban in nature (impressive defensive walls, ramparts, church buildings, etc.), the great development of pottery production and decoration in view of its achievements, the extremely advanced textile production, the general use of seal presses, the appearance of irrigation systems in several places, etc., many researchers have come to the conclusion that at the time of the flourishing of the Halaf culture, between about 5000 and 4500, state formations of considerable importance must have already existed in Northern Mesopotamia and Syria (1. Shachermeyr 1979, pp. 14-15). Under such circumstances, however, we must also assume with a very high degree of probability that, as a linguistic projection of this cultural homogenization and the state formations that are also highly probable, a considerable degree of linguistic equalization must have occurred in the area of the Halaf cultural circle among the different populations. It was evident that a common language, a lingua franca, had to develop among the various idioms of the groups of origin. This common language The language, however, was undoubtedly based on the dialects of the two most culturally active groups of the Halaf cultural circle, namely primarily the Halaf and secondarily the Samarra ethnic group (or ethnic groups), and was mainly created through their mixing and equalization, but with a certain degree of Halaf dominance, given that ultimately the Halaf culture spread to the Samarra areas and not vice versa.

This linguistic state, in accordance with the archaeological nomenclature of the region, can most appropriately be called the Halafian communicative linguistic stage of the Proto-Asiatic language Koine (Samarra-), the parenthetical designation "Samarra" also indicating that this communicative linguistic stage very likely had two more or less understandable antecedents in the separate lives of the Samarra and Tell Halaf cultural circles, between ca. 5500-5000, namely a separate Samarranian and a separate Halafian communicative linguistic stage. To support this opinion, it is sufficient to point out that

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that in the Samarra cultural circle, around 5400 BC, there are remains of multi-level irrigation systems in two places (in Tell es Sauwan and 13000 BC in Mami): archaeologists have excavated traces of them. However, for the construction and continuous maintenance of irrigation systems, the existence of a common language spoken by larger groups of people is essential. Based on this latter consideration, we must date the period of the separate Samarra and Halafian communicative linguistic stages to between about 5500 and 5000 BC. This linguistic state was then followed by the unified (Samarra-) Halafian communicative linguistic stage between about 5000 and 4500. This "lingua franca" had already spread over a considerable area. The boundaries of its use are formed by the borders of the core areas of the Halaf cultural circle, i.e. the Lebanese-Syrian coast of the Mediterranean Sea and Cilicia in the west, the Diyarbakir area and Lake Van in the north, the Zagros Mountains and the Kermanshah region in the east, and the Baghdad heights, the middle section of the Euphrates to the great bend of the river (east of Aleppo), and then back to the Syrian coast.

At the same time, however, in Southern Mesopotamia we must also take into account the separate, common language of the settlers of the 1st and 2nd periods of the Eridu-Ubaid civilization there (between approx. 5400-4500 BC), and even at a much more developed level there than in the north, since the creation and continuous operation of irrigation systems, which are essential for farming in Southern Mesopotamia — in the case of such huge rivers as the Tigris and the Euphrates — is unimaginable without effective central control and common understanding between the population of large areas. The bases of this southern linguistic equalization were obviously the Samarra dialects of the earliest settlers of the 1st period of the Eridu-Ubaid culture, which were then expanded with a considerable amount of Halafian linguistic elements during the 2nd period of the Eridu-Ubaid culture (cf.: in the 1st period of the Eridu-Ubaid culture, mainly Samarra parallels can be detected in the archaeological finds, and in the 2nd period, mainly Halafian parallels; — 1. e.g. Amiet 1977, p. 522). So already at the time when this southern Mesopotamian, Eridu-Ubaid 1st-2nd The language community that can be called a communicative linguistic stage of the 1st millennium BC (between ca. 5400-4500) was still relatively insignificant compared to the contemporary (Samarra-) Halafi, it was limited to a much smaller area than the latter, each of them undoubtedly had to contain numerous common linguistic elements, given that in the end both arose through the mutual areal equalization of the smaller clan-tribal languages of the Samarran and Halafi ethnic groups — only the northern language community could have been based more on Halafi (cf. with the superstratification of the Halafi culture on the Samarran civilization), and the southern one primarily on Samarran (cf. the first southern Mesopotamian settlers came from the area of the Samarran civilization, according to the data available to us).

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Then, starting around 4500 BC, roughly in the former Samarra-Halaf areas, but also including Southern Mesopotamia, and extending to the western half of Iran and a further strip of Southeastern Anatolia (as far as Malatya), we find the cultural koine of the 3rd and 4th periods of the Eridu-Ubaid culture, in the more northern region called the Northern Ubaid cultural circle, in which, however, Southern Mesopotamia clearly played the leading role. At the same time, the artefact complexes of this cultural circle testify to a much greater degree of cultural equalization than was evident in the preceding Halaf period. In other words, in the case of this cultural complex, we are talking about the pre-Asian cultural dominance of the extremely culturally active southern Mesopotamian population — which, according to all available archaeological data, was already Sumerian in this period (pp. 698-706 of our book 1) — extending to far-flung regions: this culture was everywhere superimposed on various smaller and larger local cultures, and even on the considerable Halaf cultural circle, and unified the cultural image of the aforementioned vast areas.

This almost incredible dynamic can undoubtedly be explained first and foremost by the demographic pressure of a new, enormous population growth, which arose as a result of the enormous surplus of products from the developed irrigated agriculture of the southern Mesopotamian plain and rivaled the first population growth after the beginning of the Neolithic Age, as well as by the continuous and rapidly increasing demand for raw materials (metals, wood, stone, gemstones, etc.) of the increasingly differentiated culture of southern Mesopotamia, which was poor in raw materials.

However, it is also obvious — as we have already mentioned above — that the development of the Eridu-Ubaid civilization with its irrigated agriculture and its expansion to the whole of Southern Mesopotamia can only be imagined within the framework of an effective central control, a clearly state-like power organization covering considerable areas, which in turn also presupposes a commonly understood language. Since after 4500 BC the Eridu-Ubaid civilization expanded to almost the whole of Near East, along with this cultural conquest, the language of the bearers of this culture, originally spoken only in Southern Mesopotamia, undoubtedly became the dominant "lingua franca" in a large part of Near East Asia. This linguistic state represents the Eridu-Ubaid 3-4th or Northern Ubaid Communicative Language Stage of the Proto-Asiatic Language Koine and its period of spread can be dated to the 3rd and 4th periods of the Eridu-Ubaid culture, between ca. 4500 and 3800/3700. However, taking into account that the population that developed and carried the Eridu-Ubaid culture was very likely already Old Sumerian or Pre-Sumerian (in the sense of the linguistic term "pre-Hungarians"), it seems more correct to date it to Eridu-1030

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We are talking about a 3-4th century Sumerian (or Pre-Sumerian) communicative language level. Then, starting around 3800 BC, the rapid expansion of the Uruk culture, also of southern Mesopotamian origin and of undoubtedly Sumerian ethnicity, can be observed in the archaeological finds. This culture proceeded in the same way as the former expansion of the preceding northern Ubaid cultural circle. 8 Clear archaeological evidence of its massive dominance also appears from the eastern coast of the Mediterranean to Central Iran (Uruk-Gaura culture, between ca. 3700-3200). The lingua franca of this cultural koine was already evidently Sumerian, and the Sumerian language remained the dominant "lingua franca" of East Asia in all respects, the international cultural, cultic, commercial and state language, until around 2000/1900 BC. The name of this linguistic state would therefore be the Uruk-Gaura lingua franca, according to the terminology used so far, starting from around 3700 BC, i.e. roughly since the presumably earliest beginnings of pictographic writing, the oldest known monuments of which date from around 3500 (Habuba Kabira, Tell Uananas; our 1st book, p. 697, also literature there).

It must be taken into account, however, that the successive Proto-Asiatic communicative linguistic stages outlined above were themselves linked by very significant linguistic connections. 1./ The linguistic equalization of the Samarra and Halaf ethnic groups created the (Samara-) Halaf communicative linguistic stage. 2./ The language of the Eridu-Ubaid 1-2 period communicative linguistic stage in southern Mesopotamia was also created through the mixing of linguistic elements of Samarra and Halafi clans and tribes. 3./ This Eridu-Ubaid 1-2 period communicative linguistic stage then mixed again with Samarra and Halafi linguistic elements, this time with the already balanced (Samarra-) with the language of the Halaf period communicative language stage, and after 4500 the Eridu-Ubaid 3-4 period or northern Ubaid period communicative language stage developed. 4./ Finally, a new linguistic equalization between the Eridu-Ubaid 3-4 period and the emerging Uruk-Gaura period communicative language stage, which resulted in the final consolidation of the Old Sumerian language, already known from written records and widely spread throughout almost all of East Asia.

It is therefore obvious that the Sumerian language must have originally contained a great many linguistic elements from the linguistic states of the (Samarra-) Halaf period, the Eridu-Ubaid 1-2 period, and the Eridu-Ubaid 3-4 period. That is, all signs indicate that the (Samarra-) Halaf period and the Eridu-Ubaid 1-2 period can rightly be considered as languages with a very strong Sumerian character.

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In addition to these Mesopotamian-Syrian rural linguistic stages of the East Asian language koine, however, in the western half of East Asia, more specifically in the area of the Inner Anatolian (Hacılar, Catal Hüyük), Cilicia (Mersin, Tarsus) and Levantine coastal (Amuğ-sıksäg, Ugarit, Byblos) coreneolithic cultural circles, we must also reckon with a more or less common language extending over these large areas. The earliest beginnings of the wider spread of this linguistic equalization can be dated to ca. 6000-5500 BC, that is, to the time of the consolidation of the aforementioned cultures and the strengthening of their mutual relations. In such ancient times and with the then relatively low level of culture, however, it is unlikely that the 7/6. At the turn of the 20th century, a truly unified, commonly understood communicative language would have developed, but initially we can only count on a few regional communicative linguistic clusters used only in smaller areas, sometimes with relatively narrow common vocabulary required by contacts, which could obviously only include the most important expressions of everyday life. This ancient linguistic state, in the smaller clusters of which linguistically equalization was earliest, was undoubtedly dominated by the linguistic elements of the bearers of the most advanced cultural circle of the region at that time, the ethnic groups of the Hacılar-Catal Hüyük cultural complex, we can call it the early Anatolian or, more precisely, — in line with our previous terminology — the partial linguistic equalization of the early Hacılar-Catal Hüyük period.

In the 6th millennium, as a result of the rapid growth of the population and the increasingly close relations between them, the linguistic equalization also took on ever greater dimensions, and thus by the middle of the 6th millennium we can undoubtedly speak of a real Anatolian Communicative Language, the former existence of which is, incidentally, attested many thousands of times by the characteristic geographical and toponyms of the ancient, pre-Indo-Germanic, so-called Anatolian-Aegean-Tyrrhenian language community that have survived to this day, from Eastern Anatolia to the Carpathian Basin, Italy and the Iberian Peninsula. (For more on this ancient Anatolian-Aegean-Tyrrhenian language community, see our book I, pp. 845-876.) This Anatolian-Levantine Communicative Language of the 6th millennium can rightly be called the Communicative Language Stage of the Late Hacılar-Catal Hüyük Period, and its use dates back to ca. We can date it between 5500 and 5000 BC. This Anatolian-Levantine language community probably had very little in common with the Mesopotamian-Syrian lingua franca discussed above, since it seems to have developed more or less independently of the latter. However, we must not forget that in the immediate south-eastern neighbourhood of this Anatolian-Levantine language community, in Syria and northern Mesopotamia, the cultural and linguistic koine of the Tell Halaf and Samarra periods began to develop as early as 5500 BC, and this cultural circle and its lingua franca were already in existence by 5000 BC.

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Around the beginning of the Halaf period proper, it had already spread to the Levantine regions and even to Eastern Anatolia and Cilicia. This clearly means that from the second half of the 5th millennium onwards we must reckon with very significant and increasingly intensive cultural and linguistic contacts between the Syrian-Mesopotamian and Anatolian cultural circles, which then reached their peak during the Eridu-Ubaid 3rd-4th periods and the Uruk-Gaura period. These archaeologically proven multi-order cultural connections clearly explain the fact, which may seem surprising at first glance, that the Anatolian-Aegean-Tyrrhenian language community has clearly demonstrable linguistic connections, on the one hand, with the ancient languages of the Eastern Anatolian-Transcaucasian region and the Zagros Mountains (South Caucasian languages, Subaric, Hurrian, Urartian, Lullubian, Gutian, Kassu, Elamite), and on the other hand — mainly through the Halaf connections — with Sumerian (Book I, pp. 847-849, but cf. also Schachermeyr 1979, p. 235).

We had to deal with these Proto-Asian proto-linguistic issues in such detail because this ancient Proto-Asian linguistic koine has an almost incalculable importance in the development processes of almost every language in the West Eurasian region. For the following reasons. It is no longer news to our readers that during the fishing-hunting-gathering economic stage of the Paleolithic and Mesolithic periods, population density could only reach such a low level that it is impossible to speak of the emergence of unified "primitive peoples" or perhaps some kind of "basic languages". In other words: under fishing-hunting economic conditions, no "basic language of a language family" could have developed, because there simply was not the compact population density absolutely necessary for such a thing. An illustrative example: today's Hungary has approx. Its area of 93,000 km² could support only about 4,650 people on average with a fishing-hunting economy (the population density in the fishing-hunting sector fluctuates between 0.01-0.09 people/km², depending on the ecological conditions of the given area; in our previous example, we calculated the average value, i.e. a population density of 0.05 people/km².)

Therefore, a significant population density that would allow a common, common language to develop over a larger area through regular contacts and mutual, permanent and close ties — that is, a so-called "language family basic language" in the perspective and terminology of 19th-century linguistics — is unthinkable under fishing-hunting economic conditions. As a result, however, any hypothesis that assumes the development of unified "Osneps" and common "language family basic languages" in the case of people groups living on a fishing-hunting economic level can be called completely outdated and even absurd in the light of our current knowledge. However,

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The development of both the Uralic and Finno-Ugric ancestral peoples and basic languages is dated to ca. 6000 BC and ca. 4000 BC, respectively, and localized to different regions of the Eurasian forest belt (Hajdú not to mention Péter's idea of an ancestral homeland in the tundra along the Lower Ob), the basic language of both Altai languages is also assumed to date back to the millennia BC and somewhere its origin, which is placed in Inner Asia, and the emergence of the Indo-Germanic primitive people and basic language, which is dated to around 6000 BC or even earlier, and the ancestral homeland of the Indo-Germans from Scandinavia to the Pamirs, in the times just listed — assumed by linguists — all areas were inhabited by fishermen and hunters, without any trace of agriculture and animal husbandry.

On the other hand, the productivity of agriculture and animal husbandry — even with the rather primitive production techniques of the early Neolithic — made it possible to increase population density by at least a hundredfold. This circumstance explains the development of the Neolithic and most of its languages, as described above. Since the Neolithic Age, i.e. food production — farming, animal husbandry — according to our current knowledge began in East Asia around 9000 BC, i.e. it preceded the beginnings of the Neolithic in other Western Eurasian regions by about 3000-7000 years (in some areas close to East Asia — such as In Thessaly — on the island of Crete — food production began around 6000, while in the Asian steppes we can only speak of the Neolithic Age from around 1800/1700 BC, after the emergence of the Andronovo culture — which culture, however, in this special case also meant the Bronze Age, so the steppe areas somehow skipped the Copper Age), therefore, as a result of this priority of several thousand years, in East Asia the large-scale population growth that necessarily goes hand in hand with the food-producing economic form naturally began several thousand years earlier than in other areas of Western Eurasia.

This Neolithic demographic explosion then led to overpopulation in East Asia — apparently over a period of about 3,000 years — and thus, around 6,000 BC, the mass migrations from East Asia began, which ultimately led to the colonization of almost all of Western Eurasia and filled the vast areas of Europe and Western Asia with East Asian settlers who cultivated agriculture, raised livestock, and later also had metalworking skills. These proto-Asian colonists, who arrived in large numbers in successive waves over a period of more than four full millennia (c. 6000 to 1800/1700 BC), reached all of southern, central and western Europe, as far north as Denmark and southern Scandinavia, in the east as far as the southern forest belt and the Ural Mountains; and in Asia as far as the Altai Mountains.

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To the Yenisei region to Central India (more details in 1. The Proto-Asiatic language is a mystery! Chapter 1, especially "The Sumerian Question", Chapter 2). These colonizations can be traced point by point with the help of close parallels and organic connections of archaeological finds, and it can also be clearly demonstrated that approximately every 500-600 years a wave of colonization occurred, moving particularly large masses, and these larger waves sometimes pushed the boundaries of the spread of the food-producing economic form further by several hundred or a thousand km. In other words, during these vast colonizations, the areally equalized linguistic grades of the ancient Proto-Asian cultural and linguistic koine, used in various 106-sectors, but apparently very close to each other (see above), undoubtedly became dominantly superimposed on the local national-tribal languages of the countless small indigenous groups living at the fishing-hunting economic level in the western half of Eurasia, and the indigenous ethnic groups, who were far behind the cultural, civilizational, economic and technical level of the Proto-Asian colonists and who were still living in primitive Upper Paleolithic-Mesolithic conditions at that time, adopted a considerable part of the lexical and grammatical elements of this Proto-Asian cultural language in a more or less distorted — pidginized — form, incorporated them into the vocabulary and structure of their own languages, and adapted their languages to them. to its own sound order. Of course, we can hardly ever fully explore these multi-stage and extremely complex paleolinguistic processes today and get to know them point by point in all their aspects. It can be clearly established from archaeological finds that settlers often arrived in a newly colonized area from two or three different cultural provinces of East Asia, who therefore obviously spoke dialects of a certain degree different from the corresponding temporal stage of the East Asian spoken language. On the other hand, it also happens several times that several waves of East Asian colonists settled in the same area one after the other, at intervals of 500, 1000 or even 1500 years, who in turn brought with them different temporal linguistic stages of the East Asian language koine. It is quite obvious, therefore, that all these circumstances make it extremely complicated to unravel such multiple linguistic superimpositions, blendings, levelings, and pidgin-like language mixings thousands of years later. This follows compellingly from the essence of the matter. However, looking at the issue globally, fortunately we have a guide, with whose guidance we can still glimpse the secrets of the course of these paleolinguistic processes from several angles. And this guide is none other than the European colonies that have taken place in the last 200-300 years, almost before our eyes, in the full light of history,

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the parallelization, precise observation and systematic analysis down to the smallest detail of language mixing, pidginization and creolization processes that took place under completely similar circumstances in almost every detail. This procedure often yields concrete and generally valid

We can also draw conclusions about the development of linguistic relations in these prehistoric times, the language mixing, linguistic superstratifications that occurred at that time, and the further independent development of pidgin-like secondary communicative languages that emerged from the pre-Asian language koine cultural language.

In these archaeologically proven large-scale pre-Asian colonizations, and in the multifaceted linguistic effects that necessarily followed from them (linguistic superstratifications, language mixing, pidginization processes, areal language equalizations, the development of secondary pidgin-like mediated languages, and their repeated branching, etc.), we find the rational reason, the obvious prehistoric explanation, for the fact that attempts were made to relate the Sumerian language to the most diverse languages of Eurasia. The fact is that very close phonetic and semantic parallels of the known words of the Sumerian language can be found in hundreds of almost all West Eurasian languages, and so we cannot be very surprised that, seeing so many correspondences, there have been researchers who have repeatedly tried to relate Sumerian to one of the Eurasian languages. However, under the spell of the genetic language family perspective that has been generally held in modern European linguistics for over a century and a half, all these language comparators sought a genetic, i.e., a derivational relationship between Sumerian and the other language occasionally compared with it, i.e., they wanted to fit the Sumerian language into the framework of some previously established language family hypothesis. — As we could very well observe this effort in the statements in this volume by János Harmatta and then by Péter Hajdú, both of whom could only imagine the investigation of the possible kinship relations between the Sumerian and Hungarian languages by demanding the reconstruction of a linguistic system that would be able to unite both the pre-Uralic and the pre-Sumerian languages into a common language family.

However, since we have previously shown that the presumed ancestral peoples and basic languages of the Indo-Germanic, Uralic-Finno-Ugric, Altaic and other languages, interpreted as genetically related language families in the 19th century, never existed due to the extremely low population density of the fishing-hunting economic stage, and since we have discussed the textbook example of these previously presumed genetic language families, the Indo-Germanic language family, which at that time had the most numerous and oldest linguistic relics - among some of whose languages the so-called "regular sound changes" were observed in the

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In the world of many old linguistic monuments, it could be worked out in much more detail and convincingly than in the case of the other mentioned language families. > The competent Indo-Germanic linguistics itself has proven in the meantime that the reconstructions of the original language based on "sound laws" were fundamentally flawed, and indeed, for theoretical and methodological reasons, they are simply impossible, and that the Indo-Germanic original people and the basic language never existed, but rather the mutual correspondences and parallels of the individual Indo-Germanic languages were created through areal language equalization, and indeed quite a few millennia later than the period to which the Indo-Germanic basic language was previously dated.

Namely, the beginnings of the development of the Indo-Germanic languages can most probably be placed after the turn of the 3rd/2nd millennium BC. There can be no doubt that the two epoch-making discoveries just mentioned — on the one hand, the fact of the extremely low population density of the fishing-hunting economic stage, and on the other hand, the theoretical impossibility of reconstructing the basic languages of a language family — mean that the concept of genetic language families, conceived in the 19th century, can rightly be placed in the cabinet of curiosities of the history of science.

Therefore, what remains for future comparative language research — as János Harmatta very correctly stated at the 1989 meeting of the academic prehistory department — is the in-depth examination of linguistic states that reached common intelligibility or the threshold of common intelligibility during areal language equalization, and then the detailed study of the loosening or breaking of linguistic ties due to various reasons — e.g. minor or major migrations — and the subsequent separation of independent individual languages; always and everywhere, if possible, primarily with the help of increased consideration of relevant archaeological finds.

However, in comparative linguistic research based on the perspective of areal language equalization, we must definitely highlight another very important aspect. Namely, that the so-called "Grimm's sound laws" cannot in all probability be considered as laws belonging to the inner essence of languages, but at most as tendencies towards the voiced consonants becoming voiceless and then fading out in the next stage — becoming "hezet", a hissing sound or a semivowel. Similarly, the so-called "regular sound changes" or "sound shifts" (Lautverschiebung), which linguistics believes are the consequences of Grimm's sound laws, are also not signs of the temporal development of languages, which develop in a regular manner over time.

would act with inexorability and, with the help of linguistic tools, would objectively determine and predictably control the changes in languages over time, but rather

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According to the sign, they play a primarily semantic role: they indicate the enrichment of the vocabulary of languages, the branching and disintegration of word families with similar meanings, and looking at the issue from this perspective, to a certain (but only quite limited) degree, they also play a role indirectly in the temporal development of languages, since the semantic enrichment of languages is ultimately a function of time. Regarding the sound laws and regular sound changes, we refer to chapters 3, 4 and 5 of our book "Towards a Proto-Asian Proto-Language", where we discussed these issues in more detail.

From what has been presented above, however, it follows that the etymological method of "phonological law" and "regular phonetic variation" that has been used exclusively in comparative linguistics so far is not sufficient in its traditional form, nor can it be used for a thorough examination of the real relationships between languages. The phonological law method breaks up semantically organic word families, and in some cases can only grasp a narrow aspect of them and set them in a genealogical parallel with each other. A new, comprehensive, complex method of language comparison is therefore needed, which not only takes into account the so-called "regular sound changes", but first and foremost examines the extent to which the parallel meanings of individual word groups with identical or very similar sound forms correspond to each other in the languages being compared. With the help of such a combined, semantic-phonetic method, it becomes possible to examine word families with similar sound forms that are organically related to each other in their meanings in a much more thorough manner than was ever possible with the previous "regular sound changes" method. In addition, this new method can also be used to better distinguish homonyms and homeophones that are identical or very similar only in their sound forms.

However, with our current historical, cultural, linguistic and especially archaeological knowledge, we can only conduct truly complex etymological and comparative linguistic studies if we do not remain within the boundaries of the so-called "language families" once established in the 19th century, but rather move towards the aforementioned "Towards a Proto-Asian Proto-Language"

Eurasian colonization in much more detail, independently of any kind of "regular" In Chapter 1 of our book, The Great West, which originated in East Asia, we also try to explore the linguistic aspects of sound change", "genetic language family", or reminiscence.

Below, as an example, we attempt to identify, in a geographical area selected after careful consideration, all currently archaeologically documentable cultural connections, parallels of finds, and the

1038

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1. y / 6 - 42516061 started in several waves, at different times, and the

|| to collect the known data on the colonial invasions that took place in the various regions of the world — as far as we are able to outline them, and the linguistic lessons that can be drawn from all of this

The area in question is one of the most important - or perhaps the most important - and primarily the most important - in terms of the ethnogenesis of the Hungarian people and the formation of our language, and its relations, and is the region of Western Asia and Eastern Europe, from east to west roughly between the Altai Mountains and the Dnieper River, or in a north-south direction from the forest belt to Iran, the Caspian Sea, Transcaucasia and the Black Sea, or in other words

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approximately the area of the Eurasian steppe belt.

1039

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Chapter 5

CULTURAL, ETHNOGENETIC AND PALEOLINGUISTIC CONNECTIONS OF THE EURASIAN STEPPE ZONE WITH FOREIGN ASIA

Western Asia

The typical pottery and tool forms of the Hassuna culture of &szk-Mesopotamia, which developed around 6000 BC, appear as early as 5300 BC in the Tepe Sialk I period of Central Iran (ca. 5300-4600 BC), and then at the turn of the 6th/5th millennium BC in southern Turkmenistan, at the sites of the Jetun or Chagalli culture and the somewhat later, so-called Mondjukli period, with agriculture, animal husbandry, village settlements and ceramics (see Propyläen Kunstgeschichte, XIII. Band 1974. p. 168). Later, but still in the first period, the influences of the ceramics of the northern Mesopotamian Samarra (between ca. 5600-5000) and then of the Syrian-Mesopotamian Tell Halaf culture (between ca. 5500-4500) appear in Tepe Sialk, which in the second They become increasingly powerful in the period (between ca. 4600-4000) (I. Hrouda 1971. p. 56). However, the ceramics of the Tepe Sialk II period, mixed with Halafian parallels, soon appear much further north, in the vicinity of Tehran (Ismaïlabad, Kara Tepe, Chesmi-Ali, etc.), with completely identical forms (I. Amiet 1977. p. 572).

It seems, therefore, that with the consolidation of the Samarra culture, starting around 5500, some Hassuna groups moved further east and north, and then when the Halaf culture expanded into the Samarra areas around 5000, part of the Samarra population also moved from their previous residences and settled further east and north, thus pushing the Hassunai who had previously settled there even further away. Thus, some Hassunai groups reached southern Turkmenistan around the turn of the 6th/5th millennium.

For a better understanding of the connections, we note in advance that, in parallel with the northern expansion of the Eridu-Ubaid culture that followed the Halaf culture (northern Ubaid cultural circle, between ca. 4500-3800), after 4500, the Halaf tribal population itself was apparently forced to move further north and east. It is known that the Halaf culture originally (between ca. 5000-4500) only directly expanded to the Lake Van region (Tilki Tepe) and the Rovanduz area (Gird Banahilk), i.e. it just reached the southwestern borders of the cultural group around Lake Urmia (yýýýýý Firuz, Hasanlu, Dalma Tepe, etc.). However, in the second half of the 5th millennium, after the emergence and dominance of the northern Ubaid culture in the former Halaf areas, the Halaf pottery

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It appears elsewhere in Teğut (near Esmiadzin), in Kul-tepe near Nakhichevan, and in Eisak-western Azerbaijan (L. Burney-Lang 1975, p. 77), and in the east — as we have just seen — in the same period, the pottery of the Tepe Sialk II period (between ca. 4600-4000) shows very strong Halaf parallels. Shortly afterwards, at the very beginning of the 4th millennium, the typical ceramics of the Urmia-t6 cultural circle, the so-called Dalma ware, appear at Ilanli Tepe in northern Azerbaijan (L. Burney-Lang 1975, pp. 80-81), and even somewhat later in the Namazga Tepe I period in southern Turkmenistan (between ca. 3800-3500), as well as in the lower layers of Anau in southern Turkmenistan of the same age (L. Hrouda 1971, pp. 57-58), after shortly before that, around the turn of the 5th/4th millennium, the influences of the northern Ubaid culture appeared more and more massively in the core areas of the cultural circle around Lake Urmia (see Burney-Lang 1975, p. 81). All this again suggests that the expansion of the northern Ubaid culture, which had been increasingly superimposed on the Halaf circle since 4500, triggered a chain reaction this time as well, pushing certain groups of the Halaf culture further north and east. These, in turn, pushed some of the populations around Lake Urmia and Tepe Sialk — partly of Hassun and Samarra origin — even further afield: to Azerbaijan, the Tehran area and southern Turkmenistan. Later, around 4000, the northern Ubaid themselves appeared in more distant areas, e.g. in the Lake Urmia region and Tepe Sialk.

These gradual partial migrations and partial stays of the ethnic groups of the successive cultural circles, while the migrated groups came into cultural, interethnic and linguistic contacts with the local fishing-hunting "szýzfódek"-natives, and those who remained in place with newly arrived immigrants from the more developed Mesopotamian-Syrian cultural circles, can be detected almost everywhere in archaeological finds. Since a larger mass migration wave repeatedly started from the culturally more developed central areas of East Asia about every 500-600 years, as a result of these repeated migrations, all these aforementioned ethnic mixtures sometimes moved a few hundred km further north, further east or further west, depending on the direction in which they lived from the central areas of East Asia. Finally, after multiple mixings and superstratifications, after the pre-Asian migrations and colonizations gradually ceased, these motley mixed ethnic groups settled in a geographical region for a longer period of time, and through regular, long-lasting neighborly relations, their languages also became more or less areally equalized. This resulted in a second-1041

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Communicative languages developed, but they were undoubtedly formed through various degrees of pidginization, mixing and layering of the different temporal linguistic stages of the Proto-Asiatic linguistic koine, i.e. each of them obviously contained a considerable number of linguistic elements from the Sumerian linguistic stages of the former Proto-Asiatic linguistic koine. Subsequently, these communicative languages branched out into individual languages for various reasons, such as minor or major migrations, while their speakers also consolidated into separate ethnic units. According to our theory, these areally balanced secondary linguals were the ones that 19th-century linguistics interpreted as the basic languages of a genetic language family, and whose vocabulary it attempted to reconstruct by means of the phonetic inferences of later words of the descendant languages using the phonological method.

Let us now return to the colonization from East Asia to West Asia. After the emergence of the Samarra and Halaf parallels in Iran in the 5th millennium, starting from the end of the 5th millennium, the northern Ubaid culture also began to spread further, e.g. in the very strong Eridu-Ubaid 4th and Susa A parallels of the three lower layers of the Tepe Sialk III period (between ca. 4000-3500), and these characteristic Ubaid-Elamite features also appear at the very beginning of the 4th millennium in northern Iran, on the southeastern coast of the Caspian Sea, in the Tepe Hissar I period (starting ca. 4000).

Somewhat later, around 3800-3700, clear northern Ubaid parallels also appear in the cultures of southern Turkmenistan. We have convincing evidence for direct Mesopotamian connections at Namazga Tepe I, Anau I and Karatepe (all between 3800-3500): several female clay figurines, for example, are almost identical to the figurines found in Mesopotamia of the Eridu-Ubaid 4th period (between 4000-3800) (L. e.g. Propyläen Kunstgeschichte XIII. Band 1974. p. 168). These similarities and several other indisputably Mesopotamian correspondences in ýýýýýýýýýý, modern research considers the aforementioned Chalcolithic cultures of southern Turkmenistan as the products of a mass immigration and colonization from Mesopotamia through northern Iran to Turkmenistan (L. Jettmar et alii 1966).

However, we saw above that in the same period, in addition to these Mesopotamian colonists, settlers from the Chalcolithic cultural circle of the Lake Urmia region also arrived in southern Turkmenistan, which is mainly evident in the close parallels between the pottery of Anau I and Namazga I with the wares found at Dalma Tepe near Lake Urmia. Moreover, these Mesopotamian and Lake Urmia settlers who arrived in southern Turkmenistan in the first half of the 4th millennium BC were already in contact with the earlier Hassun, Samarra and 1042 ===== PAGE 1031 ===== Ethnic groups originating from the Tepe Sialk region have been found in southern Turkmenistan (see above). This is an excellent example of what we said earlier about the extreme complexity and diversity of language mixing and language equalization: we can now see concretely that in these colonizations originating from East Asia we are often confronted with very complex, multi-leveled acculturation, ethnogenetic and language development processes, the systematic research of which has not actually even begun,

Here we would like to draw the attention of the younger generations of our prehistorians, archaeologists and linguists, the scientific "recruitment", to the fact that there are almost inexhaustible research opportunities in this field, in which they can choose between not only the so-called "associate theses", which are often mentioned with a rather pejorative aftertaste, and which are mostly completely insignificant and practically irrelevant in terms of the further development of the respective scientific discipline, but also truly significant, completely new insights, not to say real discoveries - serious opportunities lie ahead, requiring a relatively moderate investment of energy, given that this field of research is still, so to speak, "virgin land" for both prehistorians, archaeologists and linguists.

As we have already seen, according to recent studies, the first significant Mesopotamian settler groups reached southern Turkmenistan in the first half of the 4th millennium. We have also briefly mentioned that at the turn of the 5th/4th millennium, strong Mesopotamian influences appeared in the settlements of the Lake Urmia cultural circle, in the form of northern Ubaid parallels. For example, in the contemporary ceramics of Pisdeli Tepe and Geoy Tepe, the Eridu-Ubaid 4th period tradition became increasingly widespread, in addition to Elamite correspondences: (L. Burney-Lang 1975, p. 81). The path of Mesopotamian-Elamite influences towards Lake Urmia can be observed very well in the contemporary finds of Tepe Giyan in western Iran. Close parallels to the pottery there — corresponding to the Eridu-Ubaid 3-4 wares — are found at Lake Urmia, especially at Dalma Tepe, Pisdeli Tepe, in layers IX and VIII of Hasanlu, and at the Gird Hasan Al settlement (L. Hrouda 1971, p. 72).

All these connections, as well as the emanations of the Halaf cultural circle in the vicinity of Lake Urmia and in northern Azerbaijan in the second half of the 5th millennium, testify that since the last centuries of the 5th millennium, there had been continuous very close Mesopotamian relations with those Transcaucasian regions, which then, at the beginning of the last quarter of the 4th millennium, around 325 BC, developed as a much more modest northern reflection of the Sumerian urban high culture that had flourished 2-3 centuries earlier — an early Transcaucasian civilization with a completely uniform appearance over large areas (also known as the Kura Araxes culture), 1043 ===== PAGE 1032 =====

whose finds show massive Mesopotamian-Sumerian parallels all the way up to the first century of the 2nd millennium BC. In Mesopotamia itself, the Uruk period began around 3800 BC, and appears in archaeological finds as an uninterrupted continuation of the last, 4th period of the Eridu-Ubaid civilization. The characteristic Sumerian urban high culture developed in the Uruk period, initially only in southern Mesopotamia, but from the beginning of the era, very powerful cultural impulses spread from this leading cultural center to neighboring areas, 566. The Uruk culture became a model for the cultures of the entire East Asia, and no other culture could compete with it. This strong centralization initially necessarily led to the insignificance of more distant areas. However, after a short time, technical development, especially the acceleration of transportation (chariots, horseback riding), bridged greater distances, and the Near Eastern civilizations adapted to each other, but in such a way that the Mesopotamian center was always accepted as the driving force (L. Hrouda 1971, pp. 76—77).

In accordance with this cultural situation, the powerful emanations of the Uruk culture can be detected in more distant areas, but of course - as we have already experienced almost regularly with the expansions of the Mesopotamian civilizations of earlier times - again only with a certain phase delay. Thus, at the very beginning of the Uruk period, the emergence of only northern Ubaid forms indicates - indirectly - that the expanding population of the Uruk culture had already reached the core areas of the northern Ubaid cultural circle and displaced certain northern Ubaid ethnic groups from their previous places of residence. We should view, for example, the aforementioned late Ubaid parallels in southern Turkmenistan (Anau I, Namazga I) in the first half of the 4th millennium, around 3800-3700, i.e., in fact, at the very beginning of the Uruk period. So, once again, the well-known, repeatedly repeated process took place: the gradual expansion of a newly formed, developed, dominant cultural circle with great cultural activity and dynamics in the central, culturally leading Mesopotamian region of East Asia into the surrounding areas initially partially displaces the former inhabitants of these closer areas to the peripheries, to even more sparsely populated regions, and then in later times itself appears in more distant areas. Thus, for example, in Iran, the late Ubaid correspondences of the first half of the 4th millennium are successively replaced by clear signs of direct emanations of the Uruk culture starting from the middle of the millennium. In the IIV1-3 layers of Tepe Sialk (between ca. 4000-3500), the forms of the Eridu-Ubaid 4th period were still largely dominant, but around 3500, epoch-making changes took place in the IIV/4 layer almost without transition; large-scale metal casting began, the Mesopotamian art of pottery was developed, and the Mesopotamian art of pottery was developed.

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The use of stamped seal impressions becomes general, and the rapidly rotating potter's wheel also appears surprisingly early, the earliest remains of which were found in the archaic layer of Uruk VII, from about 3600/3500 BC. All this is a sign of a rapid technical and economic development (L. Hrouda 1971. 72. p., Amiet 1977. 572. p.) Shortly afterwards, at Tepe Sialk 111/6-08, between about 3400-3300 BC, the typical Uruk pottery appears, followed by Sialk IV. In the period (from 3300 onwards) the characteristic vessel forms of Uruk ware (so-called "flower pots", cans with slanted spouts, etc.) become increasingly common, 5 soon Mesopotamian-Sumerian seal cylinders and around 3000 Elamite-Sumerian pictorial clay tablets appear (L. Amiet 1977. p. 572).

Elamite-Sumerian pictograph tablets from the same period (c. 3200-3000 BC) were found in the V layer of Godin Tepe in western Iran, together with typical grayish-black Uruk vessels brought by a colonial population of Mesopotamian or Western Elamite origin (L. Amiet 1977, p. 552). According to the results of archaeological research, Godin Tepe was a walled Sumerian-Elamite colonial center around 3000 BC (L. Strommenger 1978, p. 18).

The Mesopotamian Sumerian civilization, which was already highly developed at this time, therefore extended its cultural influence to the western and central Iranian areas very early on, and even sent swarms of settlers there.

In southern Turkmenistan, a completely parallel cultural development can be observed at the same time. In the Namazga 11th period (c. 3500-3000), the use of seal presses begins, and copper becomes widespread. These Mesopotamian influences can be traced even further, through Afghanistan to Baluchistan. In the Tepe Hissar I period in northern Iran (c. 4000-3000), seal presses and cast copper are also present from the very beginning - i.e. before 4000 (L. Amiet 1977, p. 555). All this points to settlers of southern origin, coming from Tepe Sialk and ultimately from the Mesopotamian-Elamite region.

In southern Turkmenistan around 3000, a significant increase in the population can be observed, and in the simultaneous cultural stage of southern Turkmenistan, the Namazga Tepe III. period (between about 3000-2600), the direct effects of the Sumerian urban culture also appear: at the end of the period, urban settlements arise, and soon we find citadels and monumental buildings (L. Propyläen Kunstgeschichte XIII. Band 1974. p. 170). At the same time — undoubtedly due to the large-scale construction around 3000

As a result of population growth, the cultures of southern Turkmenistan themselves expanded their settlements to increasingly distant regions: at the beginning of the 3rd millennium, agricultural colonial settlements of the cultures of southern Turkmenistan appeared one after another in the neighboring northern and eastern regions, starting from the Aral Sea and along the Amu Darya all the way to Fergana,

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with a deceptively similar pattern to that of southern Turkmenistan (L. Jettmar et alii 1966).

All these phenomena were the consequences of a massive wave of immigration from Mesopotamia to southern Turkmenistan around 3000 BC, which was so large-scale that its concretely verifiable consequences can already be found in anthropological findings: the anthropological composition of the southern Turkmen population of the 3rd millennium BC is very close to the Mesopotamian bone material of the same period (L. Jettmar et alii 1966. Part I, afterword.).

Most researchers explain these extremely rapid Mesopotamian-Sumerian cultural emanations towards Iran, southern Turkmenistan, Afghanistan, and Baluchistan, which affected distances of several thousand kilometers, with the development of the lapis lazuli trade routes. The Sumerians held lapis lazuli in great esteem, as it was abundant in Afghanistan. It was therefore important to organize and ensure its continuous import. We cannot yet decide how the Sumerians solved this task: whether through a chain of colonial settlements or military bases, or in some other way. However, it is certain that lapis lazuli flowed continuously and in large quantities into Sumerian Mesopotamia from the second half of the 4th millennium, which in turn presupposes not only firm control of the trade routes, but also the organization and large-scale development of mining in Afghanistan. We do not believe that we are far from the truth if we attribute the rapid development of urban settlements after the turn of the 4th/3rd millennium BC — precisely along the northern lapis lazuli line, which led through Western Iran and Southern Turkmenistan, bypassing the inner Iranian desert, to Afghanistan (Tepe Sialk IV period, Tepe Hissar II period, Turang Tepe in Iran; Altin Tepe, Namazge Tepe III period in Southern Turkmenistan; Mundigak in Afghanistan) — to conscious Sumerian colonial activity. — All the more so because in this context we must absolutely emphasize that one of the deposits of a raw material that was much, much more important than lapis lazuli for the Sumerian culture, namely tin, which was also located in the same direction, near Mesopotamia: in Eszakket-Iran, around MeSed. According to the generally accepted view of the research, after the Sumerians discovered the secret of producing real tin bronze around 3200 BC, they obtained the tin essential for bronze production from the sites around Mexed (I. Müller-Karpe 1974, p. 126). MeSed is located near the Persian-Turkmen border, along the northern lapis lazuli route leading to Afghanistan, not far from Namazga Tepe and Altin Tepe.

Undoubtedly, not only the events that occurred in southern Turkmenistan around 3000 BC can be directly linked to these important trade routes

1046

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large-scale Mesopotamian colonization, although archaeological observations also show that even after this, the contacts of Southern Turkmenistan with Mesopotamia were not interrupted, and the slow but continuous influx of Mesopotamian settlers into Southern Turkmenistan continued throughout the 3rd millennium (L. Jettmar et alii 1966.).

The archaeologically — and in many ways anthropologically — detectable Sumerian settlements and colonizations also prove that the extremely rapid growth of the Sumerian population, which practiced advanced agricultural farming, lived within an organized state framework, and had a very high cultural level, made it necessary to remove the population surplus at regular — and not too long — intervals, which, according to the facts presented above, was solved by sending out swarms of colonists and settlers. We have seen above that this process began during the Neolithic Age, became increasingly intense during the Chalcolithic Age, and the areas of Iran, Southern Turkmenistan, Afghanistan, and Southern, Western, Central, and Eastern Europe, which had previously been sparsely populated by fisher-hunters, were filled with repeated waves of agricultural settlers from Near East.

In northern Iran, southern Turkmenistan and Afghanistan, in the following period from about 2500 to 2100/2000 BC, which corresponds to the Namazga IV period, cultural development is initially uninterrupted. Turang Tepe, Namazga Tepe, Altin Tepe and Mundigak grow into real cities. For example, Altin Tepe, according to the excavating Soviet archaeologists, was surrounded by a wall. There, a Sumerian-type stepped tower temple (ziggurat) with a floor area of 27.5x10.5 m and a height of 8-10 cm was built, which was rebuilt several times and significantly expanded in later times. In its final form, its facade was 55 m long and at least 12 m high. An even more monumental Sumerian-type tower temple was found in the mid-1970s at Turang Tepe, in the southeastern corner of the Caspian Sea. — These ziggurats are undoubted evidence of the pre-Asian hegemony of the Sumerian civilization in the 3rd millennium, extending as far as Turkmenistan — writes, for example, Amiet (L. Amiet 1977. 137, p.).

However, by the end of the 3rd millennium — according to Soviet researchers, somewhat later — the development of culture came to a halt, and the cities of southern Turkmenistan were soon depopulated, as were the Mundigs in Afghanistan. No definitive explanation has yet been found for this striking phenomenon. Most people seek the cause in ecological changes (the drying up of the oases). The question of where these peoples of pre-Asian-Mesopotamian origin and, although only provincial in nature, ultimately Mesopotamian-type culture disappeared around 2000, and who at that time could look back on cultural traditions dating back almost two millennia, can only be answered by an archaeological survey of the adjoining areas of Asia.

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As mentioned above, after the second large-scale wave of Mesopotamian Sumerian colonial settlements around 3000, the civilizations of southern Turkmenistan, whose population increased dramatically and who embarked on the path of urbanization, expanded their settlements as far as the Aral Sea and along the Amu Darya as far as Fergana at the beginning of the 3rd millennium. In the framework of these expansions, as a consequence of such cultural emanations from southern Turkmenistan, approx. Around 2700 BC, the Eastern Minarian culture can be traced along and north of the Aral Sea, as well as as far as the Ob-Irtis region, with its characteristic comb-impressed ceramics, and in several places signs of the beginning of agriculture, but with a clear predominance of hunting and fishing, i.e. still at a fairly primitive Coreneolithic-Subneolithic economic level, without the knowledge of metals. We will return to the question of the origin and spread of comb pottery, so for now it is only necessary to note — for the sake of a better understanding of the connections — that this type of pottery, as a so-called variant of the typical pottery of the Chalcolithic cultural circle around Lake Urmia, first appeared at the very beginning of the 4th millennium BC on Ilanli Tepe in northern Azerbaijan, and then — also in the company of other finds parallel — Dalma Tepe goods" provincial to the cultural circle around Lake Urmia — it also appeared in the layers of the Anau I and Namazga I periods in southern Turkmenistan around 3800-3700. It was evidently from here that it later migrated to the Kelteminari settlements.

However, through the connections of the Eastern Teminar culture extending to the Ob-Irtysh region, comb pottery reached the West Asian forest belt in the second half of the 3rd millennium, to the ancestors of the groups of the so-called "Ural-Finno-Ugric" peoples living east of the Urals. The appearance of comb pottery in the West Asian forest belt can in all probability be explained by the northward migration of Eastern Teminar people groups. This is confirmed, for example, by the striking Eastern Teminar parallels, in addition to comb pottery, that can be observed in the Eastern Ural fishing-hunting settlements excavated by Chernyetsov.

In connection with this issue, it is necessary to note that no archaeological connection can be demonstrated between the eastern — Trans-Ural — and western — European groups of the forest-belt ethnic groups, interpreted by 19th-century linguistic and prehistoric research as a people and language family of common origin under the name "Uralic-Finno-Ugric", in the periods discussed here, although in the times around 2500-2000 BC — the period we are currently dealing with — the Uralic-Finno-Ugric language family should have existed for 3500-4000 years according to the chronological hypothesis of Finno-Ugric linguistics, since the development of the "Uralic" basic language is dated to around 6000 BC, so for example, the clay of comb ceramics, which was also widespread among the "Finno-Ugric" people groups of the Eastern European forest belt, was always diluted with shell powder in Europe without exception, while in the Uralic forest-belt ethnic groups living east of the mountains

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and 25086061, or sometimes with gravel, neither of the two methods of production is ever mixed: no ceramic specimen with soapstone or gravel is known west of the Urals (L. László 1961, pp. 183-184). It should also be noted that combed pottery itself, as well as the custom of thinning with shell powder, came to the peoples of the Eastern European forest belt mainly from the Dnieper-Donets culture in the Pontic region and partly from the late Tripoli culture (see Müller-Karpe 1974, pp. 349-350), i.e. it was completely independent in its direct origin from the East-European forest belt groups of the Eastern European forest belt.

In the Altai, Sayan Mountains and Yenisei River region, around Minusinsk, the Afanasevo culture emerged around 2200 BC, the first food-producing and copper-processing culture of these regions, the finds of which show numerous close Mesopotamian parallels — through Western Iranian and Southern Turkmen transmission — especially in the shape of tools and ceramic motifs (L. Hanéar, 1956). Very close connections can also be demonstrated with the late pit-grave culture of southern Russia (between ca. 2500 and 2000) and with the Tripoli culture.

However, apart from the correspondences in the material relics, the most surprising -- and by far the most important -- connection is that the human appearance of the populations of the Afanasevo culture and the late pit grave culture is also the same. In both areas we find the same broad-faced, low-forehead and rather short-headed "plump-Europid" anthropological type, which in later times, until the appearance of the Mongoloids, would constitute the dominant anthropological type of the Eurasian steppes (L. Childe, 1960; Hanéar, 1956; Jettmar et alii, 1966). Very important in this context is the circumstance that this human race first appears in the second half of the 3rd millennium only in the Altai-Yenisei region and in southern Russia, at a distance of about 3000 km from each other, while in the vast area between them there is still no anthropological connection between the two populations, living very far apart but anthropologically identical.

From these antecedents, around 1800/1700 BC, the completely uniform Andronovo culture emerged in a vast area stretching from the Altai and the Yenisei to the Ural Mountains and the Volga, and from the forest belt to southern Turkmenistan in a north-south direction. Its population, now uninterrupted from Eastern Europe to the Altai, was of the same "plump-Europid" anthropological type as the populations of the Afanasevo and late pit-grave cultures in the second half of the 3rd millennium.

The development of the extremely extensive Andronovo culture is associated with the Afanasyevo, South Turkmen and Late Pit-Grave cultural circles, as well as the Middle Volga and South Ural regions, since 2000 BC.
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The research explains the slow, continuous cultural and ethnic integration of the emerging Early Bronze Age cultures (see 1. below) (1. e.g. Hanéar 1956.), while in the first centuries of the 2nd millennium the migrations from the region of Southern Turkmenistan to the north continued uninterrupted. In this period, agriculture becomes general in the steppe and forest-steppe zones of West Asia, and almost without transition a very high standard of bronze art appears - both in terms of its form and craftsmanship - which is already produced for export (1. Jettmar et alii 1966.).

The data listed above clearly show that there are very significant cultural-historical, but even more important ethnogenetic, connections between the depopulation of the southern Turkmen cultures at the end of the 3rd and beginning of the 2nd millennium and the development of the Afanasyevo and then Andronovo civilizations. Between 2500 and 1600 BC, the appearance of a new ethnic element bringing a higher level of civilization from the west and south can undoubtedly be detected throughout the Eurasian steppe. This new population introduced agriculture and animal husbandry, and they also brought knowledge of the production and processing of metals and bronze to the Cro-Magnon type of indigenous population (l. Randa, 1954.). B. Stawiski writes the following in connection with the depopulation of the cultures of southern Turkmenistan around 2000 and after: "...In the middle of the 2nd millennium BC this development was interrupted. Events that have not yet been clarified, by all indications, led to large-scale migrations. The migratory tribes settled in the vast steppes of Eurasia, in southern Ukraine, in the Volga region, in the southern Urals region up to the northern regions of Central Asia: eastern Kazakhstan, southern Siberia, and possibly even Mongolia." (l. Stawiski 1979. p. 19.).

The reason for the emergence of the Andronovo civilization and the relatively rapid population of the vast area between the Volga and the Altai can therefore be sought primarily in these large-scale migrations originating from the region of Southern Turkmenistan (and Northern Iran). The question, however, is how it was possible that the rather limited agricultural lands of the southern Turkmen oases could feed such large populations, which were then sufficient to populate the vast area stretching from the Volga to the Altai in just a few hundred years. This well-founded, completely obvious objection can only be resolved if it can be shown that in the second half of the 3rd millennium BC and in the first centuries of the 2nd millennium BC, large and repeated immigrations also took place in the cultural circles of Southern Turkmenistan itself.

Indeed, such repeated immigrations into the southern Turkmenistan (and northern Iran) region during the period in question are indeed well documented archaeologically.

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Thus, for example, at the site of Tepe Hissar in northern Iran, already mentioned in another context above, the settlement suddenly expanded massively at the beginning of the III. Period B. layer (between approximately 2350 and 2150), becoming a city4, and the population increased significantly, In addition to substantial residential buildings, a representative two-story building measuring 27x13 m was also excavated, in which the local ruler apparently resided. The rich finds clearly show that Tepe Hissar was connected at this time by diverse connections with Akkadian and Ur III Mesopotamia, as well as with contemporary Anatolian cultural centers (Alaca Hüyük, Horoztepe, etc.) and the Kuban-Maykop culture (l. Müller-Karpe 1974, p. 367). All this suggests that at the beginning of layer 111/02, a large number of immigrants arrived in Tepe Hissar, who brought about the aforementioned cultural boom and population growth. Given the sudden onset of the phenomena outlined above — around 2350 (1. above) — these migrations can most likely be explained by the fact that Mesopotamian peoples who fled the pressure of the Akkadian empire that had emerged at almost exactly the same time — around 2400 — settled in Tepe Hissar. - All the more so because the ground plan and layout of the aforementioned Tepe Hissar palace are very similar to the IStar temple of the 6th archaic layer of the city of yyyyy, which was built between 2500 and 2400. This, in turn, suggests that part of the population of the Sumerian colonial settlements in later Assyria that can be dated before the Akkadian rule probably fled to the northeast from the Akkadian expansion and presumably settled in Tepe Hissar.

The Mesopotamian connections are even more pronounced in the rich tombs of the yyy layer of Tepe Hissar (ca. 2150–1900). At the same time, the appendices again show far-reaching parallels with the finds of the Anatolian and Cuban princely tombs already mentioned in the IIVB layer (l. Müller-Karpe 1974, p. 355). In other words, the continuity of the culture of the IIVB layer still existed, the population of the yyy layer maintained close relations with exactly the same distant centers with which connections had already been established during the previous layer. It is also worth mentioning that a large amount of amber was found in the III/C layer of Tepe Hissar, which indisputably proves that Tepe Hissar was already involved in the long-distance trade in amber during the IIV/C layer, i.e. at the end of the 3rd millennium. In this context, it is also essential to point out that amber appeared in the southern regions only much later, around the middle of the 2nd millennium BC, and from East Asia — apart from the Tepe Hissar III/C layer — we only know of early amber from the 3rd millennium BC (l. Reallexikon der Assyriologie II. Band. p. 1) and from Alaca Hüyük (l. Müller-Karpe 1974. p. 124 and 853).

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amber finds from. In other words, with such find contexts, it is obvious that the large amount of amber clearly shows that the inhabitants of Tepe Hissar were actively involved in the long-distance trade network that the Sumerians had been regularly maintaining since at least the second half of the 4th millennium, and to which AS3ur 65 Alaca Hüyük had already belonged earlier, around the turn of the 3rd/2nd millennium. Thus, during the III/C layer, we must again count on Sumerian immigration in Tepe Hissar, namely, logically following from the origin of the layer between 2150 and 1900, groups that left Mesopotamia in the extremely turbulent times around or after the fall of the IIrd dynasty of Ur, when complete anarchy reigned there as a result of the Amorite immigrations.

At Turang Tepe (in the southeastern corner of the Caspian Sea) in the second half of the 3rd millennium BC — just as in the period of Tepe Hissar III — a surprisingly rapid cultural development can also be observed. The settlement here also expands enormously during this period, reaching urban dimensions, prosperity visibly increases significantly, and metalworking shows a sudden boom. These phenomena can undoubtedly be assessed as the results of direct and regular contacts with the Mesopotamian high culture, which existed in the region already during the early Sumerian dynasties, in the first half of the 3rd millennium — states, for example, Amiet (1. Amiet 1977. p. 137). However, these relations became much more intense and intimate in the second half of the 3rd millennium, which ultimately led to the appearance of the most characteristic manifestation of Sumerian culture and religious cults at Turang Tepe: in the 1970s, archaeologists excavated a huge Sumerian-type stepped tower temple (ziggurat) from the highest hill of the large settlement. This ziggurat is evidence that during the Akkadian and Ur III periods, Turang Tepe was not only confronted with regular Mesopotamian trade and other relations, but also with a large number of newly arrived Sumerian colonists, among whom — as evidenced by the design of the huge ziggurat, the organization of its construction and its successful execution — were obviously represented in significant numbers by the leading elements (priests, scribes, master builders, highly qualified experienced state officials) and the skilled artisan middle classes (masonry, metalworkers, etc.). Therefore, we can rightly conclude from this phenomenon that during the Akkadian rule, considerable communities - presumably entire smaller cities - migrated from Mesopotamia in one mass.

We must undoubtedly expect the appearance of similarly large, organized Mesopotamian-Sumerian groups in southern Turkmenistan in the second half of the 3rd millennium, during the Namazga IV period there, ca. 1052
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| between 2500 and 2100/2000. Starting with this period, the pace of cultural development in the southern Turkmen settlements also accelerates remarkably, 8 and during the second half of the 3rd | millennium, real cities develop, such as | Altin Tepe and Namazga Tepe.

| | y In Altin Tepe, during the Namazga IV period, the settlement also grew, with the built-up area expanding by at least 80 hectares. Altin Tepe was a substantial walled city at this time. The most significant building was the ziggurat mentioned above, the first version of which was built around 2300. In its original form, its floor area was 27.5x10.5 m, with four -- gradually smaller -- floors, the total height of which was at least 8-10 m. Its walls were divided by the angularly projecting vertical pillars and recessed niches typical of representative Mesopotamian buildings (l. Propyläen Kunstgeschichte XIII. Band 1974. p. 175; Amiet 1977. p. 548).

This ziggurat is therefore identical in detail to the Sumerian stepped tower temples of the 3rd millennium BC in Mesopotamia, and its origin — taking into account the rapid growth of the settlement and its fortification with a wall that can be observed at the same time — can very likely be directly linked to the appearance of Mesopotamian-Sumerian colonial groups around 2400-2300. This is also supported by the fact that the local production of tin bronze began almost exactly at the same time in Altin Tepe — but also elsewhere in southern Turkmenistan (l. Propyläen Kunstgeschichte XIII. Band 1974. p. 171), which had previously been a Sumerian monopoly (l. Burney-Lang 1975. p. 138). In the following Namazga V period, between ca. 2100/2000 and 1900/1800, the Altin Tepe ziggurat was rebuilt several times and greatly expanded. As mentioned above, in its final form, its façade was 55 m long and at least 12 m high. Another very important Mesopotamian agreement — and also evidence of direct, linear Mesopotamian Sumerian traditions — is the discovery of a large subterranean temple at the base of the Altin Tepe tower temple, just as subterranean temples are always found next to Mesopotamian Sumerian ziggurats (l. Brentjes 1977. p. 17).

The repeated rebuilding of the Altin Tepe ziggurat during the Namazga V period attests to the presence of a large number of Mesopotamian-Sumerian settlers in southern Turkmenistan during this period, who continued to cultivate indigenous religious cults. The large-scale expansion of the tower temple at the very end of the 3rd millennium and (or) the very beginning of the 2nd millennium, however, suggests that after the fall of the Ur III dynasty, new Mesopotamian-Sumerian groups appeared in the southern Turkmen region.

If we now consider the archaeological findings in the area of northern Iran and southern Turkmenistan presented above at the end of the 3rd millennium and the very beginning of the 2nd millennium
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With the help of correlations, we take into account the Mesopotamian-Sumerian immigration waves that can be detected multiple times and appear repeatedly and bring them into a reasonable connection with the above-mentioned contradiction, namely, how is it conceivable that the populations of southern Turkmenistan and northern Iran, which had fairly limited agricultural areas, were able to populate the vast steppe areas stretching from the Volga to the Altai in a relatively short time, while

We come to the conclusion that the Mesopotamian-Sumerian colonists, who were present in at least two waves in the settlements of northern Iran and southern Turkmenistan in the second half of the 3rd millennium and at the very beginning of the 2nd millennium, evidently only stayed temporarily in the mentioned region and, after a shorter or longer period of time, undoubtedly migrated further to the steppe of Western Asia, after they had to realize sooner or later that the agricultural yields of the oasis cultures of northern Iran and southern Turkmenistan were not sufficient to feed larger masses. Thus, northern Iran and southern Turkmenistan were evidently only temporary stations for the settler groups that occasionally arrived there from Mesopotamia: after they soon realized that their permanent livelihood was not assured there, they soon migrated even further north. This phenomenon, for which we have listed a number of specific archaeological evidences above, is, in our opinion, the most obvious explanation for the aforementioned demographic contradiction.

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if the closer and more distant connections of the Neolithic and Early Metal Age archaeological cultures of the Eastern European, Pontus-Dnieper-Don-Volga and Southern Ural regions in all their details and from the earliest beginnings to the Bronze Age and Chalcolithic cultures, because the development of Eastern European culture — similarly to the cultural situation in West Asia — was also determined by cultural influences and colonizations originating from the Pre-Asian primary agricultural and animal husbandry cultures. In the present case, of course, the influences of the Pre-Asian area closest to Eastern Europe, i.e. Transcaucasia, were the strongest. In Transcaucasia and the Urmia Lake area in the Neolithic and early Chalcolithic periods, between ca. 6000 and 4500 BC, we find independent cultures. After 4500, however, the large southern cultural circles that began to actively expand around that time increasingly influenced the local cultures. At first, the influences of the Syrian-northern Mesopotamian Tell Halaf cultural complex appeared, but around the turn of the 5th/4th millennium BC, the so-called northern Mesopotamian Eridu-Ubaid cultural circle began to emerge.

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The direct expansion of the "Ubaid" phase (Eridu-Ubaid 3-4th period (between ca. 4500 and 3800) also reached the Lake Urmia region. In parallel with this, the cultures around Lake Urmia themselves [9 expanded northwards. For example, very close parallels to the 5th millennium BC Dalma Tepe pottery near Lake Urmia were found at Ilanlı Tepe in northern Azerbaijan from the beginning of the 4th millennium BC. Also at ÿÿÿ] Tepe and also at the very beginning of the 4th millennium BC, the earliest appearance of the comb-impression decoration on clay vessels, according to our current archaeological knowledge, is 6. Burney-Lang 1975. pp. 80-81.). So the homeland of the decorative motif that later appeared in large areas of Eastern Europe and Western Asia (Dnieper-Donets culture, Tripoli culture, East-Minar culture), as well as in the Eurasian forest belt, was Transcaucasia. We have already mentioned above that comb pottery reached the Eastern Minyan culture from Transcaucasia: Anau I. 65 In the Namazga Tepe I period, close parallels to the Dalma Tepe were appeared as early as 3800-3700 (1. Hrouda 1971. pp. 57-58).

Throughout the 4th millennium, increasingly strong cultural influences arrived in the areas around Lake Urmia and other Transcaucasian regions from the south: initially from the northern Ubaid cultural circle, and then after 3800 from the Uruk-Gaura cultural complex. These have already been described above.
After such antecedents, at the very beginning of the last quarter of the 4th millennium, around 3250 BC, the unified early Transcaucasian culture (the Kura-Araxes culture of Soviet archaeology) developed in the Caucasus, the Upper Euphrates and Lake Urmia region, encompassing Transcaucasia, Eastern Anatolia and Northwestern Iran. The striking uniformity of the early Transcaucasian culture circle, which appears with completely identical artifact complexes over vast areas, cannot be explained by the simple further development of earlier, mosaic-like local cultural centers. The assumption of a common ethnic base seems to be the only acceptable solution. All indications are that at the beginning of the civilisation, around 3250 BC, a new population appeared and rapidly took possession of the entire area in question (1. Burney-Lang 1975. p. 100).
When examining the identity of this new population, we can only seriously consider two peoples. One is the Subar, Subur or Subir people, who often appear in Sumerian written records of the first half of the 3rd millennium, whose places of residence are called Subar-ki, Subur-ki or Subir-ki (in Hungarian, Subar-föld) by the relevant sources and are localized in Northern Mesopotamia and north of it. The other people is the Hurrian people, who appear more and more frequently in Mesopotamian written records starting from the second half of the 3rd millennium, and are mentioned in the same areas. According to our source data, these Hurrians appear in increasing numbers in the Akkadian period, between ca. 2450-2300 BC, in central Mesopotamia, mainly by the Akkadians.

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In the areas of the Ur-dynastic period, and during the III. Ur dynasty, around 2200-2100, more and more Hurrian personal names appear in written records in Southern Mesopotamia as well. These data definitely indicate that in the second half of the 3rd millennium we must reckon with a significant southward movement of Hurrian peoples, who undoubtedly started from the region formerly called Subar-land. It also seems very likely that these Hurrians were in some way closely connected with the Subar peoples previously also mentioned north of Mesopotamia, although it seems that we cannot speak of the identity of the two ethnic groups. Likewise, archaeological finds, namely the strikingly uniform complexes of finds of the early Transcaucasian culture found in the areas inhabited by the Subarcs and Hurrians, testify to the close relationship between the Subarcs and the Hurrians, as well as the fact that the Subarc-Hurrian area in question had been part of the Sumerian civilization in Mesopotamia since the middle of the 4th millennium, just as the areas inhabited by the Hurrians in the 3rd-2nd millennia were also organically part of the Sumerian culture.

Around 2300 BC, the third stage of early Transcaucasian civilization began. What is striking in this period is, on the one hand, the loosening of the previous cultural unity, and on the other hand, the rapid development of the metal industry. The types of weapons and other metal tools are completely identical to the Sumerian products of the same period. This phenomenon was the result of a new, even stronger direct Mesopotamian influence, which direct impulse is also evident in the Maykop and other kurgan burials in the Kuban region north of the Caucasus during this period. For example, in the Maykop princely burial dating from 2400-2300. The highly developed Mesopotamian Sumerian metal culture appears quite suddenly, without any transition, coupled with the types of bronze and precious metal finds found in the rich tombs of Alaca Hüyük in Anatolia, which in turn are ultimately also of Mesopotamian-A1-Sumerian origin. The Kuban-Maykop culture is by far the most developed cultural center north of the Caucasus in this period. It greatly surpasses the cultural development of the surrounding areas. In accordance with this outstanding level of culture, the archaeologically well-marked expansion of the Kuban-Maykop culture towards the north and northeast soon begins, one of the consequences of which is explained by research into the beginning of Ural ore mining already at the end of the 3rd millennium BC (I. Childe, 1960; Hanéar, 1956.).

Before we go any further, however, we must also outline the earlier stages of Eastern European cultural development in the 4th and 3rd millennia, so that we can then recognize the overall picture more clearly.
At the beginning of the 4th millennium BC, the Upper Paleolithic-Late Gravettian archaeological site was discovered in the eastern half of present-day Ukraine and the adjoining territory of southern Russia.
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We find the descendants of the eastern branch of the Paleolithic-Mesolithic fishing-hunting culture. Then, at the turn of the 4th millennium, in the area of the Dnieper rapids, on the Igren Peninsula, as well as on the Sursky and Sulayev Islands, near Melitopol and Zhitomir, primitive comb-pitted ceramics appear, with characteristic pointed-bottomed vessels and clay thinly fired with shell powder (I. László 1961. pp. 118-119.).
We already know that this method of vessel decoration first appeared at the beginning of the 4th millennium at Ilanlı Tepe in northern Azerbaijan, as a provincial variant of the pottery of the ancient local cultural circle around Urmia (Dalma Tepe, Yanik Tepe, etc.) (I. Burney-Lang 1975, p. 81). It is therefore obvious that it spread from there to the northern Pontic areas.
For us, this comb-pitted pottery is particularly important and interesting because in later times it gradually spread to the entire area of the Eastern European forest belt, where both the comb-pitted decoration method and the custom of thinning the clay with shell powder persisted for thousands of years in the culturally less developed northern regions (Fig. 1).
László 1961, p. 121).

The inhabitants of the aforementioned settlements in the North Pontus countryside, who used ceramics with comb-pit decoration, were not yet familiar with agriculture, and only in some places (e.g. Igren) can traces of the just beginning of animal husbandry be found. This culture was therefore still at a sub-neolithic stage (I. Müller-Karpe 1968, p. 165).
From these beginnings, around 3000 BC, the Dnieper-Donetsk culture developed, several synonymous names of which are used in the literature, which can often lead to very significant misunderstandings, as we saw above in the case of János Harmatta, who, without any further explanation, simply interpreted the Dnieper-Donetsk culture and the pit grave culture as two completely separate, simultaneous archaeological complexes, even though the two names partially overlap in terms of territory and follow each other in time. Therefore, in order to avoid misunderstandings, we will list the other names of the Dnieper-Donetsk culture again:
2. North Pontic culture; 3. Mariupol culture; 4. Early Ochre-grave culture; 5. Early Pit-grave culture; 6. Kurgan I culture (Gimbutas). — This culture represents the earliest beginnings of the Neolithic in the region east of the Dnieper, although the population still lived largely from fishing and hunting. Traces of agriculture and animal husbandry can only be detected in the western parts, closer to the Dnieper — i.e. the Tripolje culture in western Ukraine. Similarly, pottery is still very rare, and what is there is a later development of the earlier comb-pit sub-neolithic type (I. Müller-Karpe 1968, p. 166 and Müller-Karpe 1974, p. 358). The burials are characteristic: long, trench-like
In pits, placed on a thick layer of ochre, closely next to each other and partly on top of each other, several dozen skeletons are found in each pit, abundantly covered with ochre from above.
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(I. Müller-Karpe 1974. p. 358). This burial custom served as the basis for the names of the ochre-grave and pit-grave cultures. The people of the Dnieper-Donets culture belonged to the long-legged Cro-Magnon Paleo-European Em- bertani type.
Then, around 2500 BC, also in the eastern Ukrainian regions, but extending eastwards to the Volga, the late pit-grave culture (synonymous names: late ochre-grave culture, Kurgan culture, or in the separate terminology of M. Gimbutas, Kurgan II culture) emerged, which, however, despite the partial agreement on the geographical area, cannot be considered a further development of the Dnieper-Donetsk culture. Anthropological data prove that the development of the late pit-grave culture began around 2500 BC from the south.

was the result of the appearance of a new short-headed population. The newcomers partly completely assimilated the long-headed Paleo-Europeans found there in a short time, and partly pushed them further north. Accordingly, the burial rite also changed radically: in contrast to the mass graves of the Dnieper-Donetsk culture, only individual burials are found. Similarly, the ceramics are fundamentally different from the previous ones: the vessel types and decorative motifs of the neighboring agricultural cultures appear (Fig. 1). Childe, 1960; Hanéar, 1956.).

If we now take a closer look at these neighboring agricultural cultures just mentioned, we find the early Transcaucasian culture in the southern neighborhood of the late pit-grave culture, whose northern and northeastern expansion in the middle of the 3rd millennium is indicated by the finds of Lugovoe, Velikent, Kayakent and Mamaikutan on the western coast of the Caspian Sea, which in turn means that the early Transcaucasian culture had already roughly reached the Terek River line in the north around 2500. The western neighbor was the Cucuteni-Tripoli culture, which developed towards the end of the 4th millennium in Moldavia and Western Ukraine through the direct eastern and northeastern expansion of the Late Neolithic cultures of the Balkans and the Carpathian Basin.

In accordance with this geographical situation, the development and ethnic composition of the late pit-grave culture was primarily influenced by the early Transcaucasian culture, supplemented by certain Balkan-Trypological influences. — To the east of the Tripoli areas, the late pit-grave culture was the mature Neolithic culture in Eastern Europe (L. Müller-Karpe 1974. p. 358). In accordance with these newer observations and a more precise chronological classification, modern research sees the focus of the late pit-grave or kurgan culture in the Kuban-Maykop culture. We can take it as proven — states, for example, Müller-Karpe - that the Kuban culture is an agricultural, metal-using Copper Age cultural complex that appeared in the second half of the 3rd millennium in the region extending from the Caspian Sea and the Volga to Ukraine, i.e. the late pit-grave or

1058
===== PAGE 1047 =====

a decisive cultural role in the development and spread of the Kurgan culture 14152016. The metal objects, weapons, decorative motifs, jewelry and vessels found in the area of the culture were all brought to Southern Russia by the Kuban-Maykop hand. The same applies to the polished -6y axes (all 1. Müller-Karpe 1974.

355-356. old.). Müller-Karpe's last remark about polished stone axes is quite obviously a side-cut towards the supporters of the so-called "battle axe" theory. In Indo-Germanocentric prehistory research, since the end of the last century, the popular traditional idea has been repeatedly brought up, according to which the polished stone axes of Eastern Europe + the "battle axes" — are the archaeological remains of the ancient, still unified Indo-Germanic population. They are called *taxteute*), but also often "braided ceramic battle axes" (*schnurkeramische Streitaxteute*), since these stone axes often occur in the company of "battle axes" (*Streit-braided ceramics*). This hypothesis has only been supported in recent decades by the so-called "ahistorical" Indo-Germanic trend, but most renowned researchers classify it as completely absurd, as Müller-Karpe clearly states: "...whether the cord-decorated potters were Indo-Germanic belongs to those invented interpretations that extend beyond the boundaries of scientific knowledge..." (1. Müller-Karpe 1974, p. 485).

Part of the long-headed Paleo-European population of the Dnieper-Donets culture, at a sub-neolithic level of culture, migrated northwards after 2500 to avoid the short-headed colonists who arrived from the south and, settling in the forest belt among the fishing-hunting groups of the Paleolithic archaeological cultures of Kunda and Sibir, in all likelihood played a very important role in the ethnogenesis of the later Ural-Finno-Ugric peoples (L. László 1961. pp. 193-195). These would therefore be the late Swede groups of Gyula László, who migrated north in the mid-3rd millennium BC and there mingled with the proto-Lapp, proto-Samoyed and proto-Obi-Ugric ethnic groups of the Kunda and Sibir archaeological cultures. Archaeological signs of these northward migrations can be seen in the appearance of comb-pitted pottery in the Baltic, Upper Volga and Kama-Ural forest belt areas starting in the mid-3rd millennium.

To the north of the archaeological cultures known so far, roughly in the area of the Eastern European forest belt, before 2500, the aforementioned Kunda and Sibir fishing-hunting cultures were located: further west, in the Baltic and Upper Volga regions, the Kunda, and to the east, in the Kama-Ural region, the Sibir. Then, in the second half of the 3rd millennium and the first half of the 2nd millennium, we find various cultural groups of comb-pitted pottery throughout the entire area of the forest belt, from Scandinavia to Western Siberia, in Europe the

1059
===== PAGE 1048 =====

The most important are the Scandinavian, Baltic, Volga-Oka and Kaedma-Urdu main groups, which are divided into numerous subgroups. However, the relatively uniform pottery in the vast area is not a sign of some kind of ethnic affinity or even identity - as Finno-Ugric studies categorically claimed until recently, and which phenomenon was evaluated as the most important archaeological evidence for the hypothesis of the Ural-Finno-Ugric people and language family - but is quite obviously related to the general nature of the cultural circle in question: the bearers of the comb-pitted pottery culture circle were Mesolithic fishing-hunting groups of largely the same cultural level everywhere, who took over the pottery from the neighboring agricultural cultures to the south (see Müller-Karpe 1974, p. 363). The direct source of the forest-belt comb-pit pottery in Europe can be considered primarily the Dnieper-Donetsk culture (1. above), but this method of decoration is also found in other places in the steppe zone (L. László 1961. pp. 118-120.). As we already know, comb-pit pottery came to the forest belt in West Asia from the Kelteminari culture. Similarly, the copper and even bronze finds that appear here and there in the forest belt are of southern — Caucasian or sometimes Pontic — origin (1. Müller-Karpe 1974, p. 363.).

The population density of the forest belt fishing-hunting indigenous population was extremely low — due to their primitive farming methods —. Only one or two families lived in the scattered, tiny settlements — more precisely, hunting lodges — mostly located several dozen kilometers apart, but they also constantly changed their place of residence. Therefore, we cannot speak of a continuous, compact population in these areas in the times before the spread of agriculture, i.e. before about 2000 BC (L. below). The fundamental, theoretical significance of this fact, which can be proven both archaeologically and ethnographically, i.e. doubly clearly, is that under such circumstances, the 19th century hypotheses of a common Finno-Ugric, and even more so, of a Uralic core population and a base language of the language family — in the absence of a sufficiently significant population — literally dissolve into nothingness.

The Kuban-Maykop culture penetrated these backward areas at the end of the 3rd millennium. 616019161 Research attributes the discovery of the copper and gold mines in the Urals to them, as we have already mentioned. This is when the first metal tools appeared in the Southern Urals — for now only as scattered finds — and these metal objects are indisputably of the Kuban-Caucasian type.

Then, around the turn of the 3rd/2nd millennium and after — presumably also due to the attractive influence of the Ural ores, but most of all, undoubtedly due to the rapid population growth of the southern agricultural cultures — more and more southern settlers migrated to the South Urals, the Volga and the Ob on both sides of the Ural River6, along the Ob, Irtysh, Ufa, Volga, Oka and other smaller rivers.

1060
===== PAGE 1049 =====

to the region. The immigration took place in several waves and continued uninterruptedly until the middle of the 2nd millennium. The newcomers arrived in large numbers. In the western regions of the Volga, the lower Kama and the Urals, the Balanovo, Abasevo, Fatyanovo, Girendavasas and Andronovo cultures were established. They had a significant impact on the paleoethnography and culture of the Finno-Ugric peoples, including the Ugric-Hungarians. We cannot yet express a definite opinion about their language and ethnic composition. However, it is conceivable that they were originally ancient Iranians. — This is how Bartha Antal describes the mass southern colonization in the Ural-Kama region, which Finno-Ugricists — In his article "Society and Economy in Hungarian Prehistory" (Magyar Őstörtéti tálműnik, 1977), he refers to this "Finno-Ugrians" - who consider to be the Ural-Finno-Ugric homeland. He later adds that he means the indigenous fisher-hunters — within a short time, these southern settlers were almost completely assimilated.

Bartha's conclusions drawn from the fact and circumstances of this southern colonization are so unreasonable, even downright fantastic, that it will be instructive to investigate what could have prompted Bartha to put together the above-mentioned absurdities. Fortunately, however, we do not have to ponder this question for long, because we can soon notice the long-overhanging horse's leg: it can be traced and demonstrated point by point that the outdated 19th-century genetic language family theory of Finno-Ugric linguistics and other similar hypotheses that necessarily follow from this basic assumption forcibly prescribed to Bartha what he was allowed to assert and what he was not allowed to assert. For the sake of better clarity, we will summarize this chain of hypotheses, which is built on top of each other and is almost unimaginably illogical, and which is imperatively required from the perspective of Bartha's Finno-Ugric linguistics-driven prehistoric thinking.

1/ In the case of the southern colonists, one must assume that they were Old Iranian, so that the Iranian-Finno-Ugric and Iranian-Hungarian word correspondences, which Finno-Ugric linguistics considers to be "Old Iranian loanwords", can be explained not only linguistically, but also historically.
2/ One can only speak of diffuse, nebulous "powers" in the field of relations between Finno-Ugric peoples and southern settlers, since linguistics has only a few dozen - Harmatta, p. 53 (1. Harmatta 1977) -- records an Iranian-Finno-Ugric (+Hungarian) szönpár connection, which is undoubtedly too small to speak of more serious -- e.g. interethnic -- relations.

3/ The sparsely populated, primitive fishing-hunting natives - whom Bartha (only incidentally noted) already called with noble simplicity around 2000 BC, Finno-Ugric - calls it "Finno-Ugric", although the existence of "Finno-Ugric" around 2000 BC is also merely a purely linguistic reflection of Finno-Ugric studies.

===== PAGE 1050 =====

hypothesis established with the help of comparisons, for which there is no concrete evidence in the world — of course they had to assimilate the southern colonists who arrived en masse in several waves over half a millennium, and who were culturally towering above the level of education of the native fishermen and hunters, since in the opposite case — which is the only conceivable possibility under the given circumstances presented above — these "Finno-Ugric" people of Bartha's kind would have to speak the language of the southern settlers, i.e., according to Bartha, Iranian.

This would be the astonishing internal "logic" of Bartha's Finno-Ugric linguistically inspired prehistoric hypothesis-pyramid. However, in contrast to all this, it is hardly possible to speak of ancient Iranians at the turn of the 3rd/2nd millennium BC, given our current knowledge, because according to the new insights of modern Indo-Germanic studies, the Indo-Germanic languages do not form a genetic language family stemming from a single root, but only began to develop after the turn of the 3rd/2nd millennium BC — through the areal equalization of the different languages of several different neighboring ethnic groups, i.e. as an areal language alliance.

But apart from this, it is especially impossible to suspect ancient Iranians in the case of these colonists who arrived from the south in the Volga-Ural-Ob region in the first half of the 2nd millennium BC. International prehistoric research has shown more than 30 years ago that, for example, the Balanovo culture has very close connections with the Caucasus and even with Mesopotamia. G. Childe, F. Hanéar and other renowned researchers established as early as the 1950s that, for example, the Balanovo chariot models in their raw materials and structure perfectly correspond to the chariots used in the Mesopotamian cities of Ur and yyy in the first half of the 3rd millennium, but also to the

2400-1300 BC and the Transcaucasian chariots from the beginning of the 2nd millennium BC. In addition, the shape of the Balanovo axes, axes, spearheads, arrowheads, jewelry, certain types of ceramics, and even folkloristic features point to a Mesopotamian origin. The language of the present-day Chuvash, their legends, and folk customs are also full of Caucasian similarities. But anthropologically, the closest relatives are found in the ancestral races of Armenia and the Eastern Mediterranean. These series of close connections indicate very strong Transcaucasian and Proto-Asiatic-Mesopotamian connections, as well as immigration from these latter areas at the beginning of the 2nd millennium BC (all this 1. Childe, 1960; Childe, 1955; Hanéar, 1956).

We emphasize that these experts in ÖstörtEnct research demonstrated the above — with the help of in-depth motif-historical analyses, anthropological and ethnographic data, and through a thorough examination of stylistic parallels — as early as the 1950s, even though at that time the previously described massive and repeated waves of southern immigrants-1062

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The archaeological traces of the Balanovo people were largely undiscovered (for the Fentic see also Randa, 1954. 19). It is particularly important in this context that F. Hantar, alone, was able to predict in 1956 precisely from the nature of the Caucasian and Mesopotamian parallels of the Balanovo culture that the Balanovo population must have appeared in the Volga-Ural region at the beginning of the 2nd millennium BC (J. Hancar, 1956.).

It is all the more incomprehensible that Bartha does not even mention these serial Caucasian and Mesopotamian connections, although today we already know much more specific data than the aforementioned about the former residences and population of the southern settlers of the Volga-Ural-Ob region. We saw above, for example, that the migration of the short-headed Transcaucasian population northwards to the area of the Kuban-Maykop culture and the closely related late pit-grave culture began around 2500 BC. We also know that large areas in the Transcaucasian region were depopulated around and after the turn of the 3rd/2nd millennium. This process can be traced archaeologically well (1. Burney-Lang 1975, p. 174). This data leaves no doubt that the migrations from Transcaucasia to the north continued at the beginning of the 2nd millennium.

The surprising archaeological observation that the Kuban-Maykop culture, which arose suddenly and without precedents between about 2400-2300 BC - which, as we have seen, can be traced directly to massive Mesopotamian-Sumerian radiations - after barely 300 years of flourishing, around 2000 BC, as suddenly as it arose, again falls into gray mediocrity (1. yyyýyyy, 1937.) seems to be very important. This phenomenon can most obviously be explained by the further migration of the population group that brought and maintained the culture. The ore miners of the Kuban-Maykop culture were the first to explore the Ural Mountains at the end of the 3rd millennium (1. above), i.e. they had close ties with the more northern regions. We can therefore look for them as the first wave of southern colonists from the Volga-Ural region at the very beginning of the 2nd millennium.

Closely related to this ethnogenetic question is the anthropological identity of the populations of the late Pit Grave and Afanasyev cultures, which was already discussed above. We have seen that this "plump-Europid" anthropological type appeared only in southern Russia (late Pit Grave culture) and the Altai region (Afanasyev culture) in the second half of the 3rd millennium, but later, since the emergence of the Andronovo culture around 1800/1700 BC, it formed the dominant anthropological type of the entire Eurasian steppe zone. Anthropology calls this human race the Andronovo type. It is extremely noteworthy that this Andronovo human type played a significant role in the anthropological composition of the conquering Hungarians, and even occurs relatively frequently among the present-day Hungarian population. However, even more important is the fact that the

1063

===== PAGE 1052 =====

The Andronovo human type constitutes the basic type of the Turanid human race (1. Henkey, 1986./, which latter human race is by far the most frequently occurring anthropological type in Hungary.

If we now add to these anthropological data the fact that there are a large number of Mesopotamian-Sumerian correspondences in the finds of both the Afanasyev, the late pit grave and the Andronovo cultures, then we can have little doubt about the ultimate origin of the populations of these cultures: all signs point to the fact that this characteristic Andronovo human type originates from the pre-colonialists who migrated to the Eurasian steppes (1. Randa 1954; Childe, 1960; Hanéar, 1956. also). But the same is also attested by the migration processes leading to the development of the Andronovo culture, which are already sufficiently known and can be traced archaeologically. We know — as already mentioned above — that the Andronovo culture arose through the gradual coalescence of the Afanasyev, late pit grave cultures and the earlier outlined Volga-South Ural Early Bronze Age cultures (Balanovo, Abasevo, Fatyanovo, beam-frame cultures), while at the same time, in the first centuries of the 2nd millennium, the population continued to flow northward from the agricultural-metalworking cultural circles of southern Turkmenistan and northern Iran, about which we have already shown above that, according to all known data, there were intermarriages between them on the one hand in the Akkadian period and on the other hand in the 3rd millennium. During the anarchy following the fall of the Ur dynasty, the Sumerian ethnic groups that migrated from Mesopotamia first to Northern Iran and Southern Turkmenistan, and then from there, due to overpopulation there, were forced to migrate further to the steppe after a short time, either voluntarily or involuntarily, also played a very significant role.

But we must also reckon with smaller or larger Sumerian ethnic groups among the southern colonists who migrated north from Transcaucasia, from the Kuban-Maykop culture and the late pit-grave culture to the European side of the Ural River and along the Volga and Oka. For example, first of all, the culturally active, leading ethnic group of the population of the Kuban-Maykop culture, as evidenced by the series of very close Sumerian correspondences and even, in some cases, even identities of the Maykop finds, was in all probability largely of Sumerian origin. All signs point to the fact that this culturally active, leading Sumerian ethnic group then migrated further from the Maykop area to the Volga-South Ural region around 2000 BC, when the Kuban-Maykop culture, after 300 years of flourishing, suddenly became completely insignificant again. In addition to all this, we should also mention that in the area of early Transcaucasian civilization, around the turn of the 3rd/2nd millennium, new immigrants appeared in many places, who brought with them and generally spread the secret of making tin bronze,

1064

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which had hitherto been a Sumerian monopoly (1. Burney-Lang 1975, p. 138). Almost simultaneously, at the very beginning of the 2nd millennium, in Syria, in the northern Mesopotamia and in the northern regions of the Zagros Mountains, the technique and forms of metalworking tools become unified with surprising suddenness (1. Burney-Lang 1975, pp. 174 and 194). This can apparently be traced back to the fact that large numbers of metal casting masters and blacksmiths from some highly developed metalworking center, either voluntarily or under compulsion, migrated to the aforementioned areas (1. Burney-Lang 1975, pp. 194-195). This highly developed metalworking center, however, in the period under examination, around the turn of the 3rd/2nd millennium BC, could only have been Mesopotamia, where the general anarchy and complete uncertainty of the Isin-Larsa period reached its peak at this time (for more information, see our book 1, p. 789). It can therefore be practically certain that these metalworking masters and blacksmiths migrated from Mesopotamia to these northern, and in this period, as a result of the southern Hurrian migrations, to the areas already inhabited by the Hurrians, where they obviously had a satisfactory living as esteemed professionals. This conclusion is also supported by the striking phenomenon that also in the 3rd/2nd millennium BC. At the turn of the 2nd millennium, we can observe very strong cultural influences coming from the south, from the direction of the Urmia Lake basin (i.e. along the ancient main route leading from Mesopotamia to Transcaucasia), in both Armenia and Georgia (1. Burney-Lang 1975, p. 195). The authors do not understand why these southern cultural influences appear with such great intensity precisely in this period (1. Burney-Lang, 1975, p. 195), although a little later they themselves emphasize that at the beginning of the 2nd millennium the Urmia basin was "really saturated with Mesopotamian cultural elements" (1. Burney-Lang 1975, p. 202).

It seems beyond doubt that all these otherwise inexplicable phenomena can only be explained by assuming the immigration of Sumerian groups. Later, some parts of these Sumerian groups probably also participated in the settlement of the territories of southern Russia and the Volga-Ural region. As a final conclusion, we can state that even with our current, still rather incomplete archaeological knowledge, the decisive cultural role of the Pre-Asian, more specifically the Mesopotamian Sumerian and Transcaucasian Subar-Hurrian cultural circles, as well as the ultimately Mesopotamian-originated Kuban-Maikop, southern Turkmenistan and northern Iran agricultural-metalworking cultures, is becoming increasingly evident, not only in the Neolithic and early Feudal cultural development of the Volga-Ural region, but also of the entire Eurasian steppe.

Even more important than this is the fact that today we no longer only have indirect and direct pre-Asian cultural influences -1065

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We also know of repeated mass colonizations originating not only in the steppe, but also from Transcaucasia, Southern Turkmenistan and Northern Iran, among whose ethnic groups, according to the archaeological phenomena presented above, we must also count with a probability approaching certainty on the presence of a considerable number of Mesopotamian-Sumerian, or — to put it more cautiously — . . . szumeroid" ethnic groups.

All these huge population movements, which over the course of about 500 years filled the vast area stretching from the Donets to the Altai with ethnic groups of pre-Asian origin, and among which — as we have just seen — in several cases it is very likely that there were Mesopotamian ethnic groups speaking a Sumerian language, also clearly and clearly explain the surprising fact that even today, in East European and West Asian languages (Slavic, Ural-Finno-Ugric, Altaic, Iranian, etc.), there are still a large number of very close Sumerian phonetic and semantic parallels (cf. our book, pp. 512-522; 553-558; 631-638 and 639-645).

These Sumerian word correspondences (1. "Towards the Proto-Asian Proto-Language", Chapter 5) - which, incidentally, appear in hundreds in almost every West Eurasian language - have been classified by linguistic research working on the basis of "sound laws" and "regular sound shifts" methodology as "coincidental coincidences". However, in direct contrast to this view, we ourselves believe that such a large number, found in so many languages and so close linguistic parallels, as those that appear between Sumerian and modern West Eurasian languages, absolutely require a concrete historical explanation, and not just a superficial reference thrown in as an incidental, but a historical explanation that can be proven many thousand times over, for a realistic prehistoric interpretation of these truly surprising mass of Sumerian linguistic parallels. "coincidental coincidences." - What's more, we not only admit this, but have just presented a satisfactory archaeological evidence in every respect.

In connection with this question, however, we must also point out that these words just mentioned, which are found in the hundreds in most West Eurasian languages and have a very close phonetic parallel with the corresponding words of the Sumerian language, are precisely why "phonetic" linguistics declares them to be "coincidental coincidences" - because the phonemes of Sumerian words preserved in written records from 4000-5000 years ago are in the vast majority of cases still identical to, or very close to, the phonemes of words with the same meaning in modern languages. And such a thing, according to the linguistic dogma of "sound laws" and "regular sound shifts", is simply

1066
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impossible, because as a result of these alleged "sound laws" in the linguistic public consciousness, words are inexorably subjected to the "citol6d4s4" of consonants over time, by which they mean that, for example, a "b" lip sound must legally change to "p" after a certain time, i.e. "shift", and then this "p" must again become "1-16" after a certain 6, etc. In other words, this linguistic belief directly requires that 4000-5000-year-old words with the same meaning, if they are related to each other, cannot have identical phonemes.

In chapters 3, 4 and 5 of our book "Towards a Proto-Asiatic Proto-Language", we have shown in detail that these "sound laws" and "regular sound shifts" are not belong to the inner essence of languages, i.e. they cannot be considered "laws", but by all indications they primarily have a semantic role, namely signs of the enrichment and branching of word families, i.e. consequently, we obviously cannot explain the striking similarities and parallels between Sumerian and modern languages with the "sound law" method, so we must approach this question with a different, new perspective on the relationships between languages.

The areal language equalization theory of the relationship of the Uralic-Finno-Ugric languages
We have previously presented in three points related to the ideas of Antal Bartha the hopeless confusion into which the convulsive attachment to the outdated hypotheses of 19th-century Finno-Ugric studies constantly forces Hungarian prehistory research. We are convinced that there is only one way out of the exaggerated irrationalities revealed there. Namely, the one that Gyula László raised in his fundamental work "The Earliest Stages of Our History" in 1961 - very cautiously for now, only as a possibility - namely that the kinship relations between the Ural-Finno-Ugric languages - if we also take into account the relevant archaeological finds, and do not just stick to the ideas of linguists - can most likely be explained by the theory of a so-called "Ural-Finno-Ugric language chain" (1. above). This is the idea of language kinship interpreted as a language chain, which -- incidentally, it should be noted -- is practically identical to that of the brilliant Russian Indo-Germanist linguist, N.Sz.

The theory developed by Prince Trubetzkoy to explain the relationship between the Indo-Germanic languages and called "real language equalization", which, at the present stage of our knowledge, is by far the most consistent with the known linguistic, linguistic-historical, prehistorical data and, last but not least, with the ancient

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with concrete facts of linguistics (1. Trubetzkoy, 1939). It is also excellently suited to explain the linguistic relations between the southern colonists who arrived in the Volga-Ural region and the West Asian steppe in the first half of the 2nd millennium BC and the indigenous population there. And this without the need for even one of the previously described, unreasonable chain of hypotheses of Antal Bartha, who worked and thought in the field of Finno-Ugric studies, which professes the assumption of a genetically related Uralic-Finno-Ugric language family.
If we start from what could only be expected under the given circumstances in the Volga-Ural-Ob-Irtysh region in the first half of the 2nd millennium BC, namely that the very sparsely populated fishing-hunting indigenous groups settled among them and in their immediate vicinity, and were in regular contact with them on several levels, carefully counting certain elements of the language of the incomparably higher educated southern colonists who arrived en masse and in several waves, agricultural-animal husbandry and metalworking, they adopted about half a thousand words (about this many definitely verifiable Hungarian-Finno-Ugric word parallels are known) and the most important grammatical features - of course not unchanged, but more or less distorted, pidginized, adapted to the sound order of their own languages - and this pidgin-like language then became a widely used and generally understood lingua franca in everyday contacts. "French" among the fishing-hunting natives, then we do not need to suspect the questionable "ancient Iranians", we do not need to assume - as it were, "necessarily" - that the sparsely populated and very primitively culturally advanced fishing-hunting natives would have almost completely assimilated the southern colonists who had arrived en masse and in several waves over half a millennium, and who were also much more highly educated, the European colonists within a short time. - Let's think about it: this is like someone trying to prove with deadly seriousness that "we need to talk about their effects", but there is no need to be so blunt of the 18th and 19th centuries in North America would have been almost completely assimilated by the native Indians there within a short time.

which he then significantly developed in his book "Our History" published in 1981. Another idea is also involuntarily raised. Namely, that a large group settles "language chain" theory. This is how Gyula László formulated his new idea, on the many different languages, isolated populations, which: "... would spread out over a large area and speak its own unified language, and the basic elements of this language would be adopted separately by the indigenous people, who would also retain the peculiarities of the sound system of their language (they would apply the adopted vital elements), and would retain a significant part of their vocabulary (these would be words believed to have arisen in the "separate life" of the languages). I myself

1068
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I see this solution as probable, but I would also add that this language probably originates from the people who are still the largest in number - the Hungarians. (1. László 1981. p. 43)

We can notice --- and many of our readers must have noticed it immediately when reading the quote --- that the linguistic kinship approach shown in the above quote can be literally transferred and applied in every detail, without change, to the paleolinguistic situation that developed in the Volga-Ural-Ob-Irtis region in the first half of the 2nd millennium BC: "... a large group settles on the many different languages, on its isolated population... etc." (1. above).

In other words, in the first half of the 2nd millennium BC, a small number of fishing-hunting clan groups living more or less isolated from each other in the wooded steppe and northern forest belt regions of Eastern Europe and Western Asia, speaking separate languages, were settled during the massive and long-lasting southern colonization by a large number of groups --- not just one, but several successive waves --- who truly spoke a unified, ultimately negotiable language derived from the Sumerian linguistic stage of the Proto-Asian language koine, since these southern colonists arrived and settled in spread out over a large area, they spoke their more or less a wide strip extending from the Oka-Volga region to the Ob-Irtis tributary. As for the language of these southern colonists, according to all the data available to us, this language was the areally balanced lingua franca of the Andronovo civilization, which had a uniform, developed, excellent bronze culture, bronze exports, and long-distance trade, and was used throughout the entire area of the civilization, from the Altai to the Volga.

The paleolinguistic situation, therefore, is thus clarified in such a way that the elements taken over from the spoken language of the Andronovo culture obviously served as the basis for the mutual equalization of the languages of the forest-belt fishing-hunting groups living along the northern borders of the cultural circle, for the development of a "forest-belt lingua franca". In this view --- completely in the spirit of Gyula László's understanding --- the Hungarian language, or more precisely the language of the pre-Hungarians or Proto-Hungarians, cannot of course be classified among the languages of the forest-belt groups, i.e. among the later Ural-Finno-Ugric languages, but the direct antecedents of our language are undoubtedly to be sought in the area of the former spoken language of the Andronovo culture.

The theory of the Ugric-Hungarian language community
However, this areal language leveling theory of the kinship relationships of the Ural-Finno-Ugric languages is also excellently suited to the languages spoken in and around the Eurasian steppe, even on a much more global scale.

1069
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to explain the nature of the relationships between different languages. If we recall what we have said above about the circumstances of the development of the Andronovo culture, and about the earlier cultural circles that played a role in this formation process, as well as about their closer and more distant connections, their origin and ethnic composition, then we inevitably come to the conclusion that the results of the excavations of the last decades have practically brilliantly confirmed, down to the smallest details, the so-called "Ugric-Hungarian linguistic community theory" of some excellent and amazingly far-sighted Hungarian researchers of the turn of the last century (Géza Nagy, Károly Fiók and others), which was rejected at the time by the Hunfalvy and Budenz school, which proclaimed the exclusively Finno-Ugric origin of the Hungarian people and language, without any closer examination, and with the assumption

"officialized" Finno-Ugric-oriented Hungarian science has consistently remained silent ever since, because it is in direct contradiction to our Finno-Ugric origin"

Géza Nagy and his colleagues included in this "Ugric-Hungarian" language community group, in addition to Hungarian, the languages of the Ogurs or Ugrians, the Onogur-Bulgarians, the Sabirs and partly the Avars, the Huns, whose affiliation is practically unknown or unclear, as well as, with certain restrictions, the languages of the Scythian-Sarmatian-Szaka peoples of the 1st millennium BC. This was a completely obvious and reasonable conclusion in the light of the wide-ranging, global prehistory and linguistic history approach they represented (1. below) and in the light of the contemporary sources, and in the light of the Hunfalvy school, which at that time did not yet "expedient" and extremely one-sided "source criticisms". These ~~expedient and one-sided "source criticisms"~~ **came from the Avar-Altaic language speakers**, that proclaimed our Finno-Ugric origin through its exclusively

They were not called Turkic because of their Eastern Turkicness, but Ugric in the true ethnic sense of the word (cf. what is said about the change of the name "Ugric" on pages 270-272 of our book) or Ugric-Hungarian language group, and at the same time they were defined as being much older than the Finno-Ugric and Common Turkic languages, and were interpreted as an ancient linguistic base layer of the latter languages: the Hungarian parallels of the Finno-Ugric and Altaic languages, but also the correspondences between the Finno-Ugric and Altaic languages, that is, the so-called

The common elements of the "Uralo-Altaic" languages were explained by the strong linguistic influences of this ancient Ugric-Magyar language group in the north and east. According to them, It was from this Ugric-Hungarian language community that words and grammatical and structural features common to Hungarian were introduced into the Ural-Finno-Ugric languages. But likewise, the Hungarian grammatical and structural similarities and parallels found in the Turkish (and Mongolian) languages --- for there are plenty of such, but Finno-Ugric linguistics does not like to talk about them, since their mere existence is very strongly influenced by the

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questions the validity of the hypothesis of an independent "Ural-Finno-Ugric" language family considered in isolation (on this issue I, p. 649 of our book) -, and those words is that Finno-Ugric linguistics considers to be Turkic loanwords in Hungarian, according to the theory of Géza Nagy and others, actually entered the Türkic (and Mongolian) languages from the language of the former Ugric-Hungarian language community.

In other words, to use the words of modern linguistics, Géza Nagy and his colleagues actually explained the common features of the Hungarian, Uralic Finno-Ugric and Turkic Mongolian (Altan) languages as linguistic influences that caused the areal language equalization of these Ugric-Hungarian languages - thus anticipating Trubetzkoy and modern Indo-Germanic research by more than four decades.

561 went even further: the Iranian (at that time, due to the perception of Sanskrit as a real “Ősnyclv”) word parallels found in Hungarian and the Finno-Ugric languages, which Finno-Ugric linguistics naturally also interprets as loanwords of Iranian (or in the last century even more Oindic; — 1. above) origin in Hungarian and the Finno-Ugric languages (true to the fundamental methodological principle of Hungarian Finno-Ugric studies, according to which only Hungarian could borrow from other languages, but Hungarian loanwords should not be sought in other languages), were also interpreted as meaning that these words had once been transferred from the language of the Ugric-Hungarian language community to the Iranian (or Indic) languages (all this 1. e.g. Nagy: Ethnology and Linguistics; Nagy: The Prehistory of the Magyars; Nagy: The from the Scythians; Branch: History and criticism; Rohonyi: The history of the conquest.).

According to this theory, the Ugric-Hungarian language community was located in the southern regions of Eastern Europe and the adjoining region of Western Asia in the 2nd millennium BC, from west to east approximately between Eastern Ukraine and the Aral Sea, and from north to south between the forest belt and the Caucasus, as well as southern Turkmenistan. This area, however, corresponds almost exactly to the archaeologically revealed areas of contact between the two, both culturally and to the extent of the anthropologically closely related Early Bronze Age Eastern European and Western Asian 2nd millennium BC Regeszeti cultures (Balanovo, Abasevo culture, beam-frame culture in Eastern Europe; Andronovo culture in Western Asia), whose ethnicities migrated mostly from Transcaucasia, Southern Turkmenistan and Northern Iran to the Eurasian steppe in the centuries immediately preceding and following the turn of the 3rd and 2nd millennium BC. In other words: both the localization and the dating of the Ugric-Hungarian language community theory of Géza Nagy and his colleagues correspond exactly to the real archaeological situation in Eastern Europe and Western Asia in the 2nd millennium BC, which only became known much later.

1071
===== PAGE 1060 =====

However, the theory of a Ugric-Hungarian language community of Géza Nagy, Károly Fiók and their followers can be strongly supported not only archaeologically but also from the point of view of modern linguistics. The outlines of the paleolinguistic relations of Eastern Europe and Western Asia that can be outlined today point more and more definitely in the direction that these wonderfully insightful researchers of ours clearly recognized almost a century ago. The theory of areal language equalization, which has become increasingly popular in international linguistics in recent decades, and the increasingly precise understanding of historical connections through archaeological research, also confirm that in Eastern Europe and Western Asia we must seriously consider the Ugric-Hungarian language community of Géza Nagy, which played a dominant and determining role in the related areal language equalization processes of the Uralic-Finno-Ugric languages in the north and the Altaic-Turkic languages in the east, but it also appears to have been very strongly represented in the consolidation and areal unification of the more eastern Indo-Germanic language groups, primarily the Iranian, Indic, Slavic and Baltic languages. All available data indicate that this Ugric-Hungarian language community group was the so-called “A-complex”, which modern areal-based Indo-Germanic studies have determined to be close to the Ural-Finno-Ugric and Altaic languages, and undoubtedly formed one of the very important linguistic components in the consolidation of the ancient Indo-Germanic language community in the 2nd millennium BC, which was formed through the areal unification of several - at least two or three - different linguistic components.

Therefore, regarding the role played by the spoken language of the Andronovo culture (and the related Eastern European Early Bronze Age cultures: Balanovo, Abasevo, and the Beam-Frame culture) in the development of the more eastern Indo-Germanic languages (Iranian, Indic, Slavic, Baltic), in order to better understand this issue, we need to deal in more detail with the already mentioned theory of areal language equalization, developed by Prince Trubetzkoy and subsequently further developed by numerous internationally renowned Indo-Germanic linguists, according to which the most ancient Indo-Germanic languages arose in the contact zones of the Ural-Altaic — more specifically, primarily the Finno-Ugric —, Caucasian, and Semitic languages, or in the linguistically immature regions between the places of residence of the peoples speaking these languages (roughly in the areas between Ukraine and the North Sea). They developed gradually, through the mutual equalization of the languages of ethnic groups that had been in close neighborly relations with each other for a long time and originally spoke different languages.

This theory then has very far-reaching further consequences, for example, that according to Trubetzkoy's thesis, the origin of the Indo-Germanic languages cannot be considered a single, temporally narrow event.

===== PAGE 1061 =====

events that took place between them, but their formation constitutes a constant process. According to Trubetzkoy's criteria, a certain language can become “Indo-Germanic” at any time under given circumstances, but it can also lose its “Indo-Germanic” character due to opposite influences. Trubetzkoy also noticed that the sound formation of Indo-Germanic languages in each case depends on the other linguistic environment in which the peoples who later became Indo-Germanic speakers once lived. Ossetian, ýy- mény, Kurdish and Ind, for example, form their final sounds in three ways 4 Trubetzkoy —, because three-part slur articulation is also found in the languages in their environment. The other Indo-Germanic languages, on the other hand, form these sounds in only two ways, because only two types of final sound formation occur in the Ural-Altaic languages spoken among them. The same phenomenon can be observed in guttural sounds, as well as in several other cases (1. Trubetzkoy 1939; — Trubetzkoy's further findings have already been described in our book, pp. 94-96 and 408-418.).

Indo-Germanic linguistics, following in Trubetzkoy's footsteps, then also showed that the process of areal language unification that led to the development of the earliest Indo-Germanic languages took place relatively late, only starting at the beginning of the 2nd millennium BC (l. e.g. Nehring: Die Problematik der Indogermanenforschung 1954; in Scherer: Die Urheimat der Indogermanen 1968, p. 399; P. Bosch-Gimpera: Die Indoeuropäer 1961; in Scherer: Die Urheimat der Indogermanen 1968, p. 511-512).

At this point, it is absolutely outstanding that, as we saw above, Géza Nagy and his colleagues also dated the Ugric-Hungarian language community to the same period, the 2nd millennium BC, i.e. the agreement between modern Indo-Germanic studies and the theory of the Ugric-Hungarian language community is indeed very striking. However, from the point of view of our current question, the most important moment is undoubtedly the finding of the newer Indo-Germanic linguistics that in the development of the Indo-Germanic languages through areal language equalization processes, the so-called “Finno-Ugric” languages played a decisive role, a circumstance that - as we saw earlier - was already pointed out with emphasis by Trubetzkoy in his 1939 debate-initiating study. Later, this complex of questions was also elaborated in more detail. Thus, for example, according to A. Nehring, in the development of the Indo-Germanic languages and peoples, we are in all probability faced with the mixing of the languages and partly the ethnicities of a “Uralic” or Inner Asian and a Caucasian or Mediterranean population, which paleolinguistic and ethnogenetic process occurred at the earliest in the 3rd/2nd millennium BC. It may have occurred around the turn of the 10th millennium, somewhere in the vicinity of the Caspian Sea (1. Nehring 1954; in Scherer 1968, p. 399). This theory is well-supported by

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explains the numerous Finno-Ugric and Caucasian connections of the Indo-Germanic languages, continues Nehring. In the case of the also very significant Semitic-Indo-Germanic linguistic correspondences, however, according to Nehring, we cannot speak directly of geographical proximity and contacts, and therefore the connections connecting the Indo-Germanic languages with the Semitic languages. The source of these parallels should probably be sought in a third, unknown language, but in such a way that we should not exclude the Finno-Ugric-Indo-Germanic similarities from this complex of questions a priori, without prior thorough examination; it is easily possible that these also originate from the same unknown language — or languages (1). Nehring 1954; in Scherer 1968, 403. old.). According to Nehring, this “third, unknown language” was not only the source of the Semitic-Indo-Germanic linguistic parallels, but we can also probably look for the ultimate origin of the Finno-Ugric-Indo-Germanic linguistic correspondences in this same unknown ancient language, which played an important role in the areal language equalization processes of the Indo-Germanic languages around the Caspian Sea around and after the turn of the 3rd/2nd millennium BC. In other words, Nehring's investigations in this direction led to the result that in the region around the Caspian Sea in the 3rd/2nd millennium BC. Around the turn of the 2nd millennium, there must have been an ancient linguistic community that was in areal linguistic relations with the Finno-Ugric languages on the one hand and with the Semitic languages on the other, and as a result, it contained all those linguistic elements and passed them on to the emerging pre-Indo-Germanic languages over time, which then appear to us today as Finno-Ugric-Indo-Germanic or Semitic-Indo-Germanic correspondences in the consolidated Indo-Germanic languages.

That is, in Hungarian terms, this “third, unknown language” of Nehring can obviously be nothing other than exactly the same ancient linguistic state that Géza Nagy and his followers called the “Ugric-Hungarian language community”, and we ourselves identified above with a probability approaching certainty with the secondary areal-equalized communicative language of the Andronovo cultural circle based on the Sumerian linguistic level of the Proto-Asiatic linguistic koine, which, in turn, was in intensive areal linguistic relations both with the local languages of the small fishing-hunting ethnic groups of the northern forest belt and with the Altaic languages neighboring to the east and dominantly influenced the linguistic unification of all these mentioned ethnicities, brought about the creation of a “forest lingua franca” (which was then interpreted by 19th century linguistics as the “basic language of the Finno-Ugric language family”), and likewise the consolidation of a spoken Altaic language, and on the other hand - since the lion's share of the population of the Andronovo cultural circle is Pre-Asian, and even a significant proportion is Mesopotamian

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It was formed by settlers of mixed origin = in addition to Sumerian, it must have undoubtedly also contained a very strong Akkadian-Semitic linguistic component, since the dialects of the colonists in question, from whose group the Andronovo language was blended, according to the archaeological finds presented in more detail above, left Mesopotamia in the Akkadian, Gutian and Isin-Larsa periods, i.e. in times when the Sumerian language was already in real symbiosis with the Semitic language of the Akkadians.

J. Pokorny himself, the co-editor of Walde-Pokorny's “Vergleichendes Wörterbuch der indogermanischen Sprachen”, the still most famous Indo-Germanic etymological dictionary, showed as early as 1936 that Russian, but also other Slavic In his dissertation “Substrattheorie und die Urheimat der Indogermanen”, examining the foreign-language substrata of the Indo-Germanic languages languages, contain many Ural-Altaic, but primarily Finno-Ugric, features in inflection, word formation and internal linguistic forms; lively verb formation, accumulation of inflections, rich further formation of verbs, etc. This basic layer north and east of Moscow is demonstrably Finno-Ugric, writes Pokorny. The same Finno-Ugric linguistic influence can be observed in the Baltic languages. In fact, Pokorny emphasizes, FN Finck pointed it out as early as 1899 (Der deutsche Sprachbau als

Ausdruck deutscher Weltanschauung) that in addition to the Slavic languages, the Indic, Iranian, Ossetian, Kurdish and Armenian languages also developed special features that unmistakably point to the influence of the Ural- Altaic languages.

Let us pay close attention: these facts can again only be explained by the effects of Géza Nagy's Ugric-Hungarian language community on the eastern Indo-Germanic languages just listed, since a cursory glance at the map immediately reveals that each of the Indo-Germanic languages mentioned is located in the immediate western, southwestern, southern and southeastern neighborhood of the Ugric-Hungarian language community postulated by Géza Nagy and his followers — that is, according to our current knowledge, the spoken language of the Andronovo cultural circle. It is also very important in this context that the "official" Finno-Ugric language family theory, due to the great geographical distance, cannot in any way explain the Ural-Finno-Ugric linguistic characteristics found in the Indic, Iranian, Ossetian, Kurdish and Armenian languages,

Having reached this point, we must definitely deal more fully with the real nature of the "Finno-Ugric" or "Ural-Altai" linguistic influences mentioned by foreign linguists - which played an important role in the areal equalization processes of the Indo-Germanic languages, since this usage of the term can very easily be understood by Hungarian readers as if the "Finno-Ugric" or the "Ural-Altai" language families would have existed already when the Indo-Germanic languages - the modern

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In the sense of the areal linguistic equilibration theory - they were still in the process of formation. After all, we are always talking about "Finno-Ugric" or "Uralic" or "Altai" linguistic influences in these cases. - Regarding this issue, we refer to pages 425-426 of our book, where we have shown in detail that such a thing is of course out of the question, but that foreign Indo-Germanist linguists quite simply speak of "Finno-Ugric", "Uralic" or "Altai" proto-linguistic influences in connection with the areal linguistic unification-like development of the Indo-Germanic languages because - not being their field of expertise - they do not know exhaustively, in all their details, the 19th-century methods that still prevail in the ranks of Finno-Ugric studies today. Thus, they cannot know — and Finno-Ugric scholars are vigilantly careful not to know — that the 8,000 and 6,000-year-old ages of the Uralic and Finno-Ugric "basic languages", which dates were once literally "attached" to the 19th-century Indo-Germanic language family chronology — which at that time was believed to be even older, about 8-10,000 years old — because otherwise there was no evidence in the world regarding the dating of the a priori assumed "Uralo-Finno-Ugric language family", are still unchangedly proclaimed by Finno-Ugric scholars today.

This deplorable situation arises from the fact that the Ural-Finno-Ugric languages — apart from the only Hungarian, whose earliest written records date back at least a millennium — have practically no linguistic monuments in the true sense. Thus, when establishing the hypothesis of the Uralic-Finno-Ugric language family, Finno-Ugric studies simply did not have any internal "handle" at their disposal, for which, in the assumed "Uralic" and "synonymous" situation, there was nothing left for Finno-Ugric linguists to do but to adapt the chronological "Finno-Ugric" could have been compared in age. In this case, the order of the Uralic-Finno-Ugric language family to those words that were quite generally widespread in the Finno-Ugric languages, which also had parallels in many Indo-Germanic languages, and which, according to the results of the 19th-century reconstructions of the proto-linguistics of Indo-Germanic studies, would have belonged partly to the still undivided Indo-Germanic base language, and partly to the common vocabulary of the already separate Indo-Iranian or Proto-Iranian linguistic stage, and would have been included as loanwords in the still unified Uralic and Finno-Ugric base languages (for more details | pp. 399-400 of our book).

In other words, the entire Ural-Finno-Ugric language family chronology is a function of the contemporary Indo-Germanic language family chronology. This means that if modern Indo-Germanic studies date the beginnings of the development of the Indo-Germanic languages not to 8-10,000 years ago, but only to the very beginning of the 2nd millennium BC, then it is also evident that in this process of language equalization we cannot speak of any ancient "Finno-Ugric" or "Uralic" basic language influences, since Uralic and Finno-Ugric

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The chronological order of the basic languages of a language family stands or falls with the currently valid Indo-Germanic temporal order. In other words: if we were to accept the Finno-Ugric idea for the time being, and remain with the genetic language family hypothesis of the Uralic-Finno-Ugric languages, then > since, according to the new areal language leveling theory of Indo-Germanic studies, we can only speak of some kind of common Indo-Germanic areal language alliance at the beginning of the 2nd millennium BC (which roughly corresponds to the "language family base language" of the old genetic language family theories), and then after its disintegration, i.e. in no case earlier than the middle of the 2nd millennium BC, about the beginnings of the formation of independent individual group languages (such as Indo-Iranian and then Proto-Iranian). However, in close parallel with this new Indo-Germanic chronology, we must necessarily bring the age of the so-called "Finno-Ugric base language" down to at least the middle of the 2nd millennium BC, but rather even later than this, since we saw above that the Ural-Finno-Ugric language family chronology is based solely on the assumption that a series of Proto-Iranian words were transferred as loanwords to the languages of the Finno-Ugric peoples when the latter were supposedly still living together during the time of the so-called "Finno-Ugric basic language".

Foreign Indo-Germanic linguists therefore speak because "Uralic" and "Finno-Ugric" or "Uralic" basic language influences in the areal language equalization processes of the Indo-Germanic languages, "Finno-Ugric" language family's fantastic 19th-century chronology, which Finno-Ugric studies has just presented as an epoch-making It is still being promoted today without any regard for Indo-Germanic insights or changes in perspective, and is adopted in good faith without thorough scrutiny. In other words; in the case of the "Uralic" or "Finno-Ugric" linguistic influences on the development of the Indo-Germanic languages in the 2nd millennium BC, we are not talking about the effects of the "Uralic" or "Finno-Ugric" basic language, but about the massive linguistic emanations of the same ancient steppe language community, which at that time also superimposed itself on the various languages of the fishing-hunting peoples of the Eastern European and Western Asian forest belts, and dominantly influenced their language equalization processes, we could say making these northern languages "Uralic" or "Finno-Ugric". This ancient steppe language community could only have been the Ugric-Hungarian language group of Géza Nagy, about which we have already shown above that, according to all the relevant data currently available to us, the Andronovo cultural circle was familiar with the lingua franca. same.

This is very likely also supported by the previously mentioned theory of A. Nehring, according to which the origin of the parallels connecting the Indo-Germanic languages with the Semitic languages can probably be sought in the same ancient language, which is also the language of the Indo-Germanic language.

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It was also a source of Man-Finno-Ugric linguistic correspondences. It is very important in this context that the spoken language of the Andronovo civilization also meets this criterion, the "lingua franca", on the one first moved to Northern Iran and Southern hand, according to all indications, mainly Akkadian, Gutian and Isin-Messemian from East Asia, and even to a large extent directly from Mesopotamia, since this language Turkmenistan in several waves during the Larsa period (between approx. 2400-1900 BC), and then developed through the mutual equalization of the East Asiatic dialects of the colonists who migrated from there to the steppe, and among these settlers, especially during the Gutian rule and in the case of the groups that migrated in the Isin-Larsa period, there were very likely Akkadian ethnic elements speaking a Semitic language. But anyway, the Akkadian language itself is also full of Sumerian loanwords and other Sumerian linguistic elements, and even during the Sumerian-Semitic coexistence in Mesopotamia, around the turn of the 3rd/2nd millennium BC and after, the two languages actually mixed with each other, so in this way the communication language of the steppe colonists, which was undoubtedly primarily Sumerian in nature, could also have conveyed a significant amount of Akkadian-Semitic linguistic influences on the peoples living in the vicinity of the Andronovo areas. In his later works (1.ŷŷ. Die Träger der Kultur der Jungsteinzeit und die Indogermanenfrage, 1949.) J. Pokorny already firmly states that the Indo-Germanic languages were formed from the mixing of at least two languages with completely different structural structures, and that one of the components that played a role in their formation was demonstrably Finno-Ugric. Thus, for example, the Indo-Germanic tense system of the Germans and Slavs was broken down under Finno-Ugric influence, he states. The development of the so-called "satem" languages can also be explained by the sound system of the Finno-Ugric languages, which is characterized by hissing sounds, while these sounds were originally absent from the ancient Indo-Germanic languages. "The signs therefore point to," Pokorny concludes his arguments in this regard, "that Indo-Germanicity is also the result of the most diverse ethnology." (1. Pokorny 1949; in Scherer: Die Urheimat der Indogermanen 1968, p. 311).

W. Schmidt In his study "The Origin of the Indo-Germanic Peoples and Their First Appearance in Europe" (Kosmos 45, 1949), he reports on the emerging field of Indo-Germanic studies. also discusses new trends in linguistics. He correctly describes, for example, CC Uhlenbeck's theory that the Proto-Indo-Germanic language arose from two components: 1./ "A-Complex", which was close to the Uralic and Altaic languages. 2./ "B-Complex", which can be associated with the Caucasian languages, similar studies, E. Forrer — Schmidt continues his discussions in this direction — and came to the conclusion that who is deeply involved in the research of ancient Asia Minor languages. He is conducting Hittite (actually 1078
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languages of the Kanesi (or more precisely, Nesi), Luwian and Tabal (Palai) cannot yet be considered as real, fully developed Indo-Germanic languages, but rather as early stages of a language development process moving towards Indo-Germanization. Forrer sees in the Finno-Ugric languages also such an early - but even older - "prc-Indo-Germanic" or "peri-Indo-Germanic" linguistic state, which also played a role in the development of the Indo-Germanic languages. Schmidt notes that although he disagrees with Uhlenbeck and Forrer's theses on some points, he nevertheless emphasizes that both theories represent a very promising new direction of research.

After this somewhat lengthy but absolutely necessary 211676, let us return to the examination of other issues arising in close connection with the theory of the Ugric-Hungarian language community. First of all, we would like to emphasize once again that upon closer inspection, the extremely significant fact soon becomes clear that the theory of the Ugric-Hungarian language community fits precisely, almost down to the smallest detail, into the picture of prehistory and population movements that we have presented above based on the excavation results of recent decades. The area of the Ugric-Hungarian language community defined by Géza Nagy and his colleagues (let us think about it: at the end of the 19th century, when the Ugric-Hungarian language community thesis was established, nothing was known about the archaeological finds of the 2nd millennium BC in Eastern Europe and Western Asia, and only some scattered references in ancient, Greco-Roman sources, as well as one or two cuneiform texts already known at that time, could be used to deduce the prehistoric situation there, but even that only goes back to the first half of the 1st millennium BC) and the massive linguistic effects postulated by them on the language groups in its surroundings, as we have already mentioned, are almost 100% identical to the extent of the Andronovo, Balanovo, Abasevo and Girendafrazaz cultural complex that has been known in the meantime and cultural, and with a very high probability, linguistic historical role that acts towards areal language equalization. In his study "History and Criticism", he explained that the Ugric-Hungarian language community was a language of Eastern Europe and the Aral Sea. Exactly the same as, for example, Károly Fiók,

Due to its central position between the 1st and 2nd millennium BC, it could easily transmit its linguistic influences in all directions: both towards the Finno-Ugric peoples living in its immediate northern neighbourhood, and towards the Turkic peoples joining it in the east, but also towards the Iranians (and Indics) living not far to the south and southeast.

In other words, to put it in the language of our current archaeological knowledge: in the Eurasian steppe areas in the 2nd millennium BC, at a uniquely outstanding cultural level at that time, both in terms of culture and

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anthropologically completely uniform 8001080961 culture and the communicative language used by the aforementioned "plump-Europid" Andronovo-type populations of the aforementioned Eastern European regional cultures organically associated with it (the former existence of which is evidenced by the high development of the Andronovo cultural complex, its artistic bronze artefacts of a high standard, even for export, its well-developed long-distance trade network and last but not least by the very likely existence of state-like tribal confederations extending over vast areas, since all these listed phenomena presuppose the existence of a common language), which was spoken or at least understood from the Altai Mountains to Eastern Europe, and which ultimately — naturally through complicated transfers and repeated processes of language mixing and language equalisation — undoubtedly became the communicative language of the Neolithic-Early Metal Age Pre-Asian linguistic koine, language, i.e., in the period in question, it quite obviously originated from its Sumerian — or at least very strongly Sumerian — linguistic stage, since we have come to know both the "plump-Europid" type of the Andronovo and Eastern European population, and the lion's share of the elements of culture as both Pre-Asian, and even for the most part directly Mesopotamian-Sumerian derivatives. So, according to all known data that can be counted, this Andronovo spoken language had a decisive influence not only on the cultural development of the primitive economic and cultural groups of the surrounding regions, but also on their languages. That is, under such circumstances, it is completely reasonable and obvious that both the Hungarian verbal and grammatical correspondences found in the Finno-Ugric languages, both the so-called "Turkish loanwords" of the Hungarian language, and the words of Iranian origin

recorded as loanwords: Iranian-Hungarian (Finno-Ugric) word parallels can be interpreted very reassuringly and without any effort as linguistic elements originating from the former Andronovo Communicative Language in the languages of the aforementioned peoples.

Having reached this point, in order to avoid possible misunderstandings or subsequent misinterpretations, we would like to emphasize that — although some early pre-form of the later Hungarian language played a very important role in this global West Asian-East European areal language equalization process, according to all the data available to us, we cannot of course consider our language as some kind of "spiritus rector" that would have determined or directed the convergent paleolinguistic development outlined above, acting in the direction of language equalization. It is even less possible to claim that the Andronovo Communicative Language was perhaps identical to Hungarian, or, to be more precise, that the Hungarian language was an intermediate form of the 2nd millennium BC.

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with no predecessor, that is, that the previously discussed Hungarian-Finno-Ugric, Hungarian-Turkic and Hungarian-Iranian loanwords would be loanwords of "Hungarian origin" in the aforementioned languages. No, for now, the only thing that can be taken for certain is that the direct predecessor of the Hungarian language in the Andronovo period did not belong to either the Finno-Ugric, Turkish or Iranian languages, but was undoubtedly related to the Andronovo spoken language, probably representing one of its dialects, one could say a provincial version. This is also supported by the fact that - as already mentioned - the Andronovo human type still occurs relatively often among the Hungarians today, and that the Turanid human race, which is by far the most common Hungarian anthropological type, for its part developed from the Andronovo human type.

Returning to the question of the processes of areal language equalization in West Asia and East Europe that apparently took place in the Andronovo period, this theory becomes particularly convincing if we also take into account that both the Hungarian words attributed to Finno-Ugric origin by Finno-Ugric studies and the so-called , and language, i.e. precisely the language from which, according to all the relevant Turkish"

In the case of our "Iranian" loanwords, an extremely high proportion of these word correspondences have very close phonetic and semantic parallels in Sumerian.

archaeological and prehistoric data currently available to us, the spoken language of the Andronovo civilization ultimately originated. The Sumerian parallels of Hungarian-Finno-Ugric word correspondences are discussed in our book on pages 513-523. pages, where in a representative cross-section, out of 61 randomly selected Hungarian words of Uralic, Finno-Ugric or Ugric origin (from the headword "hön" to "kés") in the second volume of the Hungarian-Finno-Ugric etymological dictionary by Lakó-Rédei, we found a very close Sumerian correspondence in phonetic and semantic terms for 51, or 83.3%, of the 61 words chosen at random, consecutive in alphabetical order. It is also worth mentioning that in this comparison we could also notice that the phonetic and semantic forms of the parallel Sumerian words are in the vast majority of cases closer to the phonetic and semantic forms of the corresponding modern Hungarian words than to the parallel Finno-Ugric words. This circumstance, however, proves beyond dispute that the Sumerian words in question were first introduced into Hungarian, or rather into the corresponding pre-form of Hungarian of the corresponding age. and only after that from Hungarian to the Finno-Ugric languages. This again only supports the thesis that the pre-form of the Hungarian language in the 2nd millennium BC should not be sought among the Finno-Ugric languages, but to the south of them, that is, obviously within the Andronovian cultural circle,

It was written by Professor Harmatta in his paper "Iranians and Finno-Ugric peoples, Iranians and Hungarians" (1, Hungarian Prehistoric Studies, 1977).

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Among the 53 Irani-Hungarian (Finno-Ugric) words listed as loanwords of ancient Siranic origin, we identified obvious close Sumerian parallels in 39 words, which represents 73.6% of the 53 words (pp. 553-558 of our book).

Then, earlier in this book, we examined Harmatta's compilation of a different composition, consisting of 28 proto-Iranian and Oirani words, from this perspective, which Harmatta also discusses as loanwords of proto-Iranian origin that have been transferred to the Finno-Ugric languages and Hungarian. In this case, the result was that 16 of the 28 words , i.e. 57.1% of the words have a very close regular phonetic and semantic correspondence in Sumerian, another 5 words (17.9%) have either an uncertain phonetic parallel in Sumerian, or the correspondence in meaning can only be demonstrated in a figurative sense, while the remaining 7 words (25%) have no correspondence in the Sumerian vocabulary known today. The certain and probable Sumerian parallels therefore total 75% in this case as well. It is absolutely necessary to point out here that due to the already well-known paleolinguistic situation that developed on the Eurasian steppe during the 2nd millennium BC, it is very likely that the 17.9% uncertain or partial Sumerian correspondences can also be classified as obvious parallels.

Finally, among the 300 or so Hungarian words interpreted by Hungarian linguistics as loanwords of Turkish origin, we compared first 21 so-called "characteristic" words in two groups, and then 40 so-called CSUVASOS "common Turkish phonetic" words, with words known from Sumerian linguistic monuments, and in both groups we found 95-95% very close Sumerian phonetic and semantic parallels (pp. 631-636 and 637-644 of our book).

The presented areal language equalization theory therefore clearly provides a much more reasonable explanation for the interpretation of the kinship relations of the Finno-Ugric languages than the genetic Finno-Ugric language family hypothesis, which has been stubbornly enforced for a century and a half, and moreover, it provides a much more obvious, linguistically more convincing and global interpretation of the real origin of the Hungarian words considered to be loanwords of Turkish and Iranian origin. Moreover, it also has the invaluable advantage of fitting perfectly into the complex network of archaeological correlations known today, down to the smallest details and without any effort.

The development, formation and further development of prehistoric and linguistic relations in the grasslands of West Asia and East Europe, roughly from the last centuries of the 3rd millennium BC to the middle of the 1st millennium BC, or in the case of Finno-Ugric languages, from the 1st millennium AD to the 700s.

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Based on our current knowledge, it can most likely be outlined as follows:
First degree

In the last centuries of the 3rd millennium BC, we can only sense the earliest beginnings of the Ugric-Hungarian language community of Gézá Nagy, namely in Eastern Europe in the Kuban-Maikop and Late Soi pit grave cultures of Transcaucasian origin, and in Asia in the South Turkmen civilizations, also of Pre-Asian, largely directly Mesopotamian origin, and in their Altai derivative, the Afanasyev culture, the populations of which cultural complexes were — as we already know — anthropologically uniform (plump-Europids"). Presumably, different communicative languages were used in each of the archaeological cultures just mentioned, which, however, must have been quite close to each other, since the vast majority of the populations of the civilizations in question consisted of colonists of Pre-Asian origin, who undoubtedly brought with them the ancient Pre-Asian linguistic koine. the lingua franca of the linguistic level of the time, which was used at the time of their migration from East Asia. The East Asiatic colonists under consideration here — as we have shown above — all, without exception, left East Asia during the period of the Sumerian high culture, during the cultural, ethnic and linguistic hegemony of the Sumerians in East Asia, and moreover — at least the colonists who appeared in several waves in southern Turkmenistan in the 3rd millennium — were, by all indications, mostly descended directly from Mesopotamia. Consequently, the lingua franca used in their circles must have been based, beyond dispute, on the Sumerian linguistic level of the East Asiatic language koine.

Second degree

In the first half of the 2nd millennium BC, on the one hand, with the development of the Early Bronze Age cultures of the Volga-Ural region (Balanovo, Abasevo, etc.), and most importantly, with the development of the Andronovo civilization, which was completely unified in terms of anthropology and culture, around 1800/1700, the Ugric-Hungarian linguistic community became concretely tangible: practically speaking, there can be little doubt that the Andronovo civilization, which had a completely unified appearance over vast areas, which - we emphasize again, because it is a circumstance of paramount importance - had a well-developed long-distance trade network according to archaeological finds, and which possessed artistically high-quality bronze art, which was even produced for export, and moreover, during its existence, we must in all probability also count on state-like tribal confederations of enormous extent, extending from the Altai to the Volga, absolutely had to have a common language, which the educated-1083

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This Andronové lingua franca, however, was by all indications identical to the Ugric-Hungarian linguistic community of Gézá Nagy and his family. In this language, the previously mentioned different, but ultimately all smaller regional, lingua franca-es of the Transcaucasian, Kuban-Maykop, Late Pit Grave, Volga-Ural, South Turkmen and Afaneshevo cultures, derived from the later linguistic stages of the Proto-Asian linguistic koine — the 4th and especially the 3rd millennium, i.e. already indisputably Sumerian-dominated — mutually balanced each other.

Thus, the Andronovo spoken language must have been closest to the Uruk-Gaura period, i.e. clearly Sumerian, stage of the Proto-Asian linguistic koine, since in the Andronovo civilization, the Proto-Asian colonists who met again after 1800/1700 BC and who were linguistically and ethnically equalized (cf. the "plump-Europid" Andronovo anthropological type that uniformly dominated the steppe) - as we have already mentioned - left Proto-Asia and Mesopotamia for the most part after the beginning of the Uruk period.

The numerous Hungarian-Finno-Ugric and Hungarian-Turkic (Mongolian) linguistic parallels can obviously be attributed to the convergent linguistic effects of this Communicative Language, which was used from Altai to Eastern Europe. The Andronovo civilization was directly adjacent to the places of residence of both the later Finno-Ugric and the ancestors of the Turks (Mongols), it constituted the outstandingly developed leading cultural element of the steppe zone, and its agricultural, animal husbandry, and even bronze-working economic form consequently represented the overwhelming ethnic majority of the population of the area in question. All these facts testify that the Communicative Language of the Andronovo civilization played an indisputably primary role in the development of the common elements of the aforementioned neighboring languages.

That is, for the most part, these Andronovo times — approx. Between 1800/1700 and 1200 BC — and the first half of the Karasuk culture that joined in time (between ca. 1200 and 1000 — cf. the first half of the Karasuk culture — no cultural or population changes can be detected in the archaeological finds, and only after 1000 BC does a new ethnic group appear from the west; I. Grjasnow, 1970, p. 104) would be the remains of those Hungarian linguistic correspondences that Finno-Ugric linguistics considers on the one hand as a "Uralic-Finno-Ugric heritage, and on the other hand as loanwords of Turkic origin" (and of course also those grammatical structural parallels between the Hungarian and Turkic languages, which we have already mentioned, but which Finno-Ugric studies do not usually talk about, because they greatly cast doubt on the possibility of a separate the validity of the assumption of the "Uralic-Finno-Ugric" language family, given that there are many grammatical and structural features that are characteristic of the Finno-Ugric

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According to linguists, it would be a genuine "Ural-Finno-Ugric" linguistic feature, and can also be found in the Turkic languages (eg. Likewise, the common Hungarian-Finno-Ugric-Turkic linguistic parallels (dictionary and grammatical correspondences) that Finno-Ugric studies do not conceal, and which appear in the linguistic public consciousness as signs of the ancestral relationship of the so-called "Ural-Altaic language family", also originate from the early Karasuk period of Andronovo.

At the same time, certain Andronovo linguistic characteristics were also incorporated into the languages of the ethnic groups living in the southern, southeastern, and western neighborhoods of the spoken language of the Andronovo cultural circle, which can be observed most directly in the very close Iranian (and Indic) parallels of several dozen words of the Hungarian and Finno-Ugric languages, and on the other hand, the observations of the followers of the newer areal language equalization theory of Indo-Germanic studies discussed earlier can undoubtedly be attributed to such Andronovo linguistic influences, according to which, for example, The so-called "satem" character of the Iranian, Indic, Slavic and Baltic languages - that is, the prominence of sibilant sounds in these languages, while these sounds occur much less frequently in the more western, so-called "kentum" group of Indo-Germanic languages - can most obviously be explained by the significant role played by the "Finno-Ugric" languages in the areal equalization processes of the Indo-Germanic languages, since the sound system of the Finno-Ugric languages also contains plenty of sibilant sounds (1. Pokorny, 1949; in Scherer, 1968, p. 311). Similarly, the lively verb formation, the accumulation of suffixes and derivations, and the rich further formation of verbs found in the Slavic languages, which is otherwise not characteristic of the other Indo-Germanic languages, is also explained by J.

Pokorny, as we saw above. Pokorny cites the Serbian word dóm (house) as an illustrative example of suffix stacking. Its further formations are as follows: domaci (belonging to the house), from which domacin (householder), from which domáéinstvo (household lordship), from which domáđinstvovati (to lord the household). Pokorny specifically notes that such suffix stackings are extremely difficult to understand for the German sense of the stem (1. Pokorny, 1936; in Scherer, 1968, p. 181). We have already shown that all these linguistic phenomena, which are generally difficult to understand for the linguistic sense of the western Indo-Germanic people, did not enter the eastern Indo-Germanic languages from the "Finno-Ugric" languages -- as Indo-Germanist linguists believe, but in fact they passed from the Andronovo-Communicative Language to the Iranian, Indic, Slavic and Baltic languages, just as the "Finno-Ugric" languages themselves took over the same linguistic features from the Andronovo Communicative Language.

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Third degree.

If we want to examine the Cimmerian-Scythian-Sarmatian cultures of the steppe regions of the 1st millennium BC in terms of language — of course, this time with the help of the specific contexts of archaeological research — then we must start from the fact that at the very beginning of this era, around the turn of the 2nd/1st millennium, decisive economic and social changes took place in the Eurasian steppe region, which naturally also had far-reaching ethnogenetic and paleolinguistic consequences. These changes can be detected archaeologically in the assemblages of finds at the earliest around 1000 BC, and are initially manifested in the palpable separation of the peoples of the open steppe from the more northern areas (the wooded steppe and the southernmost strip of the forest belt). Thus, for example, The descendants of the flourishing, advanced Early Bronze Age civilizations of the Middle Volga and the Southern Urals (Balanovo, Abasevo) become completely insignificant, the earlier rich finds virtually disappear, and at the same time, signs of a strong cultural boom can be observed in the southern areas, so that the culturally active, or more economically wealthy, and leading sections of the populations of the northern civilizations of the 2nd millennium BC obviously moved further south. The reason for these migrations is already quite well known: archaeological finds clearly indicate that around the 2nd/1st millennium BC. At the turn of the 20th century, the equestrian nomadic economic form emerged in the Eurasian steppe zone (more precisely, it reached the stage of development during this period when it is undoubtedly visible in archaeological finds), which was able to economically exploit the vast expanses of grassy steppes with bound soil that had previously been unused — because they were not yet suitable for farming with the primitive tools of the time — and thus made them habitable for humans.

It should be noted that during the 2nd millennium BC, in the areas of the Andronovo culture and the Volga-South Ural rural cultures, agriculture was only possible in the more northern, forested steppe regions and river valleys, where the looser soils made cultivation possible even with the technical readiness of the time. As a result, in the river valleys with looser soil, but especially in the areas of the forested steppe that could be ploughed using the slash-and-burn method, the southern colonists, who had been engaged in farming and animal husbandry since the beginning of the 2nd millennium BC -- having multiplied enormously by the turn of the 2nd/1st millennium BC (we can count on a minimum 6-8-fold increase in the population over 7-800 years) -- sooner or later had to look for new ways of making a living if they did not want to starve. Thus, the economic development of the steppe horse-nomads can undoubtedly be attributed to them.

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system and way of life, mainly for the following reason: only the descendants of these agricultural-animal husbandry colonists had the appropriate economic and technical prerequisites (already a relatively large livestock, as well as vehicles, carts, metal tools, weapons, etc.) to gradually transition to the equestrian-nomadic way of life, due to their previous mixed agricultural-animal husbandry economic form and knowledge of bronze art. This is the most obvious explanation for the decline in importance of the cultural circles of the more northern, forest-covered steppe areas and the simultaneous flowering of the more southern steppe cultures around the turn of the 2nd/1st millennium BC.

"After these north-south migrations, there were still plenty of free areas on the steppe for a few centuries for the population growth of the horse-riding nomads. However, it seems that the space began to narrow in the 700s and 600s of the 1st millennium BC: on the one hand, the east-west migrations on the steppe began (first the Cimmerian-Scythian people movements, and later the gradual western expansion of the Sarmatians), and on the other hand, the northward conquests of the steppe Scythian-Sarmatian cultures, which can be traced archaeologically, into the areas of the wooded steppes and the southernmost strip of the forest belt, which, for example, were characteristic of the steppe colonial culture The Ananino culture of the middle Volga-South Ural region and the closely related Ob-Irtysh and Upper Volga cultures of a very similar structure, collectively known as the "Gorodishche cultures", can be seen in the archaeological finds in their appearance around 600-500 BC. In these archaeological cultures that developed in a wide strip in the northern fringes of the steppe Scythian-Sarmatian equestrian nomadic cultures, from the Moscow region to the Ob-Irtysh region — as we have already shown in more detail in the case of the Ananino culture — all indications are that the Scythian-Sarmatian cultural circle was a farm-like, organized animal husbandry and, to a lesser extent, agriculture. We are faced with colonial settlements whose indigenous population groups were subjugated by the steppe colonists and made to work for their own benefit (our book 1, pp. 320-321).

These more recent, closer contacts between the steppe ethnic groups and the northern forest-dwelling Eastern European and Western Asian peoples in the 1st millennium BC must be evaluated as secondary, learned influences in linguistic terms, and we can obviously attribute to these later linguistic contacts a considerable part of the vocabulary and grammatical similarities and parallels that we can observe between Hungarian and the individual smaller Finno-Ugric language groups (Obi-Ugric, Permian, Volga-Finnic, Baltic-Finnic), which are, however, absent from the other Finno-Ugric language groups. During this period, however,

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We can also very likely assume that the forest-region lingua franca, or "basic language" of Finno-Ugric linguistics, which developed in the Andronovo period, between about 1800/1700 and 1200 BC, has already lost its general intelligibility as a result of smaller and larger migrations, primarily eastward and westward expansions, and the resulting loosening and breaking of mutual relations, and the languages of the aforementioned smaller ethnic groups living in direct proximity to each other, left to their own devices, have embarked on the path of separate development and have already — secondarily — significantly diverged from each other. Thus, during the northern colonization of the steppe ethnic groups around the middle of the 1st millennium BC, the linguistic communities of the individual forest-belt groups, which were divided into separate Ob-Ugric, Permian, Volga-Finnic and Baltic-Finnic partial "lingua francas", were also reached separately by the new linguistic influences arriving with the steppe colonists, depending on the number of

the steppe colonists who created the "Gorodishche cultures" arrived, and how intense the economic, cultural, ethnic and, of course, linguistic relations between the indigenous people and the settlers eventually became.

Fourth grade

However, if we claim that our theory should form a complete whole and, if possible, explain every linguistic connection between the Hungarian and Finno-Ugric languages in an obvious way, without any effort, by switching off the genetic language family hypothesis and using areal language equalizations, then we must also take into account a tertiary layer of Hungarian areal linguistic influences on the Finno-Ugric languages, much later than the 1st millennium BC, which is primarily evident in the languages of the Ob-Ugric and, to a lesser extent, the Volga-Finnic (especially the Cherenis): in these languages, there are far more Hungarian similarities and parallels than in the other Finno-Ugric languages, and their degree of affinity to Hungarian, expressed in percentage, also shows significantly higher ratios than in the other Finno-Ugric languages (1. László 1961, p. 34, and (page 502 of our book)

And indeed, there is more than one historical and archaeological evidence of such late contacts between the ancient Hungarians and the mentioned forest Finno-Ugric groups: in the 5th-7th centuries AD, written sources from the same period speak of the migrations of the steppe peoples of the southern Pontic-Caucasian-Kuban region to the north, the middle Volga and the southern Ural regions. The first such data comes from Priscus, who reports around 463 that the peoples living in the ocean region drove the Avars out of their previous homes, who in turn drove the Sabirs out of their previous homes, and these forced the Onogurs, Urogs (Ugrians), Saragurs and Akacirs to migrate there. 1088

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I'm going to migrate. Since the 1950s, mainly as a result of the original source research of HW Haussig and F Altheim, it has been considered proven that around 463 the Sabirs (and later, in the middle of the 6th century, the Avars as well) bypassed the Caspian Sea from the south and migrated west through today's Azerbaijan and Dagestan, i.e. they invaded the Terek-Kuban-Pontus region from Transcaucasia (for more details, see 1. all this on pp. 255-257 of our book - cf. also pp. 1008-1010 of this volume), and not through the region between the Ural Mountains and the Caspian Sea, as previously generally assumed by research (and which is still unchangedly proclaimed by representatives of Hungarian prehistory), it follows that the latter peoples (i.e. the Onogurs, Ugrians, Saragurs and Akacirs) the Sabirs could only move northwards, and later, in the middle of the 6th century, they could also move northwards from the Avars. This is also proven by Jordanes' statement from the 6th century, from which we learn that in his time one of the divisions of the Huns after Attila's death, namely the Hungurs (i.e. the Onogurs by which Jordanes means the Sabirs), controlled the fur trade of the northern regions and transported the precious northern furs to the Byzantine markets.

From the second half of the 7th century, contemporary wellheads can provide information about new migrations to the north: after the collapse of the Onogur-Bulgarian empire of Kovrat in the Kuban-Don region, some of the peoples of the empire migrated to the middle Volga region. Together with these so-called "Volga Bulgars", Sabirs (and, according to many researchers, Hungarians) also appeared in the middle Volga region, and the newcomers, like their Hun-Ugric predecessors, soon took control of the northern fur trade.

The correctness of all these source data has also been clearly confirmed by archaeological research: in the middle Volga and Kama regions, the southern origin appeared at the end of the 4th century AD. the first, so-called "Harina" group of the Lomovotovo archaeological culture. This was followed by the Nevolino group around the turn of the 5th/6th century, in whose ethnicity we must undoubtedly see the Onogurs of Priscus and the Hungurs or Sabirs of the fur trader Jordanes, and finally, around the turn of the 7th/8th century, the finds of the Volga Bulgarians of written sources appear in the middle Volga region, and among these, a characteristic group of finds can be clearly distinguished archaeologically, showing more or less close parallels in many respects with the Hungarian finds of the time of the conquest, which can in all probability be identified with the archaeological relics of the Hungarians who remained in the east, found by Julianus in the middle Volga region in the 13th century (more on this in the presentation of István Erdélyi's lecture I. above).

"Research into the archaeological remains of the Hungarians before the conquest of Hungary in the Soviet Union"

1089

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And since our common folk names used abroad, Ungar, hungarus, unгри, venegri, etc. It obviously comes from the folk name Onogur (Hunugur), but this name is closely related to the name of the Sabirs (Jordanes), which Emperor Constantine, who was born in Pibor, mentions as the older name of the Hungarians — in the form "Sabartoi Asphalo" — under such circumstances, we can rightly count on significant Hungarian-speaking groups among the Onogurs, Hunugurs, Onogur-Bulgars and Sabirs who moved from south to north in the period between the 5th and 8th centuries AD, and their presence in the middle Volga-South Ural region in the middle of the 1st millennium AD, approximately in the 5th and 8th centuries, clearly explains the Hungarian parallels in the languages of the Ob-Ugric and Volga-Finns (Cherenis), which show particularly strong linguistic connections, which are absent in the other Finno-Ugric languages. But apart from all this, it is also certain from later times We have data on the presence of Hungarians in the middle Volga region: Julianus found one of the larger groups of Hungarians who remained in the east there in the first half of the 13th century.

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1091

===== PAGE 1080 =====

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===== PAGE 1081 =====

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===== PAGE 1082
=====

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=====

PAGE 1083 ===== 770 a

SZEMELYNEVMUTATO Abdi
Asirta 928-929 Achaemenid
Dynasty 85 Achilles 836

Adam 338, 378
Adasi 759-760 Ady
Endre 966 Aeneas 865
Agathangelo 1008
Agathias 266, 306 II. Agum
754-755, 761 III. Agum 754
Ahliquist, A. 63-64, 115, 547
Ahriman 14. Ahriman
Ahura Mazda 835-836 Alkim, UB 665, 668,
736-737, 745, 771, 775, 719
Almásy György 214, 242 Ámos
231, 253-255, 306, 343, 537, 627 Altheim, F. 38, 86-88, 90, 98, 101, 210-211,
236-237, 239, 241, 257-262, 264-265,
269, 284-285 287-293, 295, 305, 348, 354, 362, 369, 382,
407-408, 410-411, 413, 429, 461, 528, 536, 563-564, 566, 568, 572, 610, 621, 653, 832, 877, 886, 1007-1008, 1089 Akulim 34 yy. Amenhotep (Amenophis) 752, 927-928 IV. Amenhotep (Amenophis) 752.927 – Amlensi (Remete) Peter 216 Amiet, P. 27-28, 665, 678,
687, 689-690, 695-697, 702, 719, 724, 726, 728-731, 748, 767, 788, 790-795, 917, 1029, 1040, 1045, 1047, 1052-1053 Ammianus
Marcellinus
225, 264-266, 269, 629, 1010 Ammiditana 37 Ammisaduga 751
An 830 Ancus Marcius 862 Anonymus 8, 219, 267, 280-281.,
358 Antal László 79, 439, 496, 545 Anzu I.
Indugud Apolló 834 Apollodoros 239, 261, 290 Arany János 497, 966 Arendt, W. 222, 232 Ariapeithes 245 Ariman (Ahriman) 835-836 Aristeas 58 Arisztotelész 968 Amal, J. 898 Arnulf 359

Árpád 252, 329, 356 357, 359 360, 365, 625-626, 628, 1012, 1019 Árpád
haz 306-307, 458
I, Arsak, (Great) 290
Arsacid dynasty 260 261, 292, 1008
Artabán 261

1, Artaban 290
Artemis 833-834, 863 864
Asarhaddon 37, 760-761 Ashi
836 Asinum
759-760 Askenász
335 Askold 231
Asparuch 590,
1011 A83urbelni3eu
757-758 Agsurdugul 759 I.
A&Sumerari 760
Asterix 369 Attila 259,
272, 269, 279,
283-284, 286-287, 306-307, 328, 351, 457, 621, 623, 627, 1008, 1010, 1089 Atys 865 Autran, C. 881-882 Aventinus 252, 281 Avi-Yonah,
M. 788
Awdijew, W. I. 90
Babelon, E. 379, 451
Bach A. 213, 587
Bagucki, P. 934 Baján
283, 591 Bajor Geográfus
624 Bakay Kornél 249,
457-458 Baktur 306
Balassa Iván 1019
Balassa József 583 Bálint
Gábor 405, 547 Balkan, K. 79, 998
Bárczy Géza
251, 526, 530, 532-540,
548, 609-614, 618, 632,
647, 654, 1022-1023 Bartatua
J. Partatua Bartha Antal
8-11, 47-48, 50, 223-302, 336, 343, 355, 362, 373, 375-377, 380, 382-383, 475, 477, 603, 984, 1061-1062,
1067-1068 Bartók Béla
843-844 Bartucz Lajos 84, 624, 626 Batu 357 Bauer, H. 569 Beattie, J. 852 Belbani 760 Belenickij, A. 437 1095 ===== PAGE 1084 =====

Bendefy László 232, 571 Benfey,
Th. 61, 63 Benkő Loránd
11, 124, 150, 199, 248, 253, 442-443, 524-531, 546, 548, 1015-1016 Benzing, J. 9, 235-236, 294, 6,10,16,6
617, 654 Beran, Th. 86 Bérard, J. 869 Bereczki Gábor 844 Berichos
284 Berneker, E.
135 Berossos 337,
338 Beruni I. Biruni Bezter
(Beztur) 306
Biruni (BErūni) 238,
354, 563, 565-576, 356,
Bittel, 351 K. 46,
438, 770 Bixtun 307
Blaskovics József 627 Blegen, C. 770 Bleyer Jakab 300-301
Bloch, R. 863, 866, 869
Boba Imre
219 Bogucki, P. 1090 Bogya
Tam Bos, 836
Bopp. 460 Bor velocity
966-967 Borger, R. 966 Bork,
F. 881 Boros János
456, 458, 622 Bosch-
Gimpera, P. 101, 418,
422-426, 429,
433-435, 1073 Bos, H.
Thsert. 53 Bottero,
J. 785, 883
Bouda, K. 848, 877, 881 Bouillon
Gottfried 216-217 Bourbon-ház 215 Böhmer, JF 366 Böles Leó 277, 355, 1011
Braidwood, RJ, 266,
856, 856 902 Brandenstein,
W. 429-430 Brentjes, B.
568-569, 796, 1053 Brinnnow 76
Bryce, J. 590 Budenz,
J. 14, 209, 299, 326,
397-398, 478, 478, 80,
494-49,84-84. 501, 586, 604, 607, 628, 646-649,
1070 L Burnaburias 758

Burney, C. (és Lang D. M.) 36, 39-40, 42-43, 45, 48, 82, 87,434, 437, 603, 668-669, 672, 695, 719-720, 726, 745 746, 775, 796 801, 803, 805 806, 810, 812-813, 826-827, 878, 948, 1041, 1043, 1053, 1055, 1057, 1063, 1065 Bűdinger,
M. 212, 219, 279, 331, 336 1096 Carthasis 240

Castrén, M.A. 839
Censorinus 933
Chadwick, J. 851-856, 949-950 Charrière,
G. 347, 835, 837

Childe, G. 24-25, 29-30, 39-41, 43, 47-49, 52, 314, 386, 526, 603, 608, 718, 723, 734-735, 739, 776-777, 798, 809-810, 819-820, 823, 828, 852, 1049, 1056, 1058, 1062, 1064 Chorenei Mézes 266-267, 1010 I. Chosrau (Chosrov)
259, 1008 Christian, V. 706, 882-884
Collinder, B. 209, 305, 430, 450-451,
480, 502-503, 551, 605, 647
Condurachi, E. 717 Correns, M. 668 Crassus 291-292 József Csalog 7 Csang-Kien
292 Antal Csengery 59
Csernyecov, V.1.29,
315, 319-320, 436,
665, 993-994, 1018,
1048 Csitki 271
Barnabás Csongor 79,
439, 496, 545 Sándor Csöke 513, 553, 649 Károly Czeglédi 210, 252, 260, 343, 984
Gergely
Czuczor 465 Dagobert 288 Daicovicu, C. 717
Dajauku 85 Dales, GF 26, 29 Daniel,
G. 935-936 I. Darius 37, 553 Darwin, Ch. 460-463
King David 926, 933 Dayton,
J. 908, 912-918,
934, 937 Deer Joseph
9, 403 Deimel,
A. 76, 99, 141-142, 150,
160, 175, 192-193, 199,
338, 448, 531, 554,
557,558, 579, 594, 872,
999, 1002 Delitzsch, Fr. 68,
193 Dencker, R. 510, 838 Dengizik 286
Dienes István 11
Diodoros Siculus 244, 836 Dionysios 38 Djakonoff, IM 81, 477 Djoser 750, 936 Doblhofer, E. 90 Dolgopolskij, AB 202 Donner, K. 839 Doulo I, Dulo
===== PAGE 1085 =====

Dudik, B. 218
Dudu 761
Dulo (Doulo) dynasty 307 Dumitrescu, V. 60 Durbin, M. A,
884 Dusanni 37 Dezsy
Dümmerth
252 Dümmler, E. 212, 279 Dyr 231 Dzaparidze, OM 826
Eannatum
34, 788 Eberhard, W. 65, 83,
240, 292, 500 Ebert, M.
239, 244 Ecseedy Ildikó 388 Edekon (Edeka) 284, 286-287, 623 Edzard,
D. y. 789 Eginhard 257
Eleunte 307 Elisa 335 Elsan 283 Elulu (Elulu-mes) 763-764 Endrey
Antal 1022-1023
Enki 830 Enkidu
836 Enlil
338, 755,
806, 830 Enmerkar 20, 34, 184,
695, 766 Entemena 788
Epiphanius
337 Stephen
of Transylvania 232, 1019,
1089 Ertl, F. 284, 289, 574 Esias 284 Euphoros 244 Eva 338
Fairservis, W. A. jr.
884 Falkenstein, A, 701, 820 : Miklós Erdy 843-844
Fallmerayer, Ph. 590
Farkas
Gyula 837 Faustus
Bizantynus
260 Fazekas Mihály 185 Feist, S. 112-113 Fejér Géza 222 Fejér György 207 1. Ferdinand
278 Ferdinandy Mihály
256, 306, 357 Fettich Nándor
231-232, 262, 370 Fick,
A. 61-62, 66 Filip, J.
24, 60 Finck, F, N.
550, 1075 Fiók Károly
14, 214, 236, 275
276, 278, 280, 300-301 305, 337, 550, 605 606, 630, 637, 645, 655, 1070 1072, 1079 Fischer 36

Fischer Károly Antal 214, 281 289 Fodor
István 8 11,79, 223,232, 235, 303 372, 373,385 386, 424, 436,439,457 458 496, \$45, 562, 666,722, 992-994, 101 3,1037 1019 Fogarasi János 463 Forbes, R. J, 912 Forrer, E.
97, 552, 1078 1079
Franke, O. 65, 83,
240, 242, 271, 500 Fredegar 288 Otto
of Freising 219

Friedrich, J. 92, 424, 477, 880 Frischler,
K, 216 217 Gabain, A. 490
Gajdukevic, V. F,
347 Gamkrelidze, Th. W,
946 983, 1005 Ganda& 754-755, 761 Gárdonyi
Géza 288 Gebauer, J. 593
Gentner, W. 151
Ghirshman, R. 86-89,
242, 245, 258-259,
262, 263-833 Giesswein, A. 102 Giles, HA 115 Gilgame\$ 35, 691, 766, 836-837
Gimbutas, M. 431-439,
800-801, 803-804,
909 910, 940, 995, 1085 Gimlita
Nurta-105-1087-837 184 Giot, PR 898-899 Giotz, G. 856 Goethe, J, W. 131 Goetze, A. 36, 46, 87,
736-737, 746, 770, 773,
775, 789, 807 Gomboczn
Zolcz, 239, 233
473, 612 Cémer 335
Gordas 612 Gordon, C, 855 Grakow, BM 814, 835 Grant, M. 860-861, 865-866
Gregoire, H. 591 Grimm, J, 8, 56-57, 65, 15, 15, 15,
13, 131 149,
160-161,
211,247, 408, 440,
460 461, 464-465, 487, 572,
946, 1037 Grjasnov, M. 320, 324,
432-43, 343, 188,
638-8 1004, 1084 Groot, JJM 65, 272, 292 Gropengiesser, H. 151 Grumbach, E. 852 853 Grygiel, R. 934 Gudea 184, 788 Guignes, J, de 209 Gulkisar 756 Gulya
JJM, 1965, 1956 ===== PAGE 1086 ===== Gutschmid, A. 65, 240, 260 Günther, H.
91-92, 94 Györfy György 10, 218,
222, 251-252, 255, 281, 353,
353-35 358-361, 1013, 1019
Györi Vilmos 574
Györkösy Alajos 127
Habsburg 213 Hacsepsut
926 Hajdú Peter
10, 50, 71, 220, 279, 304,
308,
3013, 1838 385-386, 425-427, 439,
455-456, 921-922, 994-995,1018,
1020-1026, 1034, 1036
Halévy, J. 379, 451, 707 Hammurabi, 74, 754 753-759, 761-762, 765, 791, 928 Hanéar, B. 9, 25, 30, 38-41, 43,
47-49, 52, 238,
430-434, 436, 603, 6,80,
791 809-810,
1049-1050, 1056,
1058, 1062-1064 Hanka, V. 593 Harmatta Janos 50, 80-81, 268, 284-285, 304, 368-369, 4-74, 28, 42, 35 439, 552-553, 557, 559-567, 645, 949, 984-986, 991-999, 1001-1002, 1005-1008,
1011-1012, 1016, 1016-1025,
1025 1036-1037, 1057, 1061, 1081-1082 Harun ben Jahja 381 Hary Györgyné
378, 442 IH. HattuSilis 929-931 Haussig, ý. W. 9, 38, 84, 210, 226, 239, 265, 267-273, 289-291, 295, 305, 353, 568, 591, 609, 622, 653, 999, 1010, Hecken, 1011-1089 Gusztáv
616 Hehn, W.
61-62, 64, 67, 69 Hein, H. 881 Heine-Geldern, R. 33, 718, 739, 768, 820, 823-824 Heisenberg, W. 467 Helck, W. Helko, 8688. Hencken, H. 869 Henkey Gyula 14, 1064 Henle, J. 852 Henning, WB 564 Heraklész 252 Heraklios 590
Herberstein, S. 277-278 Herodotus 37,58 59, 236, 239, 244-24, 249, 249, 288,
322, 726, 834, 836, 848,
864-865 Herzfeld, E. 675
Hesychios 856 Heyerdahl,
Th. 825 Hieronymus 337 1098 Hippodamus 769 Hippocrates 236, 340 Hirt, H. 102 Hirt Frigyes 306-307 Hitler, Adolf 77, 246, 915-916, 950 Hóman Bálint 208, 25, 375, 35 360,
609, 1009-1010 Hommel, F. 849
Horvat István 8, 208-211, 268, 494
Horvath Tamás
627 Howell, JM 935 Hrouda, B. 20, 33, 665, 6, 796, 498, 4666 712,
715, 718-719, 726,
728-729, 731-732, 740,
742-743, 745-746,
750-752, 763, 765-766,
770-773, 765-766, 770-773,
1970, 174-10,141-1
1043-1045, 1055 Hrozny,
B. 77, 90, 92, 700,
880, 954 Hu-Lu-Ku
307 Humbaba (Huvava) 836
Humboldt, A. 824

Hunfalvy Pal 14, 219–220, 222, 269, 275–282, 299–301, 305, 306, 326, 331, 335–336, 359, 375, 379, 397–39, 48–47, 487 493, 496, 628, 646, 1070 Hunor 627 Hunwulf 286-287, 623 Huot, JL 26, 86, 665
Husserl, E.
467 Hii-Lii-Kiiian-Kii 307 Hiising,
G. 80, 83, 24 Ibbisin-24. 44,
762, 789-790 Ibrahim
ibn Jakub 381 Igigi 763
Ilma 756 Iljics-Szvitics, VM 202 Ilyés
Gyula 217, 220 Ilmarinen 830
Imdugud (Anzu) 743, 83, 837
Inna, 863
Inmi 695
Indra 78 Iphigenia 834 Ipsen,
G. 97 Isbierra 762, 789
Isfandar 836
Isidorus Hispalensis 337 I, ISmedagan
759-760
Istár 792-793 I. István,
Szent 381,
István 194
Márvánovits
954-958, 960-962,
969, 972,
974-975, 977, 979-981 Iwanow,
W. W, 946-983, 1005 =====
PAGE 1087
===== Jacobsen, Th. 73, 828
Jacobson, R. 965, 968, 982 Japheth 335-337 Jacob 45 Jakucs László 13, 922-923 Járainé
Komlédi Magda 922 Jarlagaba 764
Jarlagas 764 Jason 836 Javan 735
Javan Jeno Jespersen, y. 129
Jettmar, K. 9, 26, 30, 44, 84, 87-89,
210, 239, 313, 320,
432-433,
436, 563, 591, 603, 608, 725, 773,
794, 794 838,1003, 1006, 1042,
1046-1047,
1049-1050
Jordanes 38,
263, 269,
278-280, 286, 288, 306,
346, 622-623, 1010,
1089-1089 Józsuá 925 Juhász Peter 450, 504, 510-512, 988 Julianusz 15, 329, 340-341, 343-345, 614, 636, 653, 1019, 1090 Jungth, Hwir 5959. Justin 244, 290, 567, 589 Kádár főbíró 595 Kádár Zoltán 833 II.
Kadasmanellil 931 Kadasmanturgu 931 Kaindl, RF 220 Kaiser, G. 463 Kalicz Nándor 24, 42, 45, 734, 781-782,
809-810 Kállay
Ferenc 207 Kám 335, 138 Kammer, 132 A. Huber.
Kánaán 335 Kandra Kabos 330, 404-405, 829 Kangasmaa-Minn, E. 502, 617 Kant, I.
109 Karainda\$ 753-754,
756-757 Karasanay 238
Kara-Tas 240, 5 Kardos 299 Kálós
Kálá. 215 Kasgari
Mahmud 9, 235, 601,
610, 612, 617, 640, 642
Kastarita 37 yy. Castille'
754-756, 758 Of 627

István Katona 207, 274 Kavád
238 Kayan
238 Ferenc
Kemény 201, 469 470 Kempinski, A,
788 Simon Kézai 261,
276, 307, 334 339, 359, 563, 595 Kifisin, A. 908 911,916, 937 Bálint
Kiss 210 211, 273 Istvan Kiszely 84
Klapproth, J. 65, 83, 240, 273
Klebel, E. 283-284,
574 Kluge, F. 110-112, 114 118, 120,
122, 127 139, 149, 151, 199,
254, 440, 530, 549, 573, 578-580 Kniezsa Istvan 582, 586, 589, 592-594, 626-627 Koch, B. 536 Janos Kodolányi 629 Kollautz, A.
207, 226 Géza Komoróczy 304, 377-378, 836, 965-968
Konstancius 629
Constantine, Bishop of
Biborban 218-219, 225-227,
234, 264, 277, 280, 283, 289, 291, 342-344, 348, 402, 537-538,
591, 622-623, 653,
997, 1090 Kosmas Indikopleustes 337

Kossinna, G. 60, 63, 114 Tibor
Kovács 24, 45 Kovrát
(Kobratos) 329, 344, 590-591, 614, 653 König, FW 37, 84-85,
87, 243 Csoma Sándor Körösi 274, 984
Krahe, H. 94, 99, 416, 429 Kramer, SN
20, 34, 338, 687, 708, 751
Krasznov, Ju. A. 348 Kraus, FR 702, 707 Krausz
Samuel 337, 338 Kreck, G.
64 Kretschmer, P. 417-418,
420-421, 424, 429-430, 846,
939, 961, 963
Kristó Gyula 921-922 Kroisos 864 Kronasser, H. 80, 101, 418, 420-421, 429, 445, 452,
529 Kuber 590 Kuhn, A. 57,
130 Kulche
(Kulke) 307 Ferenc Kunszabó 843-844 Kuras (Kyros) 84 Kurrelmeyer, W.
254 Kurszán
252-253, 255, 360
Kus 276, 335-336 Kusid
(Kusán, Kuszál) 252 Kussmaul, F.
29, 435 Kühn, H. 102
Kündü 252 1099 =====
PAGE 1088 ===== Kybele 864
Kyros |. Kuras
Lagercrantz 504, 988 Louis Philip
215 György Lakó 513-523,
558, 565, 1081
Lang, DM Id.
Burney
C.

László Gyula 24, 40-41, 87, 209, 213, 222, 226, 229, 249-250, 252, 254, 256-257, 262, 279, 283, 289, 203, 393, 383 315-318, 322, 333, 343-344, 355, 357, 362, 364-366, 375, 381, 395, 409-410, 430, 433, 454-456, 47-47,
47, 45,45,456 509-512, 590-591, 608, 617, 619-620, 624, 722, 805, 815, 823, 831-833, 835-836, 984, 986-988, 990-919, 919, 919, 1995 1018, 1049, 1057-1060, 1067-1069, 1088 I. László, Szent 457, 835 Lau-Schang 307 Lehmann-
Haupt, C F. 86-87, 90, 227, 245, Lenormant, F. 829-831 Lessiak, P. 284, 288, 574 Leunte
307 Levente (Liüntika) 256,
355-357, 1012. 1019
Levin, S. 854 Lewy, H. 74 Ligeti Lajos 5, 10, 22, 2012, Pál
2012 257, 314, 362-363, 405,
505, 990, 1018 Liu, Mau-Tsai
240, 353, 500,
571 Liintika 1. Levente Lloyd, S. 664-665, 683,
685-694-695-696
699-700,
702-705, 707, 724, 728-729, 750,
763, 785, 826, 828, 917 Lopez, RS 287 Ludas Matyi 184-185 Lugalbanda
766 Lugalbanda 769, 768-788 Lukasevics,
P. 592 Lukonin, W. G
88, 258 Lining, J. 934-935 Maag, V. 86 Madai 335 Madyas 37, 85, 87, 243 Magna Mater (Potnia theron) 88, 836-837 Magóg János, 744 895,
909-910, 919-920
Mallovan MEL 685 Manetho
913-914 ManiStusu
763, 787-788 Mann, St. B. 103 Mao-
Tun 306-307 Marduk
754 1100 Margueron, CJ
606, 7506 Márki Sándor
366 Markwart,
J. 38, 86,
236, 291, 380-381 Marstrander,
S. 103 Masaryk, TG 593 Maszudi 38, 233-234, 270, 306, 342, 62
János Matzoc, 324 János. 727, 770-771, 777, 834 Mayrhofer M. 236, 408 Meillet, A.
73-74, 438-439, 845
Meinander, KFW 456 Meissner
B. 76 Mellaart, J. 668
Mellink, J. 584 Menander.

Ménrét (Menproth, Nemproth) 335-337 Merlingen,
W. 103 Mernephtah
932 Mesa 929 Mesek
335

Gyula Meszaros 9, 242, 244-246, 438, 602
Gideon Meszoly 329

Method, Szent 219
Metzger, H. 832
Meyer, E. 96, 429, 434
Micrajim 335
Patriarch Mihály 264, 349 Mike
307 Mikkola,
J. 591 Miklosich, F.
549, 582-589, 8593 Miklosich, V.  j. 307
Mitra 78 I. Mitridentes
261, 290
Mitscha-
M rheim, H. 226, 285-289,
305, 623-624 Mnacakjan, A. 974 Moln r Erik 9 Mommsen, Th.
65-66, 81 Montelius 60
Montet, P. 750
Moor Elem r 265, 473-474, 501,
549 Moortgat, A.
43, 438, 570, 701,
705, 718, 785, 788, 806 Gyula Gyuvc 236, 306,
606, 1009-1010 Moscati, S. 54 M ller, H. 68, 72, 74 Muageris 38, 622 Munk csi
Bernat 59, 278, 330, 352-353, 403-404, 931 Muvallis
Millenhoff, K,
58-59, 238, 244, 274, 281
M ller, Fr. 64-65
M ller, M. 61, 459 ===== PAGE 1089 ===== Ny 0 \

From and to
M ller-Karpe, H. 431-432, 441, 665, 687, 697, 716-717, 722-724, 729, 736-737, 747, 751, 753, 764, 767-770, 773-774, 778-782, 785, 788, 792-793, 798-799, 802-805, 810-812, 896, 903-906, 911, 913, 934, 995, 1046, 1049,
1051, 1057-1060, 1077 Miinchausen 445 Nack, E. 54 Nagel, W. 54 G za Nagy 9, 14-15, 58, 214, 236,
239, 260, 269,
279-281, 305,
405, 458, 550,
605-609, 621, 625, 627-630, 637, 645, 649-650, 655, 1070-1075, 1077, 1083-1084 K roly Nagy 257, 358-359, 366, 380 Ott  Nagy 380-381 Tsar Peter the Great 324 Alexander the Great 240, 836, 914 Nanna 683
Nanum 763 Naplanum 762 Napoleon 357
Naramsin 37, 763-765,
786, 788 Narr, KJ 432,
665-666, 669, 686-687, 690-691,
697, 712, 717,
723, 733-734,
737, 745-746,
773-775, 777-782,
799-800, 811-813, 893-894, 896-903,
906-907, 910 Nasatya 78 y. Nabu-kudurru-usur (Nebuchadnezzar) 929-930, 932 II. Necho 930 Neckel, G. 94 Nehring, A. 75, 100-101, 414-415, 418, 551, 850, 1073-1074, 1077 Lajos N met 366 Gyula N meth 222, 234,
242, 306, 328
Chronicle of Nestor 46, 277-278 Neustadt, MJ 311-312 Nielsen 504,
988 Nimr d
334-338 Ningirsu
833 Ninsun 836 Ninurta (Nimurta, Nimruda) 338 Noah 307, 338 Otto Nordheimi 328
Nov ki Gyula 616
Numa Pompilius 862 Oberhuber, K. 54 Odoacer
286-287 Dettinger 286 Dleg 231 Olma
231 Olympiodoros 285
Onegesios 284
Oppenheim, M. 36,
94, 431, 663,
678, 718, 807,
820 Orda 357 Orthmann, W. 48 Osiris
914 Osten, HH
88 89 Ostrogorsky, G.
356 Otten, H. 86
Oxenstierna, E. 228
Paasonen, H. 73 Pad nyi Viktor 36, 38, 249-250, 256, 300, 328,
355, 357,
360-361 Palacky, F. 593 Pal di-
Kov cs Attila 1018
Pallottino, M. 407, 424, 846, 864-872, 877 P l czi-Horv th Andr s
369 P pai
K roly 404 Parrot, A.
54 Partatua
(Bartatua) 37, 85, 87,
243 Patkanov, S. 329, 346
Pauler Gyula
218, 593 III. PazuraSSur
758

Pedersen, H. 353
Perlov, B. 908-911, 937 Perrot,
J. 665 Pertz, GH
207 Petech, L. 78,
80, 82, 84 Petrie, F. 913 Phokas,
Nikephoros
355-356 II. Phraates 290 Piaszt család
599 Pictet, A. 57, 59,
61, 238, 494, 978
Piggot, S. 974 Pittioni, R. 413, 418, 723, 783,
906-908 Pjotrovskij,
Bjotrovskij, 86, 88, 86. 832 Plato, N. 717, 852 Pliny
38, 236, 244, 261, 536, 588, 629 Pokorny,
J. 94, 97, 101, 413-414,
418, 429, 55, 95, 1945, 175 1078, 1085 Pomponius
Mela 629 Poppe, N. 611 Porada, E. 86, 88, 245, 258, 832 Potnia theron | Magna Mater Pott, F. A. 63,
463-464 Pray Gyorgy 207,
273-274 Preyer,
W, 124 Priszkosz 38, 284, 286-287, 652,
1008 1010, 1089 Pritsak, Okopios, 307
May. 38, 268, 629, 914, 1008
1101 ===== PAGE 1090 =====
Pulgram, E. 80,
101, 418-420, 429, 962-965, 969-970, 972, 972-970, Pusztay, 845
495-497 Put 335

III. Puzura\$Sur 758, 760 Puzursin
759-760 Pythagoras
469 Quitta, H. 905
Ra 837 Rächel 45
Rajkay
László 896
Rákos Sándor 836
Ramateja 853 Ramstedt
GJ 450,
616,165,164 839 II. Ramszesz 868, 929-933 Randa, A. 31,
48, 226, 526, 591, 603, 718, 828,
1050, 1063-1064 Ránk, G. 405, 504, 835, 83984 Rapsanyi-988 László 456, 458, 622
Räsänen, M. 611 Rask, R. 105 Raumer, R. 463-464
Raun, A. 502, 617 Ravila, P. 504, 988 Rédei
Károly 513-525, 515
Reguly, 1865
Number 330, 375, 403-404
Rehork, J. 319, 731
Renfrew, C. 894-901,
906, 908-912, 918, 934-946, 953 Reynolds, RL 287
Rhode, IGzo, 454, F. 624. Ribez. Rîfât 335
Rimus (Remus) 759, 763,
787 Knight, E. 65 Roga 287 Rohonyi Gyula 214, 280, 305, 358, 550,
605-606, 1071 Rolle, R.
274, Romulus
838-836

Róna-Tas András 90, 125, 223, 303, 313, 326, 372, 386, 44 1- 444, 446-452, 469, 506-507, 509-510535, 545,61,61,666 921-922, 984 Rostovcev V, M, 799 Rova 287 Rölliig, W. 570 Rössler, C. 212,
219, 279-280, 331, 358 Rua
287 Rubruk
232, 277 Ruga
287 Rurik 295 195 Safar, F. 683 Säftund, G. 869
Saggs, y.
WS 672, 701, 705,
755-756
Sajnovics János 377

Salamon 328,
924-926 Salmanassar
Id. Sulman ASaredu Samo 288

I. Säm\$ladad 757-761 II.
yyyyyyyyyyy 760-761 Samson
933 Samsuditana
754-755 Samsuiluna 755-757
Sanesan 260 Sargon (Sarru-
Kenu, Sarrukin)
43, 244,763-765, 784-788, 797 Sarkali\$arri 763-765 Sarlag 764 Sarlagaba 764
Sarrukin (Sarru-Kenu) Id.
Sargon

Sasinek, FV 366 Saul
926, 933 Saveanu,
S. 741 Sayce, AH
378, 383-384, 451 Saygun, A. 843-844
Schachermeyr, F. 24-25,
52, 386, 687, 709. 716-717, 733-734, 739, 745, 777, 779 820-823, 845-851, 869, 956-957, 983, 102: Schettelowitz 74 Scherer, A. 97, 413-416, 419-422, 429-430.
435,437,438, 452,
962-964, 970, 1073-1074. 1077, 1085 Schlegel, AW 65 Schleicher, A. 461-464 Schliemann, H. 375 Schlöizer, AL 212, 219, 275, 278-279,
331. 336, 358, 397
Schmidt, J. 64, 97 Schmidt, W.
552, 1078-1079 Schmökkel,
H. 46, 86, 100, 338, 476, 478, 682-683, 700, 705, 748, 772, 785, 788-789, 791,
806-807, 833 Schrader,
O. 57, 59-67, 69, 74-75, 83, 94, 114,
221, 238, 245, 273, 397-398, 410, 546, 976 Schuchardt, H. 73, 90, 92-94, 108 Schulz, W. 104 Schwarz, E. 289, 589 Gyula Sebestyén 621, 629
László Sebestyén 307, 358 Segert, S. 968 Seleucus II 290

Semmel weiss Ignác 93 Semper,
M. 80, 82, 90, 435, 438 Servius Tullius 862
Setälä, E. 73, 405 =====
PACE 1091 ===== / ih
dy ke in 1 1 8 Siegfried 836, Simeon,

Simeon,
880
355-357, 359-361,
1012 Simonyi Dezső
623-628 Sin 683

Sinor és 601, 603, 609, 611, 620 Sirelius, U.
T. 324, 328 Skottas 284 Skylax
58 Skymnos 58
Sode, W 37,
100, 438, 478,
579, 692, 701, 705, 752, 754, 756-761, 763, 785, 787 Solta, G. 97-98, 101, 108, 413, 418, 429, 435, 462,
959-962 Spuler, B. 84 Sraosha 836 Srejovic, D. 905 Stawiski, B. 31, 322-323, 436,
526, 603, 808,
827, 1050 Stehli,
P. 934-935 Steinitz,
W. 839 Stephanos Byzantinus 237, 279 Stiehl, R. 211, 236-237, 239,
290-292, 354, 382, 528,
566, 572, 1007
Strabo 237, 270, 290, 353, 567, 571, 629
Strommenger, E 19, 46, 86-87, 98, 687, 696-697, 730, 748 -749, 751, 832, 1045 Sudurul 763
Suess, HE 893 Sulman ASaredu 37 yy. Sulman ASaredu
929 V. Sulman ASaredu 757 Suren (Surena) 291-292 László Szabédi 399, 547-548, 604-605 Bence Szabolcsi
842 Samu
Szádeczky-Kardoss
249 László Szalai 218-219,
593 Sassanid dynasty 88-89, 292
Gyula Szekfő 208, 215, 357,
360 Szem 276 Oszvald
Szemerényi 563 Szemjonov, Sz 975 István Szili 367
József Szinyei 330,
1022-1023 Enikő Szij 1013-1015, 1019
Szmírnov, A. P. 622 Viktor Szombathy
457 Szomjas-Schiffert György 842 Szycs
István 8 Subutai 357 Suleiman II 627
Szapotluk
218, 300, 359, 593 Tabiti 59, \$36
Tacitus 69 Taddei, M. 80,
82 Tailleux, y, G. 81
Talbot Rice, T 83, 87, 90, 227, 245, 835,
837 Tardy János 922 Tarquinius
Priscus 862 863 Tarquinius
Superbus 862 863 Tarsis 335
Teispes 85 Teodorice 286 Terdzsüman,
Mahmud 627
Teseba 85

Tesup 85
Thana 335-337
Theodor 366
Theon 933
Theophanes 225, 306, 622, 1010
Theophylaktos Simokatta 38, 249, 267, 278, 291, 306, 591 Thierry, A. 275
Thomsen, V. 73
Thrane, H. 60
Thureau-Dangin,
F. 55 Thureau-krónika 306
Thure József 214, 277-279,
289, 305, 496 II. Thutmose 753 Timur Lenk 571 Tirasz
335 Togorma 335
Tolsztov, SP 26-27,
237, 271,
315-316, 319,
700, 720 Tomaschek, W. 58-59, 274, 281 Torda I. Turda

Torma Zsófia 738, 741
Tormás 537
Tóth Tibor 14, 457-458, 560-561, 622, 630 Trombetti, A.
72, 846, 869-870 Trubeckoj, N. Sz. 8,
14-15, 64, 80, 94-97 99, 134, 203, 304, 408, 418-419, 429-430, 435, 550-551, 606, 940, 942, 1067-101,101-377 Tsubinasvili, TN 826 Tubal 335 Tullus Hostilius 862

Turda (Torda) nemzetség 595, 597 Tyler, SA
884 Tyrhenos 865
Uhlenbeck, C. C.
552, 1078-1079 yyyy 830 Ulamburias
754-755
Ungnad, A. 35, 37, 76, 94,
98, 87, 807, 948 Urhitesup 931 1103 ===== PAGE 1092 ===== Urnammu
762
Urningirsu 788 Utuhegal 762 Uzitar
85 Uziwatar 85 Vámbéry Armin 214, 482, 482, 498, 494,8 493-497, 501, 523, 582,
585-587, 628, 646-649 Vanden Berghe, L. 791 Varga Zsigmond 379, 450, 790-882 Vargyas Lajos 842, 1016-1017
Vávár I. 614-617 Vdzsapidze,
O 974 Vejnemojnen 830 Velden, Fr. 72-73,
831 Velikovskiy, J. 924-933 Ventris, M.
848, 851-854,
949-950 Vénusz 673
Vernadsky, G. 230-231,
255 Verner (-fele
törvény) 121 Victoris László,
223 224 842, 844, 1016-1017
Vilkuna, K. 456 Virchov 932 Vixtun 307
Vlassa, N.
740-744 Vöcel, I. E. 64 Voigt Vilmos
841 Wadler, A. 132 Wagner, GA
Wahle, 138 Wahle, E.
238. 869 Walde, A. 414 Walluk 288 Ward
Perkins, JB 869
Weber, A. 7
Weinberger,
S. 770 Wheeler, M. 768
Whitehouse, D. és
R. 734, 782 Wicki
369 Wieland 836 Wiesner, 634, 348, 345,
348, 837 Wirth, H.
93 Wittich 283 Wolff, L. 93 Woolley, L. 677, 748,
785 Xenophon
38 Zakar András 338 Zakharias the rhetor 265
Zakharov, A. 222, 232
Zakynthinos, AD 226,
235, 235 589-591 Zarathustra 1. Zoroaster Zariagaba
764 Zeki Validi
Togan, A. 566 Zeuss, K. 58-59, 86, 212, 238, 244, 274,
278-279, 281,
336 Istv and
Zich, 2026 324-325, 328, 571
Zimutar 85
Zoroaster (Zarathustra)
211-212, 337, 836 Zöllner,
E. 226 Zsirai Miklós 279, 303
Zutima 85 TARGYMUTATO Afghani alásztan 767-768
349-354 Allatmetkezések
811-814 Anatolia
663-672, 714-724, 744-747,
769-776 anatóliai-Aegean-tyrrenian 845-877,

944-946
ancient Anatolian spoken language 877-880
Andronovo culture 30-31, 49-50, 320-326, 1003-1004
anthropology 313-314
mother goddess 836-837
areal language equalization approach 333-334, 498 511, 940-946, 1002-1005, 1067-1069
Aryan ruling class 77 82, 949-950
Arctic cultural center 840-841
people of Asi 270 272
1104
Avar Avars, Onogurs, Sabirs 267-270 Hephthalite-Huns, Avars, Hungarians 273-275 Griffes-Indás style 293-294 Onogur-Sabir-Avar vándorlás 265-267 Asia 26-31 West
1040-1054
Basque language Sumerian connections 877 Pecheneg-Futás 255-256, 297-298
Bible and Hungarian literary tradition 334-339 Bulgarian-Turkic languages 234-236, 619-629
y 893-895, 901-903, 918-924 boat burial 369-370 Chuvash criteria 609-620
===== PAGE 1093 =====

Southern Russia 800-805
Hephthalite-Huns / Huns Aegean Anatolian-Aegean-Tyrrhenian language community 845-877, 944-946 region 776-781 Egypt 749-751 chronology 924-933 Elam 679-689 tree of life motif 367-369, 835 reasons for migrations
from Elassia 815-828 cultural and linguistic koine 708-713, 1027-1039 development of their cultures, their connections 19-21, 663-713 influence 21-52, 714-814 linguistic influence 845-887 etymology 253-254 Etruscan
culture and language 859-875 Europe 21-26, 776-781 South-East 733-744 East 333-334, 801-805, 1054-1067 Central and West 808-811 European origin of metalwork 912-918 European origin of the Neolithic
autochthonous 893-936 European origin of metalwork Andronovo, Ugric" 320-326 910-916 Finno-Ugric 7. Ural-Finno-Ugric agriculture (Finno-Ugric and Hungarian) 250-251 Griffes-Indás art style 293-294 Grivna question 226-227
Georgian ecclesiastical jewelry 296-297
Funeral Speech 532-541 sound changes (sound laws) 105-131 semantic role 133-157 sound crossing, polarization 152-154 Hittite language 90-92, 410-411, 954-957 events of the conquest 354-362 metal plates for the
eyelids and mouthpieces of tombs from the conquest period 344-349 double conquest 364-367 Huns Hephthalite-Huns, Avars, Hungarians 273-275 Hun-Hungarian affinity 305-307 historical role 263-265

Iberian languages' Sumerian connections 877 Indo-Germanic ideology of cultural priority 367-369, 377-379, 380-383, 429-439
linguistics 56-101, 246-248 ancestral homeland "migration" 937 983 historiography 56-101, 246-248 India 768-769 Iran 729-733, 766-767, 796-780 North 791-794 West 672-674
Kárpát Basin 733-744 pre-conquest population 256-257, 362-363 Caspian region folk names 289-293 Caucasus 31-52 First Meg Transcaucasia Khazar, Sabir, Hungarian folk names connections 233-234 Eastern
European culture 313-320, 1018 double conquest 364-367 Hungarian rule in Kiev 255 koine (cultural and linguistic) 708-713, 1027-1039 cosmic duel 835-836 Kuban region 798-800 Kurszan theory 252-253 Kushans 272-273

Aegean-Tyrrhenian and Sumerian base layer of the Latin language 875-877 Linear A and B tablets 851-856, 949-950 Lomátovo culture 262-263, 293-294 Luristan 790-791 Magna Hungaria 340-344 Hungarian agriculture
250-251 ethnonym Sabir, Khazar connections 233-234 language words of Finno-Ugric (Ural-Finno-Ugric) origin 304-305, 471-541 sound development 532-541 loanwords Altaic (Turkish, Bulgarian) 234-236, 601-645 Indo-
Germanic 545-551 Iranian 552-572 German 573-582 Slavic 582-600 dogmas of linguistics 303, 308 etymological methods 253-254 contemporary approach 439-458 paleontology 251 methodology of historical
research 326-331, 354-362, 364-367, 373-387 linguistic taboos 294-295 pan-Indo-Germanic approach 367-369

1105
===== PAGE 1094 =====
bourgeois positivist historical perspective 299-301 archaeological terminology 314-315 literature references 249-250 objectivity 255 history before 1848 207-212 after 1848 our Finno-Ugric origin school 212-223 opposition
275-289 latest results 984-1026 changes of consonants 138-157 changes of consonants B, D, G 147-152 attrition of consonants zbk 145-147 RL transition 138-139 Medes 82-85 metathesis, inversion of syllables
154-156 Mesopotamia South 679-689, 698-706 North 672, 674-679, 689-698 chronology 751-766 Mitanni 77-79, 81-82, 949-950 art 86-90 griffes-indás style 293-294 Georgian ecclesiastical jewelry 296-297 Sassanid artistic
style 86-89, 258-259, 295-296 Scythian art 244, 831-837

Son of the Sun 837 nasalization 143-145 ethnographic connections 829-844 nomadization 224-225 language parallels, Etruscan-Sumerian 872-875 Finno-Ugric-Iranian-Sumerian 999-1001 Germanic-Sumerian 166-168 Greek
(Ancient Greek) Hebrew 162-164 Sumerian 164-165, 857-858 Hebrew-Greek 162-164 : Indo-Germanic Semitic 169-172 Sumerian 158-204 Iranian-Finno-Ugric-Sumerian 999-1001 Latin-Sumerian 165-166, 875-876
Semitic Indo-Germanic 169-172 Sumerian 158-204
1106
Slavic-Sumerian 168 Sumerian Etruscan 872-875 Finno-Ugric 999-1001 Germanic 166-168 Greek 164-165, 857-858 Indo-Germanic 158-204 Iranian 999-1001 Latin 165-166, 875-876 Semitic 158-204 Slavic 168 I. still
Hungarian language our linguistic monuments 532-541 linguistics etymology 253-254 development 407-417 linguistic Darwinism 459-470 linguistic paleontology 251, 417-428 linguistic statistics 498-513 I. still
Hungarian linguistics Nyevolinian culture ethnicity 293-294
Onogur Avars, Onogurs, Sabirs 267-270 Onogur-Sabir-Avar migration 265-267, 1008-1010 Russia I. South Russia
ecological changes 825-827 evaluation of prehistoric hypotheses 373-375
paleobotany 308-313 paleoclimatology 318 paleontology 7. linguistics Palestine 667-668 Parthians 259-262
Parthian-era folk names in the Caspian region
289-293
Pasian people 270-272 polarization, sound crossing 152-154 Pontic region 31-52 Proto-Akkadian theory 706-708
radiocarbon dating I. C"
archaeology formation of Finno-Ugric peoples 333-334 chronology 319-320 Hungarian prehistory research 375-377 terminology 314-315 Uralic homeland 315-318
sabarol nep 270-272 sabartoi asphaloi 225-226
===== PAGE 1095 =====
sakauraka 270-272 satem-kentum variations 139-143 Iranian origin of steppe steppe peoples 236-246 connections of the steppe zone with Near Asia 1040-1090
Sabir Avars, Onogurs, Sabirs 267-270 Sabir, Khazar, Hungarian folk names connections 233-234 Onogur-Sabir-Avar migration 265-267, 1008-1010 Sabir-Hungarian question 289-293 bibliographic references (. Hungarian
historical research European origin of ribbon-decorated ceramics 723, 904-908 Saltovo culture and the Alans 349-354 Szarvasjny
832-835 Sassanid artistic style 86-89, 258-259, 295-296 salinization 827-828 Syria 667-668 Scythian Hungarian Scythian tradition 334-339 Scythian art 831-837 Iranian-ness of the Scythians 58-59, 236-245 Scythian-Hungarian
relations 339-340 Slavic archaeological traces in Eastern Europe 228-233 syllable reversal, metathesis 154-156 Sumerian immigration to Mesopotamia, its predecessors, its ancestral homeland 698-706 Proto-Akkadian
theory 706-708 Basque language Sumerian relations 877 isolation 379-380 more distant connections 880-887 I. more parallels of languages filter theory 282-289

Tatar plates 735 744, 908 0
Tyrrhenian Anatolian-Aegean Tyrrhenian language community
845-877, 944 946
tochar nép 270-272
Transcaucasia 744 747, 769 776, 796-800, 805-807
overpopulation 815-825
Turkmenistani 767, 794-796
Ugric burial in a 'csénak 369-370 metalwork 320-326 ethnonym 277-281 Ugric-Hungarian language community 1069-1090 nomads 331-333 Neolithic autochthonous European origin 893-936 Ural-Finno-Ugric agriculture 250-251
chronology of people and language family 397-406 development 333-334, 498-513, 650-653, 986-991 modern approach 838-840, 1067-1069 linguistics 523-532 methods of language comparison 480-498, 513-523 ancestral
homeland paleobotany 308-313 paleoclimatology 318 archaeology 315-318 migration 385-387 primitive anthropology 313-314 migrations 331-333 Ural region 31-52

hunting brothers 832-835 world mountain 835 vocalization 133-138 Volga region 31-52
parallels of our musical traditions 841-844, 1016-1017
1107
===== PAGE 1096 =====
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===== PAGE 1097 =====

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